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CONTENTS

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Book Reviews

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The submission fee will be waived for student authors. Submissions from students should be accompanied by a faculty member's certification of their student status.

IN THIS ISSUE

RALPH H. TURNER is professor of sociology at the University of California, Los Angeles. He is engaged in an effort to refine and integrate theories of role and self. He is also engaged in the empirical study of collective behavior through an investigation of the community response to earthquake threat in southern California.

MELVIN L. KOHN is chief of the Laboratory of Socio-Environmental Studies, Intramural Research Program, National Institute of Mental Health. The second edition of his *Class and Conformity*, including a reassessment based on research done here and abroad in the eight years since its original publication, has been published by the University of Chicago Press.

CARMI SCHOOLER is a social psychologist in the Laboratory of Socio-Environmental Studies of the National Institute of Mental Health. His areas of interest are the nature and personality correlates of perceptual and psychophysiological processes in both normals and schizophrenics, the examination of psychological consequences of occupational experience, and the study of the structural and cultural antecedents of both normal and abnormal adult functioning.

MORRIS ROSENBERG is professor of sociology at the University of Maryland. Long interested in social influences on self-concept formation, he is the author of *Society and the Adolescent Self-Image* and coauthor of *Black and White Self-Esteem* (with Roberta G. Simmons). His new book on the sociology of the self-concept will be published by Basic Books in 1979.

LEONARD I. PEARLIN is a research sociologist in the Laboratory of Socio-Environmental Studies, National Institute of Mental Health. His program of work in recent years has been concerned with the effects of social organization on a variety of aspects of psychological functioning. He is currently engaged in longitudinal studies of the relationships between social organization and psychological well-being.

JAMES B. RULE, currently on leave at the Center for Advanced Study in the Behavioral Sciences, is associate professor of sociology at the State University of New York at Stony Brook. He is the author of *Private Lives and Public Surveillance*.

PAUL BURSTEIN is assistant professor of sociology at Yale University. His main interest is the theoretical and empirical analysis of political change in democratic countries. He is now studying the causes and consequences of change in federal antidiscrimination legislation since the 1950s.

WILLIAM FREUDENBURG is a Ph.D. candidate in the Yale University Department of Sociology. In addition to his work in political sociology with Professor Burstein, he is interested in sociological issues related to energy and the environment. His primary focus in that area is on the social consequences of large-scale energy developments in the Rocky Mountain region, and he has recently completed 16 months of fieldwork in energy-impacted communities of western Colorado.

JACQUES DELACROIX is assistant professor of sociology at Indiana University, Bloomington. His general interest lies in the sociology of economic development. At present he is working primarily on quantitative modeling of dependency–world system theory. The present paper will be followed by another one in the same area, also coauthored with Charles Ragin; it is to be published in *Comparative Sociology* in 1979.

CHARLES RAGIN is assistant professor of sociology at Indiana University, Bloomington. His major interests are historical and comparative sociology and methodology. He is currently engaged in studies of nationalism, economic development, and metatheoretical issues in the social sciences.

CLAUDE S. FISCHER is associate professor of sociology at the University of California, Berkeley. He is the author of *The Urban Experience* (1976) and coauthor of *Networks and Places* (1977). He is currently completing a study of how the characteristics of communities affect the personal social networks of their residents.

EVE SPANGLER is currently a postdoctoral research fellow on the American Bar Foundation's Law Student Activity Patterns project. She is also doing research on the impact of socioeconomic origins, sex, and race on educational and early career development and is teaching sociology as a Visiting Five College Lecturer at Smith College.

MARSHA A. GORDON is a veteran of the American Bar Foundation's Law Student Activity Patterns project, where she was a research associate for two years. She is currently working as a research and statistical analysis specialist at the University of Massachusetts, Boston, investigating various problems in the sociology of education.

RONALD M. PIPKIN is associate professor in the Legal Studies Program at the University of Massachusetts, Amherst, and Affiliated Scholar of the American Bar Foundation. He is currently engaged in research on law student time budgets and professionalization.

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IN THIS ISSUE

MURRAY WEBSTER, JR., is professor of sociology at the University of South Carolina. He continues research into justice processes and is writing a chapter on roles for the 1979 *Annual Review of Sociology*.

LE ROY F. SMITH is a graduate student at the State University of New York at Buffalo. His primary research interests are interpersonal influence, justice, and social movements.

MICHAEL HECHTER, associate professor of sociology at the University of Washington, is the author of *Internal Colonialism: The Celtic Fringe in British National Development, 1536–1966.*

LARRY J. GRIFFIN is assistant professor of sociology at Indiana University, Bloomington. He is doing research on structural and individual-level determinants of social inequality and changing patterns of inequality over time.

KARL L. ALEXANDER is associate professor in the Department of Social Relations, Johns Hopkins University. Currently he is involved in several research projects in the general areas of educational and social stratification, including studies of career aspirations and career development, of school effects on standardized aptitude and achievement test scores, and of postsecondary institutional differentiation.

CHANDRA MUKERJI is assistant professor of sociology and communications at the University of California, San Diego. She is continuing her studies of material culture, object design, and social change.

RICHARD HERBERT HOWE is a graduate student in sociology at the University of Illinois. He is working on a book on Max Weber's order of discourse.

GODFREY J. ELLIS is assistant professor of family relations and child development and of sociology, and faculty associate of the Family Studies Center at Oklahoma State University. His major interests include comparative family analysis and social psychology. He is currently working on an introductory family text.

GARY R. LEE is associate professor of sociology and associate rural sociologist, Washington State University, and director of research on aging for the Social Research Center. He is the author of *Family Structure and Interaction: A Comparative Analysis*. He is currently doing research on social integration among the elderly as well as on comparative family organization.

LARRY R. PETERSEN is assistant professor of sociology at Memphis State University. His principal research interests are the family, religion, and social exchange.

JOHN R. LOGAN is assistant professor of sociology at the State University of New York at Stony Brook. He is currently applying the notion of stratification of places to the study of metropolitan development in the United States in the postwar period. In addition, he is completing research on transformations in state-working-class relations in contemporary Spain and Portugal.

LEONARD BROOM and F. LANCASTER JONES are professors of sociology and PATRICK McDONNELL is a research fellow in the Research School of Social Sciences, Australian National University. PAUL DUNCAN-JONES is a senior research fellow in the Social Psychiatry Research Unit. The authors jointly directed the national sample survey upon which the paper in this issue is based. A report of the method of the survey and auxiliary documents are contained in their volume *Investigating Social Mobility*. Broom and Jones are also the authors of *Opportunity and Attainment in Australia,* a study based largely on the findings of an earlier national survey. The four authors have produced several recent joint publications on social stratification.

DENISE B. KANDEL is adjunct associate professor in the School of Public Health and Department of Psychiatry, Columbia University, and head of the Division of Youth and Family Research in the Department of Child Psychiatry, New York State Psychiatric Institute. Her research interests include adolescent socialization and development, processes of interpersonal influence, and cross-cultural comparisons. She is completing a longitudinal study of adolescent use of legal and illegal drugs and planning a follow-up in early adulthood. She has also recently initiated a study of young women and drug use and is editor of the newly published volume, *Longitudinal Research in Drug Use: Substantive Findings and Methodological Issues.*

Errata

In the review of *Sociological Ambivalence and Other Essays* by Robert K. Merton (*AJS*, March 1978, pp. 1277–80), two words were transposed and one was misspelled in lines 1 and 2 on page 1278. The passage should read: " '. . .with persisting interest in the alternative forgone only temporarily if at all.' "

In the review of *Young John Dewey: An Essay in American Intellectual History* by Neil Coughlan (*AJS*, March 1978, pp. 1280–82), the bibliographical information should read as follows: Chicago: University of Chicago Press, 1975. The price is $10.75.

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American Journal of Sociology

Volume 84 No. 3 November 1978

CONTENTS

Commentary and Debate

Review Essay

Book Reviews

IN THIS ISSUE

WAYNE M. ALVES is an NIMH postdoctoral fellow in mental health evaluation research and adjunct assistant professor in the Department of Sociology, University of California, Los Angeles. His current research interests focus on monitoring delivery of urban public services, developing indicators of performance of social units larger than the individual (for example, families or households), and evaluating community-based treatment of mentally ill and developmentally disabled persons.

PETER H. ROSSI is currently professor of sociology and director of the Social and Demographic Research Institute at the University of Massachusetts, Amherst. His current research and writing are in the fields of evaluation research, intervention experiments in criminal justice, and the politics of natural disaster policies.

STEVEN L. NOCK is assistant professor in the Department of Sociology at the University of Virginia. His current research attempts to develop an alternative to the life-cycle approach to studying families that will include childless couples as well as parents in second and third marriages.

VIVIANA A. ZELIZER is assistant professor of sociology at Barnard College, Columbia University She is the author of *Morals and Markets: The Development of Life Insurance in the United States*, to be published in 1979. She is currently doing research on the changing economic and emotional value of children's lives at the turn of the century.

MICHAEL T. HANNAN is associate professor of sociology at Stanford University. His research interests include organizational ecology, social change, and welfare policy and the family. *National Development and the World System*, a collection of research with John W. Meyer and students, will be published soon. He is also completing (with Nancy Brandon Tuma) a monograph on models and methods for studying change.

NANCY BRANDON TUMA is assistant professor of sociology at Stanford University. She is currently engaged in research on the formation and stability of interpersonal relationships, including marriages and friendships. She is also studying social mobility, including both occupational mobility and movement in and out of the labor force. The development of models and methods for studying change over time is another ongoing research interest.

LYLE P. GROENEVELD is a sociologist at the Center for the Study of Welfare Policy at SRI International (formerly Stanford Research Institute). In addition to his work evaluating the impact of the Seattle and Denver Income-Maintenance Experiments on marital stability and family functioning, he is conducting research on juvenile delinquency in low-income populations.

ANDREW CHERLIN is assistant professor of social relations at Johns Hopkins University. Currently, he is continuing his research on remarriage.

He is also studying other trends in family life, such as the postponement of first marriage among young adults and the increase in cohabitation.

VOLKER BORNSCHIER is assistant professor of sociology and director of the research project "Multinational Corporations, Economic Policy, and National Development" at the Soziologisches Institut, Universität Zürich. He has done extensive research on multinational corporations and development, including two recent books: *Wachstum, Konzentration, und Multinationalisierung von Industrieunternehmen* and, with Than-Huyen Ballmer-Cao, *Multinational Corporations in the World Economy and National Development.*

CHRISTOPHER CHASE-DUNN is assistant professor of social relations at Johns Hopkins University. He is currently studying the effects of international economic dependence on the development of national urban systems; he is also studying the ways in which changes in the level of conflict among core states affects the core-periphery division of labor in the world economy.

RICHARD RUBINSON is assistant professor of social relations at Johns Hopkins University. At present he is studying processes of political transformation in the capitalist world economy in the 19th century and conducting a study of state formation in the United States.

KENNETH L. WILSON is assistant professor of sociology at the University of South Carolina. Besides continuing his work on status attainment models, he has forthcoming articles with John Butler on race relations in the military, with Louis Zurcher on status inconsistency, and with Alejandro Portes on the labor market experiences of recent Cuban immigrants to the United States.

CARDELL K. JACOBSON, associate professor of sociology at Central Michigan University, is continuing to study reactions to court rulings and desegregation processes. He is also collecting data on attribution processes in the formation of emotions.

DAVID SNYDER is associate professor in the Department of Sociology at Indiana University. His current research includes comparative and historical work (with Paula M. Hudis) on occupational sex segregation, studies of collective action and violence, and network analyses of world system structure.

MARK D. HAYWARD is a fourth-year graduate student at Indiana University. His current research centers on specifying the structural determinants of earnings, British strike behavior, cross-cultural occupational segregation, and components of change in the number of female family heads.

PAULA M. HUDIS is assistant professor of sociology at Indiana University. She is working with David Snyder on a comparative and historical study of occupational sex segregation and with Anne S. Macke on a study of the relationships among sex-role attitudes, employment, and fertility of American women. She also continues to do research with Arne L. Kalleberg

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TO OUR READERS

Because of a printing error, publishers' advertising scheduled for the September 1978 issue was omitted. It follows the preliminary pages for this issue.

The Role and the Person[1]

Ralph H. Turner
University of California, Los Angeles

As a complement to the familiar idea of self-conception, the concept of role-person merger is proposed as a more behavioral approach to understanding the social construction of personality than has been taken previously. Person and role are said to be merged when there is a systematic pattern involving failure of role compartmentalization, resistance to abandoning a role in the face of advantageous alternative roles, and the acquisition of role-appropriate attitudes. Three principles concerning appearance, effect, and consistency provide the basis for a series of propositions concerning interactive determinants of merger. Three other principles—consensual frames of reference, autonomy and favorable evaluation, and investment—provide the basis for propositions concerning individual determinants of role-person merger.

By each individual, some roles are put on and taken off like clothing without lasting personal effect. Other roles are difficult to put aside when a situation is changed and continue to color the way in which many of the individual's roles are performed. The question is not whether the role is played well or poorly or whether it is played with zest or quite casually. Role embracement (Goffman 1961*b*, p. 106) can coexist with strict role compartmentalization. An accomplished thespian can give himself unreservedly to a role and take great pride in producing a convincing portrayal of the part but return to being a very different kind of person when the play is over. The question is whether the attitudes and behavior developed as an expression of one role carry over into other situations. To the extent that they do, we shall speak of a merger of role with person.

Many of the discrepancies between role prescription and role behavior in organizations can be explained by the individual's inability to shed roles that are grounded in other settings and other stages of the life cycle. Merger of role with person is often the source of role conflict, as Killian (1952) demonstrated for emergency workers whose more deeply merged family roles infringed on the performance of their rescue roles in a disaster situation. When a role is deeply merged with the person, socialization in that role has pervasive effects in personality formation. When there is little

[1] I am grateful for support from the National Institute of Mental Health (grants USPHS MH 16505 and MH 26243) and comments from Steve Gordon, Sheldon Messinger, Jerald Schutte, Stephen Spitzer, Thomas Tyler, and Lewis Zurcher.

or no merger, role-socialization effects remain strictly compartmentalized.

The observation that there are differences in the extent to which role and person are merged supplies a useful point of departure for a wide-ranging examination of the relationship between role and person. The aim of this paper is to develop that approach by suggesting some of the conditions that should determine whether a role will be merged with the person of the role incumbent or will be evoked only when the situation dictates.

THE NATURE OF ROLE-PERSON MERGER

The Concept of Person

Most investigations into social determinants of personality employ a psychologistic view of personality as consisting of traits. Traits are related to role allocations in looking for the "fashioning effects" (Carl Backman and Paul Secord, in Gordon and Gergen 1968, pp. 289–96) of role on personality, "role selection" (Thomas 1968) in harmony with personality, and "self-role congruence" (Sarbin and Allen 1968). But the traditional concept of a *person* as "a human being who has acquired status and engages in social interaction" (Hoult 1969, p. 237) is amenable to formulation in terms that relate more meaningfully to social structure. In the broadest sense the person consists of all the roles in an individual's repertoire, with some qualification about how well each is played. But in keeping with the analogy of stage role playing, sociologists are reluctant to infer anything about the person of the actor merely from the nature of the roles that are played well or poorly. Role repertoires are organized into hierarchies. The person is best described in terms of the roles that are still played when not called for and that color the way in which other roles are played.

The idea of person is related to the ideas of self and identity, and merger of role and person is related to identification with a role. Two differences from these concepts are important, however. First, identity can be strictly situational, and identification with a role is often used to indicate the quality of situational involvement in a role (Nye 1976, p. 23). Although the terms "self" and "self-conception" are usually reserved for an object that resists strict compartmentalization by role-defining situation, this is not always the case. Second, the ideas of self and identity are generally conceived subjectively, according to the arousal of self-feeling. Kuhn's Twenty Statements Test (Kuhn 1954; Spitzer, Couch, and Stratton 1970) relies on the personal experience of self-feeling as the basis for subjects' enumerating components of the self-conception. Jack Preiss (in

Gordon and Gergen 1968, pp. 207–18) explores the emergence of a professional self-image by asking medical students when they expect to feel like physicians. This approach has been and will continue to be important, but it has limitations. Reports of self-feeling are more difficult to verify than reports of behavior and are probably less reliable. People often report self-feeling in fleeting and atypical roles and situations, which are plainly not the major anchorages for person or personality. Perhaps it is because of this reliance on reported self-feeling that the self-conception has been a less fruitful predictor of behavior in empirical research than was anticipated (Wylie 1968; Spitzer et al. 1970).

The idea of role-person merger is offered as a more behavioral complement to the subjective idea of self-conception. Careful study of the correspondence and discrepancy between self-feeling and role-person merger should enhance our understanding of the person as social product.

Criteria of Role-Person Merger

Three principal criteria are suggested for use in empirical studies of role-person merger. The first is failure of role compartmentalization: a subject can continue to play a role in situations where the role does not apply. In the motion picture *A Double Life,* Ronald Coleman depicted an actor whose obsession with the stage role of Othello led him to act out the role in his private life offstage. Strodtbeck and Mann's (1956) demonstration that interaction in experimental jury panels is pervasively affected by the sex of the jurors suggests that sex roles are deeply merged with the persons of many of the experimental subjects. The professional who carries the office bearing and air of authority into family and community dealings has *become* to a considerable degree the professional role played at work.

A second criterion is similar in principle to the first: one resists abandoning a role despite available, advantageous, and viable alternative roles. When "class consciousness" is intense, a laborer may be emotionally unable to accept proffered advancement to a supervisory spot. A craftsman in a declining occupation may be unable to face the possibility of shifting to a skill that is in greater demand. The academic person par excellence rejects promotion to a more highly paid and prestigious administrative post as a betrayal of commitment to the academic role. The retired worker who keeps returning to the office and giving advice to younger workers has been unable to divorce the role from his person.

A third way to recognize salience and resistance to compartmentalization is to examine the attitudes and beliefs that a person holds. Merger of role with person is indicated by the acquisition of attitudes and beliefs appropriate to the role. Lieberman's (1950) demonstration that industrial

workers promoted to foremen came to hold more promanagement attitudes than they did before promotion indicates at least a modest merger of the role of foreman with the person. The traditional sociological concern with "occupational attitudes" (Bogardus 1927) has been an effort to understand how the occupational role defies compartmentalization in supplying a personal orientation in varied situations.

A fourth but less conclusive criterion also exists: the experience of learning a role or putting it into practice. It is more difficult to state this criterion in simple terms for several reasons. (*a*) Sometimes the merger occurs simultaneously with learning or executing the role and sometimes only after the role has been played for a period of time. (*b*) Sometimes role-allocation processes bring the role and actor together on the basis of a preexisting congeniality—that is, personal predispositions that suit the individual to the role. And (*c*) varying degrees of anticipatory socialization precede allocation, adoption, and enactment of a role.

When there is no merger with the person, adding a new role to one's repertoire is generally a simple matter of learning how to enact the role. But when the new role is being fitted into the personal hierarchy, there is normally some personal disruption and reorganization. Hence the process of learning and adopting a role that is also being merged into the person is usually more tumultuous than learning a role that is merely a situational resource. There is a paradox here: transition into a role with pervasive significance for personal behavior can often be a stressful experience. Marrying, joining a church, and assuming an office of public trust can all be more difficult steps for persons who do not compartmentalize these roles than for those who do.

Determinants of Merger

To some extent the merger of role and person is imposed on the individual in the course of social interaction. The individual is partially constrained to be the person who corresponds to the assumptions that others are making about him. But unlike role allocation, role-person merger requires more than external compliance with social pressures. Hence individual determinants also require careful attention. I discuss interactive determinants of merger first, then individual determinants. I begin each of these sections by asking what functions are served for alter and ego by the concept of person, and next I suggest some guiding principles governing the selective merger of role and person, consistent with the functions identified. Then I list propositions inferred from the guiding principles. These sections are followed by a brief discussion of articulation between the two sets of determinants.

INTERACTIVE DETERMINANTS OF MERGER

Interactive Functions of Role-Person Merger

In order to hypothesize about ***when*** there will be a merger of role with person we must first ask ***why*** the attitudes and behavior that express one role should be experienced in situations where other roles are more obviously relevant. Why should people make the assumption that there is something called a person who is more real than the visible role-playing actor? Following the classic functional approach (Dewey 1922; Malinowski 1945), I assume that alters employ a conception of ego as person, based on what they see in one or more of ego's roles, when being able to conceive of a person is useful to them in their interaction. Understanding the utility of being able to interact with a putative person rather than a mere role incumbent should supply the primary clues to when the role will and will not be equated with the person.

When the same people interact only within a single role complement, questions sometimes arise that can be answered more adequately by assuming the existence of a person behind the actor than without that assumption. Two questions in particular pertain to such situations: (*a*) Will the same role allocations apply in successive interaction episodes, or will it be necessary to reestablish who is playing which role on each occasion? (*b*) Can the actors be depended upon to carry out the implied and expressed commitments of their roles? In each instance, postulating a person more fundamental and continuous than the actor helps in finding answers to the questions. If the role has become a significant part of the person, reallocation at the actor's initiative is unlikely, and commitments will probably be honored. It simplifies matters for everyone if this assumption can be made.

The idea of person becomes more significant when the individual is viewed as the incumbent of different roles in various settings. Here the utility of the idea of person lies in facilitating answers to two other questions: (*c*) Which role will govern the individual's orientation in the not infrequent instances when the boundaries between settings are not airtight? (*d*) What role will govern the individual's orientation in situations that are undefined, occurring outside plainly marked institutional settings?

All four of these questions are material to the processes of social control and to the judgments of credit and responsibility that enter into decisions on whether to apply control techniques or not (Ralph H. Turner, in Gordon and Gergen 1968, pp. 93–106). Social control, to be effective and lasting, requires a less ephemeral object than the actor playing a particular role. Hence the concept of person is shaped to a great degree in the service of social control.

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Interactive Principles

Appearance principle.—The concept of person is a simplifying assumption, and recurrent interaction in the same roles is simplified when participants can assume that the roles correctly reveal the persons involved. If we assume an economy-of-effort principle—people tend to act on the basis of the simplest assumptions that seem to work for them—it is reasonable to suppose that most interaction proceeds on the basis of appearances. The appearance principle can be stated as follows: in the absence of contradictory cues, people tend to accept others as they appear. This means that people tend to conceive another person on the basis of the role behavior they observe unless there are cues that alert them to the possibility of a discrepancy between person and role.

This may seem a surprising assumption in the light of prevalent concern with lack of trust, insincerity, and calculating role performance. But attribution theorists constantly find a strong tendency to attribute the cause of behavior to the person, even when the observed behavior is expressive of a strictly delineated role (Jones et al. 1972). And the impressionistic evidence is considerable. We take for granted that people who frighten us are aggressive, that people who kill or steal are murderers or robbers in more than a situational sense, that people who stop and render aid are kind, that people who give gifts are generous.

Effect principle.—A natural extension of the functionalist logic is the observation that we pay closest attention when our own fate and course of action are at stake. If the assumption of the existence of a person behind the actor facilitates interaction, the need for that assumption will be greatest when the potential effect of the interaction on the interactants is greatest. Accordingly the disposition to conceive people on the basis of their role behavior will vary directly with the potential effect of the role on alter.

Consistency principle.—As a simplifying assumption, the idea of person depends upon positing some individual consistency over time and among situations. People are disposed, in the absence of strong contradictory indications, to accept the most obvious and least complicated view of the person that facilitates interaction. Accordingly people should view a particular role enactment as accurately revealing the person when doing so adds to a consistent picture of the person and should distinguish between role and person when failure to do so imports inconsistencies into the image.

Interactive Propositions

Role and person in a single role complement.—The simplest situation is that in which ego and the social circle (Znaniecki 1965) of alters interact

in only one set of relationships (i.e., one role complement). Four propositions relevant to such situations are suggested on the basis of the appearance principle.

1. The more inflexible the allocation of actors to a role, the greater the tendency for members of the social circle to conceive the person as revealed by the role. People are most likely to see the role as the person when they have no opportunity to see the actor in alternative or contradictory roles. Astonishment over the unprepossessing but thoroughly reliable bank clerk who acts the hero during a bank robbery, or the one who is exposed as an embezzler, allows us to appreciate how much we take for granted that the role *is* the person when we have seen the individual in only one role. In contrast, when we see people regularly in alternative or contradictory roles, we are sensitized to the distinction between person and role and inhibited from making our usual automatic assumptions about the person behind the role. When role allocations in any situation or group of people are unstable, so that people readily exchange roles in the course of interaction or between episodes of interaction, there can be little disposition to confuse role with person. But when the same actors play the same roles in successive interaction episodes, people increasingly assume that role-related attitudes will be expressed in other situations and take for granted that they express the personal orientations of the actors.

Many conditions affect the rigidity of role allocations. One of these is the existence of conflict. Conflict accentuates the demand for loyalty and secrecy. Allocation to roles that are in mutual conflict tends to be relatively inflexible once the initial role allocations have been made. Hence a derivative proposition: (1*a*) The more conflictual the relationship between roles, the greater the tendency for members of the social circle to conceive the person as revealed by the role.

2. The more comprehensively and strictly differentiated the role, the greater the tendency for members of the social circle to conceive the person as revealed by the role. When differentiation is slight, there is a great deal of role overlap, so that many of the group tasks or characteristic attitudes are shared by incumbents of different roles. In one family unit, man's work and woman's work are quite distinct, while in another family unit husband and wife share most of the home responsibilities and differ very little in most of their characteristic attitudes and sentiments. From the appearance principle, it follows that a system of highly differentiated roles displays readily distinguishable patterns of performance, and consequently the roles seem to tell a great deal about the role incumbents. When roles are only weakly differentiated, the patterns of behavior are only peripherally distinguishable, and the roles seem to say little about the persons.

3. The higher and more consistent the judgments of role adequacy, the greater the tendency for members of the social circle to conceive the person as revealed by the role. Poor playing of a role suggests that the role is not a good clue to the person. At the same time, role adequacy does not necessarily imply a positive judgment. If the role is negatively valued, high role adequacy may bring even more unfavorable judgment than low adequacy. Just as the highly effective lawyer, craftsman, or parent is most likely to be regarded and treated as if the role were the person, so the "successful" criminal is more often viewed as being a criminal at heart than the unsuccessful felon.

Judgments of role adequacy carry less weight when the role is easy than they do when the role is difficult. Even a judgment of quite high adequacy for an easy role may not be viewed as telling a great deal about the person. Hence a contingency proposition: (*3a*) The more difficult the role is thought to be, the stronger the relationship between judgments of role adequacy and the tendency for members of the social circle to conceive the person as revealed by the role.

4. The more polar the evaluation of a role as favorable or unfavorable, the greater the tendency for members of the social circle to conceive the person as revealed by the role. Roles that are quite positively evaluated and those that are quite negatively evaluated attract more attention than neutral roles. Because appearances are more striking, inferences about the person are likely to be stronger. Very positively valued roles are also usually thought to be fairly difficult to perform well. The same is true of very negatively valued roles, because of the assumption that most people are held somewhat in check by mores and values of the group and society. There is also a riskiness about switching role allocations when the roles are strongly valued either positively or negatively that contributes to inflexible allocations.

The effect principle suggests three propositions applicable to interaction in a single role complement:

5. The greater the potential power and discretion vested in a role, the greater the tendency for members of the social circle to conceive the person as revealed by the role. This proposition needs no comment.

6. The greater the extent to which members of a social circle are bonded to role incumbents by ties of identification, the greater the tendency for them to conceive the person as revealed by the role.

7. The more intimate the role relationship among actors and social circle, the greater the tendency for members of the social circle to conceive the person as revealed by the role. These two propositions deal with qualitative differences in the power of role incumbents over their role alters. Personal followings depend heavily on identification: the followers experience the

successes and setbacks of their leader as if they were happening to themselves. Vicariously feeling the leader's elation and depression, the identifier can hardly admit the possibility that the leader is someone different from the person seen in the leader role. Identification is the typical relationship of followers with their leader and also of individuals in many relationships of admiration and dependence, such as that of child with parent.

A relationship of intimacy requires that customary defenses be dropped —that usual boundaries of self-disclosure be abandoned. The person whose role involves intimate revelations, such as the counselee, reveals aspects of behavior, attitude, and sentiment that could be quite damaging if the counselor were to betray the trust and pass on the revelations. Accordingly it is nearly impossible to remain in such a relationship unless the counselee assumes that the counselor displayed in the role corresponds to the person.

Role and person in multiple settings.—The distinction between person and role becomes more critical for social interaction when social circles overlap so that people may interact with each other in different role complements according to the demands of the situation. The community structure organizes the relationships among sectors, establishing that certain classes of roles will be the key roles (Hiller 1947, p. 339), whose significance transcends the boundaries of the role-specific situations. The person-defining body then ceases to be the circle for a specific role and becomes the community. Here again, four propositions are inferred from the appearance principle.

8. The broader the setting in which a role is lodged, the greater the tendency for members of the community to conceive the person as revealed by the role. Roles vary from an office held in a little-known club to age and sex roles that overlap the major institutional sectors of society. Between the extremes are roles lodged in broad institutional sectors, such as occupational and family roles. Since roles lodged in broader settings are more widely and frequently visible, they should more often be accepted as clues to the persons. Sex roles do indeed seem to afford confirmation for such an inference. Probably no assumption has been more generally and uncritically made than that men and women really are different—that they are not merely playing roles. Likewise, research has often confirmed that sex is one of the most powerful correlates of a wide range of attitude differences. Similarly, assumptions about the age-specific personal characteristics and dispositions of children, adolescents, adults, the middle aged, and the elderly are rarely questioned.

9. The more representative or external a role in relationship to its role system, the greater the tendency for members of the community to conceive the person as revealed by the role. A distinction has been made between internal and external roles. The external or group representative roles

(Parsons 1951, p. 100) are concerned with relations of the group and its members to other individuals and groups. These roles are especially visible to an audience outside the organization, thus considerably extending the basis for identifying the individual with the role.

10. The more conspicuous and widely recognizable the role cues, the greater the tendency for community members to conceive the person as revealed by the role. Roles are recognized by means of role cues or role signs (Banton 1965), which can be either relatively conspicuous and easily recognized or hidden and recognizable only to insiders. Uniforms, entourages, badges, hair styles, clothing styles, and distinctive speech patterns are among the conspicuous role signs. These signs extend the association between the role and individual beyond group boundaries, lend credibility and emphasis to the role allocation, and reduce ambiguity. Accordingly, in the absence of counterindications, they should accentuate the tendency to view the person through the role.

11. The more a role exemplifies the goals and nature of the group or organization in which it is lodged, the greater the tendency for community members to conceive the person as revealed by the role. In the context of multiple settings for roles, there are two distinct sources of information about any role incumbent. One is the role and the other is the nature of the setting or group. Certain roles in groups can be called exemplary because they exemplify the goals, the values, or the nature of the organization or group in some way. The architect exemplifies the architectural firm more than the janitor or accountant in the same company; the pastor exemplifies the church; the teacher exemplifies the educational institution more than the registrar does. In these cases the two sets of information—role and group membership—are mutually reinforcing. Accordingly, we infer that the incumbent of the exemplary role will more often be identified as a person through the role than will the incumbents of roles that do not exemplify the group.

Two closely related propositions concerning multiple settings are suggested by the effect principle:

12. The greater the extent to which a role in one setting determines allocation and performance of roles in other settings, the greater the tendency for members of the community to conceive the person as revealed by the role. If there has been a tendency to look for clues to a woman's personality through her family roles but to uncover a man's personality in his work role, it may be partly because the work role has traditionally been a precondition for the man's family role but not for the woman's family role. Banton's (1965) distinctions among basic, general, and independent roles suggest a continuum from basic roles that determine the individual's eligibility for many role allocations, to independent roles that

have no implications for other roles. There is a great deal of overlap between this dimension and the breadth of setting in which a role is lodged. But the dependence of one role on another goes beyond any general classification of roles along the basic-independent dimension, as the illustration of men's and women's work roles indicates.

13. The greater the extent to which the allocation to a role in an organization precludes incumbency or restricts the performance of roles in other settings, the greater the tendency for community members to conceive the person as revealed by the role. Total institutions (Goffman 1961*a*) and greedy institutions (Coser 1974) deny the individual alternate settings in which to display personal characteristics at variance from those expressed through performance of a role in the institution. For inmates there is an almost inescapable merger between inmate role and person in the eyes of the community.

The consistency principle is the source of two further propositions:

14. The greater the extent to which a role is viewed as appropriate to and dependent on the actor's more broadly based roles, the greater the tendency for members of the community to conceive the person as revealed by the role. In spite of the tendency for the most broadly based roles to serve as determiners of eligibility for less broadly based roles, individuals may still be found in roles that are not considered wholly consistent with or appropriate for their broadly based roles. A man may be a nurse, although the female sex role is the usual precondition to the nurse role. In addition, there are roles whose allocation is unaffected by a broadly based role. Because the broadly based role is identified with the person, there is also a tendency to see any role for which it qualifies the person as being a natural extension of the broadly based role and consequently a source of further clues to a consistent view of the person behind the role. But when the less broadly based role is not in keeping with the broadly based role, it will often be discounted so as to avoid introducing inconsistencies into the image of the person derived from the broadly based role. By applying this observation, we can extend the illustration of women and men in occupational roles. Even a female head of household who must enter occupational life to support herself and her children has traditionally not been identified as a person through her occupational role. The male role makes a man eligible for most occupational roles, so these roles become extensions of the broadly based male role, and the man is assumed to be the person displayed in his occupational role. But the female role has not traditionally qualified women for most occupational roles, so these roles have not been viewed as extensions of the broadly based sex role and have not been seen as providing important clues to the woman's person. Similarly, a woman nurse has been more likely to be perceived as person through her occupational role than a woman in business

and a woman elementary school teacher more than a woman professor, if our inference is correct.

15. The more temporary the allocation to a role is understood as being, and the more discontinuous in content the role with respect to preceeding and succeeding roles, the greater the tendency for community members not to conceive the person as revealed by the role. The idea of an exemptive or time-out role was first developed by Parsons (1951) in his discussion of the sick role. By qualifying for the sick role and accepting its obligations, one escapes both the judgment of low role adequacy or deviancy in the neglected roles and the inference that behavior in the role is a clue to the character of the person. Other exemptive roles include bereavement and, depending upon cultural values, drunkenness (MacAndrew and Edgerton 1969). Two features of these roles may be crucial. First, they are temporary, and the whole pattern of obligations and privileges is premised on their transitory nature. Gordon (1966) presents evidence that lasting impairment leads to a pattern different from the sick role. Second, these roles are not sought or voluntarily assumed.

A class of roles that is voluntarily assumed is similar in resisting merger with the person. The principal example here is the student role. Students have traditionally been granted considerable license and their foibles discounted as indications of their personal qualities. The student role differs from other preparatory roles in being discontinuous with respect to the ultimate role. Unlike the company assistant manager, whose role performance has only slightly less grave consequences than the manager's for co-workers and customers, the student is separated from the work setting, and his work is of no consequence to others except as a sign of his academic progress. We assume that the shift from medical student to hospital intern is a significant transition in this respect.

The exemptive and student roles have in common discontinuity and an explicitly temporary character. These characteristics in combination seem crucial for discounting the role as an index of the person. They also relate to appearance and effect over time.

INDIVIDUAL DETERMINANTS OF MERGER

Individual Functions of Role-Person Merger

It is a paradox of social interaction that, for an individual to understand, predict, and control others' behavior (except by force), he himself must be reasonably comprehensible and predictable to them. The easiest way to be comprehensible to others is to be the person they have constructed from one's roles. The first function of selective role-person merger for the individual is to establish a basis for understanding, predicting, and con-

trolling others by becoming reasonably understandable and predictable to them.

A second individual function of role-person merger is to economize effort when called on to play many roles. The probability of playing all our roles equally well is slight for most of us. But by exporting favored roles into marginally relevant situations, the individual can discount low adequacy in the displaced roles and need not work for mastery in any but the most significant roles. Similarly, if we assume that individuals seek to complete invested lines of action (Mead 1938), they can drop unfinished lines of action in their less involved roles.

Closely related is a third function of facilitating control and its obverse, autonomy. In the absence of a trans-situational anchorage, the actor is strictly a creature of each situation and the associated roles. Being able to draw upon an identity that transcends the situation gives the actor discretion and mutes the effect of many of the positive and negative sanctions that regulate the situationally indicated role enactment. In keeping with Waller and Hill's (1951) less-interest principle, the individual who is less exclusively involved in a role can often turn this to advantage over one who is fully involved.

A fourth function is to enable the individual to maximize favorable interaction. Some roles provide more gratification than others, either because the roles carry more rewards or because the individual is more proficient at them and better able to secure their rewards. If the individual can arrange to continue playing these roles in situations where other roles are indicated, he may escape some of the costs of the less gratifying roles.

A fifth function is to enable the individual to realize rewards commensurate with the investments made in particular roles. Investments in a role are often disproportionate to the strictly role-related rewards. The child who practices piano for months in preparation for a single recital and the mother who makes great sacrifices for an unappreciative child are examples of high investment for low return. Extrinsic rewards like money can be detached from the role for use in situations unrelated to the source role, but intrinsic rewards such as admiration of artistic virtuosity or filial devotion cannot. In the latter case, the individual may be able to supplement a meager reward by continuing to play the heavily invested roles in situations where the role is uncalled for.

Principles

Consensual frames of reference principle.—Individuals tend to merge into their persons those roles by which significant others identify them. This occurs because of the mutual understanding and control paradox already discussed, and because the cultural frames of reference that view

certain roles as key or person roles are learned and tend to be taken for granted by the individual.

Autonomy and favorable-evaluation principle.—The principle that the individual will tend to merge role and person selectively so as to maximize autonomy and positive self-evaluation stems directly from the corresponding functions.

Investment principle.—Following Homans (1961), investment has been an important concept in sociological theorizing as part of an exchange model of social behavior. The guiding assumption is that individuals will merge into the person those roles in which investment has been greatest or in which adequate return from investment is yet to be realized.

Individual Propositions

Several propositions are rather directly derivable from the principles just presented:

16. The more intensely and consistently significant others identify a person on the basis of a certain role, the greater the tendency for the individual to merge that role with his person.

17. The more actor-discretion incorporated in a role and the wider the range of settings in which the role behavior can be made meaningful, the greater the tendency for the individual to merge the role with his person.

18. Individuals tend to merge positively evaluated roles with their persons.

19. Individuals tend to locate their persons in the roles they enact most adequately. Some evidence in support of these last two propositions is found in Rosenberg's (1967) study of selectivity in self-esteem formation. Following Davis (1949), we shall call the evaluation derived from allocation to a role "prestige" and the evaluation based on the adequacy with which the role is enacted "esteem."

If a person's most prestigious role is also the one played best, it should be salient in the individual's role hierarchy. Likewise a low-prestige role that is played badly should be handled with considerable detachment. But most roles probably fall in the more interesting category marked by discrepancy. Propositions 18 and 19 will then yield contradictory predictions. Three preliminary suggestions will be offered concerning how people resolve such dilemmas.

First, persons fall on a continuum from unitary to divided. One solution to conflicting rewards is a sharply divided personal organization so that one can "be" whichever role provides the more favorable evaluation in the situation at hand. Mowrer (1935) described a life pattern of dual roles in which contradictions between roles never became an issue because the worlds in which they were enacted were totally segregated. (18–19*a*)

The less positive the correspondence between the prestige and the esteem derived from different roles in one's repertoire, the greater the tendency to divide the person between different roles in different situations.

The significance of role adequacy also depends greatly on the visibility of role performance or of evaluations of role performance. When criteria of role performance are vague or performance is not subject to public view and evaluation or effects of role performance are only assessable after long delay, role prestige will count much more heavily than esteem in the selective merger of role and person. (18–19*b*) The more visible and readily appraisable the role performance, the more the tendency to merge prestigious roles with the person will be modified by the tendency to merge roles that are played with high role adequacy with the person.

People live primarily within relatively restricted or wide social worlds. The distinction between locals and cosmopolitans (Merton 1957) suggests such a difference. In general, esteem based on role adequacy is accorded by the social circle of a specific role, while prestige is accorded by both the social circle and the larger community. Hence the relative importance of these two kinds of evaluation is affected by the scope of one's social world. (18–19*c*) The more the scope of an individual's social world exceeds the boundaries of the social circle for a given role, the more the merger between role and person is determined by the prestige rather than the esteem of the role.

20. The greater the investment of time and effort in gaining or maintaining the opportunity to claim a role or in learning to play a role, the greater the tendency to merge the role with the person.

21. The greater the sacrifices made in the course of gaining or maintaining the opportunity to claim a role, or in learning to play a role, the greater the tendency to merge the role with the person.

22. The more publicly a role is played and the more an individual has engaged in explaining and justifying a role and its standpoint, the greater the tendency to merge the role with the person. As this principle indicates, roles that involve socializing others to a role or enforcing adherence to role boundaries are especially likely to be merged with the person. For example, Lowe (1971) found that students who served as tutors in school played the student role more fully and intensely. It might be assumed that they would also have carried the role into other settings.

23. The greater the unresolved role strain, the greater the tendency to merge the role with the person. The effort and preoccupation required to cope with role strain constitute another type of investment. Traumatic conditions lead to persisting preoccupation that contaminates performance of other roles. Unresolved problems are difficult to let drop when the opportunity for change of roles comes along. Because an individual usually

tries to divest himself altogether of roles that are very costly, we are led to a paradoxical observation. The same role that would have been abandoned entirely at an early stage, had it been possible or had the costs been known, is the role most likely to become merged with the person at a later stage.

Assumptions underlying the last proposition are contradictory of the assumptions in some of Burr's (1972) propositions concerning ease of role transition. In his propositions 5 and 6 (pp. 410–11), Burr states that role strain facilitates movement out of a role and impedes movement into a role. The latter hypothesis is consistent with the present argument. But I believe the former overlooks two principles: (*a*) the amount of strain produced by low role adequacy is directly related to the extent of identification with that role, and (*b*) undergoing and coping with strain in a role is a way of investing in the role, making it more difficult, rather than easier, to shed it. For example, Waller (1930) and many after him have supplied case studies to illustrate how difficult it can be for people to adjust to divorce after a stormy marriage.

24. The more intrinsic the benefits derived from enacting a role the greater the tendency for the actor to merge the role with the person. The creative experience of writing or the aesthetic satisfaction of making music are benefits that can be realized only in the setting of role enactment. Actors, artists, craftsmen, writers, scientists, and scholars, whose intrinsic role benefits are especially prominent, often resist opportunities for favorable role reassignments. Extrinsic rewards are no less valued than intrinsic ones, and roles are sought no less for their extrinsic rewards. But the extrinsic return from personal investment is fully realizable without constant reenactment of the role or clinging to the role past the appropriate stage in the life cycle, so there is less impetus to merge role and person.

This proposition suggests a common basis for lack of articulation between the person-conception held by others and the personal organization, and for strict role compartmentalization. When intrinsic satisfactions come in a recreational role, the individual is more likely than the community to anchor the person in that role. People who turn their hobbies into vocations and women who translate intrinsically rewarding volunteer work into paid employment are often seeking to resolve this lack of articulation so as to gain community acceptance in keeping with the way they see themselves.

THE PROBLEM OF ARTICULATION

It is unlikely that hypotheses of the simple form listed for interactive and individual determinants of role-person merger can be formulated to describe the resolution between competing principles. Hence the discussion of

articulation will be limited to a few general points and some consideration of the process.

While the individual will often be socialized over an extended period of time into merging roles with his person selectively in conformity with the assumptions of significant others, there are at least two additional reasons for convergence. First, the appearance principle signifies a predisposition on the part of others to accept ego's presentation of his person. Thus convergence is facilitated by a two-way process in which the individual internalizes group conceptions and the group accepts what is apparent in the individual's behavior. Second, interactional and individual processes are often convergent in their operation. For example, personal investment in a role is probably correlated with rigidity of allocation, breadth of anchorage, and the extent to which one role is a qualifying role for access to other roles. Likewise, discretion in role enactment turns up as a correlate of role-person merger in the discussion of both interactional and individual determinants.

Nevertheless, convergence is imperfect, and the discrepancy will sometimes be great. Maximum convergence comes with continuity and integration of personal experience in a well-integrated society. Idiosyncratic socializing experiences and major changes in social setting increase the likelihood of discrepancy, with the marginal man (Park 1928) as a classic example. But we must adduce structural conditions in order to understand such widespread discrepancies as: frequent rejection of the community's presumption that the wife-and-mother role most fully reveals the woman as a person, the endemic effort to compartmentalize occupational roles, widespread resistance among the elderly to accepting an age role as personal anchorage, and disavowal of the implication of a deviant role for interaction in other roles.

If we assume an individual is pulled toward being the person others treat him as being, the question is what circumstances intensify or nullify such pressure. In discussing intrinsic role benefits, we noted that the individual might achieve convergence by shifting to a role that is similar in essential respects but includes the characteristics necessary for the community to identify the role with the person. The availability of suitable substitute roles is important in enabling individuals to resist pressures to change their personal role hierarchies.

The individual may hold on to a nonconvergent personal anchorage while complying with behavioral expectations if validation of personal anchorage is available from some reference group and if social circles are highly segregated. Overlapping social circles mean that interaction is often on a person-to-person rather than role-to-role basis, so discrepancies become problematic. Segregated social circles make interaction on a strictly role-

to-role basis more feasible. Social movements such as the women's movement support dissident personal anchorages, as do deviant communities. Mitchell (1966) describes a pattern of calculating role playing in prison, in which prisoners find mutual support for deceiving prison officials concerning the correspondence between prison roles and the person.

In a complex, loosely ordered society like our own, there are many possibilities for developing and maintaining a personal organization centered about roles that are not salient in the community's view of persons. But the potential strain is considerable and may help to explain uneasiness and unpredictability in role behavior.

In order to give sufficient emphasis to the active part played by the individual in the articulation process, it is tempting to borrow the concept of negotiation. Role allocating and role making are often accomplished through negotiation (Cicourel 1970). But negotiations deal principally with the visible currency of role behavior in delimited situations and in fairly brief episodes. The actor who is cast in a given role as the outcome of negotiation need not carry that role with him into other situations. The merger of role with person should be conceived more as a process of socialization, taking place over an extended period, building on the outcomes of many episodes in which role allocations are negotiated.

CONCLUDING DISCUSSION

In concluding this discussion, I shall look briefly at the relationship between the approach used in this paper and attribution theory and call attention to some important unresolved problems.

Attribution Theory

"Attribution theory deals with the rules the average individual uses in attempting to infer the causes of observed behavior" (Jones et al. 1972, p. x). The preceding discussion of interactive determinants could be viewed as an examination of one class of popular attributions. Attribution theorists have concentrated their attention on external versus internal attributions of causation, which can often be translated into the question of whether the actor is merely playing a role (external) or whether the behavior and sentiments expressed through the role are those of the person (internal, with person and role merged). While the aim of attribution theory is to identify the causes and consequences of the cognitive phenomenon of causal attribution, my aim is to use causal attribution as one among several determiners of the behavioral phenomenon of role-person merger.

In principle, we should be able to incorporate hypotheses from attribution theory into the interactive determinants of role-person merger. For

example, evidence that role compliance by high-status persons is more frequently attributed to external causation than role compliance by low-status persons (Thibaut and Riecken 1955) is consistent with hypothesis 4 or 5, though the explanation is different. Thus far, however, attribution theorists have more often chosen variables for investigation that are not readily translated into role-theoretical terms (Jones et al. 1972; Shaver 1975). The reason seems to be that most often, attribution theorists have dealt with strictly transitory role allocations of the type that can be simulated in the one-time laboratory experiment, using subjects who are strangers to each other. In addition, they have employed a rather mechanical and incomplete concept of role. These and other limitations for translation into sociological theorizing have been well stated by Alexander and Epstein (1969).

However, current conclusions from attribution theory and research may appear to raise three questions for the theory of person and role advanced here. First, attribution theorists' finding that role behavior is discounted as an indicator of personal disposition (Jones, Davis, and Gergen 1961; Harold Kelly in Jones et al. 1972, pp. 1–26) calls into question the underlying assumption that some roles are significantly merged with the person in the eyes of both actors and audiences. Second, the same discounting principle from attribution theory seems to discredit the *appearance* principle, which is one of the foundation stones of the theory advanced here. And third, the finding that observers, but not actors, tend to infer personal dispositions from behavior even when that behavior is under obvious direction or contraint (Edward E. Jones and Richard E. Nisbett in Jones et al. 1972, pp. 79–94), provides an important answer to the first two questions but seems to challenge the assumption that actors' identifications are strongly influenced by their social circles' identifications of them.

Two important observations help to place the attribution findings in context. First, while there has been overwhelming empirical support for the hypothesis that obviously in-role behavior produces fewer internal attributions than role-independent behavior, the evidence as summarized by Jones and Nisbett (Jones et al. 1972, pp. 79–94) shows a marked tendency to make internal attributions under both conditions. Ring (1964) speculates that people are confused and suspicious about out-of-role behavior, instead of preferring it as a basis for inferring personal dispositions.

Second, attribution theorists' experiments are generally designed to alert subjects to the likelihood that personal disposition and role may be contradictory, rather than to ascertain what people take for granted in the absence of conflicting cues. In certain of the experiments the observer-actor relationship is explicitly between supervisor and worker (Strickland 1958) or between high- and low-status members (Ring 1964), creating

reasons for the observer not to accept the actor's behavior at face value too readily. But if attribution theorists have focused on a special class of cases in which subjects must choose between conflicting appearances, have they not thereby accepted the appearance principle implicitly? If, in the face of conflicting appearances, subjects nevertheless make internal attributions more frequently than predicted, is it not reasonable to suppose that in the absence of contradictory cues the tendency to identify the person with the role should be even more widespread? I suggest that the appearance principle has been an unstated and unexplored assumption underlying major attribution theories, while the effect and consistency principles have direct counterparts in attribution theory.

If the appearance principle and the assumption that others frequently conceive the person as revealed in the role are consistent with attribution theory, there remains the question whether the difference between observer and actor tendencies vitiates the assumption that ego tends to become the person that significant others treat him as being. In a masterful exposition of the difference between actor and observer perspectives, Jones and Nisbett (Jones et al. 1972, pp. 79–94) explain the actor's preference for external attributions and the observers' preferences for internal attributions on the basis of different information available and different principles of information processing for actor and observer. The force of Jones's argument may be weakened, however, when ego's enactment of the role is recurrent and when there are continuing opportunities for communicating role-person conceptions between the actor and social circle. The findings may also be an artifact of the choice of situations for investigation in which the actor is disposed to escape responsibility rather than take credit for accomplishments.

But the reported differences between actor and observer attributions have direct bearing on a theory of self-conception rather than a theory of role-person merger. The concept of role-person merger is behavioral rather than cognitive and allows explicitly for the possibility that the individual's stated self-conception may be at variance with the behaviorally relevant merger of person and role. These insights from attribution theory may help to explain interesting discrepancies of this sort.

Some Remaining Problems

The questions raised in this paper are not new but have seldom been treated as part of an integrated concern with the merger of role with person. The entire exercise might reasonably be viewed as an effort to approach the traditional problems of self and self-conception from a more behavioral standpoint that avoids the well-known weaknesses of subjective indicators of self-locus. My aims have been to identify a problem and

suggest an approach for dealing with it. The few principles suggested here are only a small beginning for a wide-ranging study of a complex and important question. Nevertheless, they may have shed light on some of the paradoxical differences between sexes in the personal significance of occupational and family roles, and on other problems in the relation between person and role. Three issues in particular are highlighted by this effort and deserve more serious attention.

The pattern of convergence and divergence between the roles that resist compartmentalization and those that are salient in the stated self-conception merits careful study. In occupational socialization, for example, the most intense self-feeling may come early when the individual is preoccupied with mastering the role, while the most inflexible commitment to the role may come after one has developed an interdependent role repertoire.

Also, some fresh insights into the perennial issue of situational behavior versus pervasive personal consistency may be derived from this line of inquiry. A personal consistency that transcends situationally limited roles is of varying utility to individuals and to their social circles. Weak pressures to merge role with person or competing strong pressures toward merger often leave situational behavior paramount.

Finally, I have dealt only with anchorage of the person in more or less institutionalized roles. An important resolution to the problem of disjuncture between community attributions and personal organization may be to remain personally uninvolved in all of one's more institutional roles and to lodge the person in ephemeral and esoteric roles. A declining tendency to locate the self in institutional roles has been noted by some observers (Zurcher 1972; Turner 1976); it might be explained by a growing divergence in the working of processes that determine community attributions and the individual's personal organization.

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The Reciprocal Effects of the Substantive Complexity of Work and Intellectual Flexibility: A Longitudinal Assessment[1]

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Our previous research, based on cross-sectional data, provided prima facie evidence of a reciprocal relationship between the substantive complexity of men's work and their intellectual flexibility. The present study employs longitudinal data to make a more definitive assessment. Using maximum-likelihood confirmatory factor analysis to separate measurement error from real change, it develops measurement models for both substantive complexity and intellectual flexibility. These models show that, over a 10-year time span, the "stability" of both variables, shorn of measurement error, is high, that of intellectual flexibility especially so. Nevertheless, a structural equation causal analysis demonstrates that the effect of the substantive complexity of work on intellectual flexibility is real and remarkably strong—on the order of one-fourth as great as the effect of men's earlier levels of intellectual flexibility on their present intellectual flexibility. The reciprocal effect of intellectual flexibility on substantive complexity is even more pronounced. This effect, however, is not contemporaneous but, rather, a lagged effect occurring more gradually over time.

From early Marx to "Work in America" (HEW Task Force 1973) it has been argued that work affects such facets of personality as values, orien-

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tation, and intellectual functioning. From early Taylor to the most recent personnel-selection manuals it has been argued—or at any rate assumed—that personality is formed before occupational careers begin, with people fitting into and perhaps molding their jobs, but not being affected by them. The issue of whether jobs affect or only reflect personality is obviously crucial to occupational social psychology. More than that, the issue of the nature and direction of causal effects in the relationship between occupational conditions and psychological functioning provides a critical test of a theoretical question central to the entire field of social structure and personality—whether social structure affects personality only through its influence on childhood socialization processes or also through a continuing influence during the entire life span.

Our own interpretation of the processes by which social class affects values and orientation, for example, is premised on the assumption that people's positions in the stratificational system are systematically linked to differences in the immediately impinging conditions of their lives which in turn affect their values, self-conception, and social orientation (see Kohn 1969; Kohn and Schooler 1969). In our analyses, we have found occupational conditions to be especially important in explaining the impact of social class on values and orientation. Our interpretation assumes that occupational conditions actually affect psychological functioning. But if the interrelationship of social class, occupational conditions, and psychological function were the result solely of selective recruitment and job-molding processes, our interpretation would be invalid.

In this paper we address the issue of the nature and direction of effects in the relationship between occupational conditions and psychological functioning by attempting to assess the reciprocal relationship between one pivotal dimension of occupational structure, the substantive complexity of work, and one pivotal dimension of psychological functioning, intellectual flexibility. We choose the substantive complexity of work as the occupational condition to be assessed in this analysis for three reasons. Our previous analyses have shown substantive complexity to be a central element of occupational structure—an important determinant of occupational self-direction and an important "structural imperative" of the job (Kohn and Schooler 1973, pp. 102–5). Moreover, substantive complexity is as strongly correlated with psychological functioning as any other dimension of occupation we have examined (ibid., p. 104). Finally, we have excellent descriptive information about the substantive complexity of work, which we have been able to validate by comparison with the objective job assessments of trained occupational analysts (ibid., p. 106).

Our choice of intellectual flexibility as the aspect of psychological functioning assessed in this analysis is made in part because it offers us the

greatest challenge—intellectual flexibility obviously affects recruitment into substantively complex jobs, and there is every reason to expect it to be one of the most resistant to change of all facets of psychological functioning we have measured. Moreover, intellectual flexibility—though not much studied by sociologists—is a part of psychological functioning so important that we must not unthinkingly assume it to be entirely the product of genetics and early life experience. Rather, we should empirically test the possibility that intellectual flexibility may be responsive to the experiences of adult life.

In earlier analyses based on the data of a cross-sectional survey carried out in 1964, we made provisional assessments of the reciprocal relationship between the substantive complexity of work and many facets of psychological functioning, including intellectual flexibility (Kohn and Schooler 1973; Kohn 1976). In those analyses, we used a method called "two-stage least squares," a relatively simple technique for estimating reciprocal causal models. Our findings constitute prima facie evidence that the substantive complexity of men's work does affect their psychological functioning, independently of the selection processes that draw men into particular fields of work and independently of men's efforts to mold their jobs to fit their needs, values, and capacities. But cross-sectional data cannot provide definitive evidence of causality; only analysis of longitudinal data, measuring real change in real people, can be definitive.

Moreover, while the cross-sectional data provided retrospective information about the substantive complexity of past jobs, it could not provide information about men's psychological functioning at the times they held those jobs. Thus we had no way of statistically controlling earlier levels of intellectual flexibility in assessing the effect of substantive complexity (or anything else) on intellectual flexibility. Nor could we examine lagged effects: for example, we could not assess the effects of earlier levels of intellectual flexibility on the substantive complexity of later jobs. Our analyses were necessarily limited to assessing the contemporaneous reciprocal effects, as of 1964, of the men's then current levels of substantive complexity and psychological functioning.[2]

[2] Robert M. Hauser subsequently suggested an intriguing method of stretching our cross-sectional data to meet some of the requirements of a longitudinal model—synthetic-cohort analysis. The essence of the method, as applied here, is to use data provided by the men who have had the median number of jobs (four) to provide estimates of the correlations involving intellectual flexibility as of 1964 and data provided by the men who have had one fewer than the median number of jobs to provide estimates of the correlations involving intellectual flexibility as of the time of the "immediately prior" job. The synthetic-cohort method enables us to answer this important question: What does the model we have constructed from the cross-sectional data imply about the relationship between earlier and later intellectual flexibility? If the model implies an improbably low correlation between earlier and later intellectual flexibility, this would cast doubt on its plausibility. If, on the other hand, the model implies an

Now we are able to assess the reciprocal effects of substantive complexity and intellectual flexibility much more adequately, for we have conducted a 10-year follow-up survey of a representative portion of our original sample. In this paper, we first describe and evaluate the data of the follow-up study. Then we explain the concepts "substantive complexity" and "intellectual flexibility" and develop "measurement models" for both of them. These models are designed to deal with the most perplexing problem of longitudinal analysis—separating errors in measurement from real change in the phenomena studied. Finally, we utilize the data provided by the measurement models to do a causal analysis of the reciprocal effects of substantive complexity and intellectual flexibility.

THE LONGITUDINAL DATA

The earlier, cross-sectional analyses were based on interviews conducted in 1964 with a sample of 3,101 men, representative of all men employed in civilian occupations in the United States. (For a general description of the sampling methods, see Sudman and Feldman [1965]; for more specific information on sample and research design, see Kohn [1969, pp. 235–64].)

In 1974 the National Opinion Research Center (NORC) carried out a follow-up survey for us, interviewing a representative sample of approximately one-fourth of those men who were less than 65 years old. The age limitation was imposed to increase the probability that the men in the follow-up study would still be in the labor force.

In this study, as in all longitudinal studies, the question of the representativeness of the follow-up sample is crucial for assessing the accuracy of any analyses.[3] Of the 883 men randomly selected for the follow-up study, NORC succeeded in locating 820 (i.e., 93%) 10 years after the original survey—in itself an interesting social fact. Apparently men who live their lives in the ordinary institutions of the society, although they may change residences a great many times, can be traced, given a modicum of cooperation from the post office, telephone company, past employers, and unions.

Of the 820 men located, 35 had died. Of the remaining 785 men, NORC actually reinterviewed 687, that is, 78% of those originally selected and

expectedly high correlation between earlier and later intellectual flexibility, this would increase the plausibility of the model Using the basic theorem of path analysis (Duncan 1966) and following procedures similar to those used by Duncan, Haller, and Portes (1968), we find the implied correlation between earlier and later intellectual flexibility to be .78. Given some degree of unreliability of measurement (an issue we shall presently address more precisely), this strikes us as being about as high a correlation as should be expected. In fact the longitudinal data that we shall soon present show this estimate of the stability of intellectual flexibility to be reasonable, perhaps even too high, for an estimate that does not take measurement error into account.

[3] For an assessment of the representativeness of the original sample, see Kohn 1969, appendix C.

88% of those located and found to be alive. In terms of current experience in survey research, these are certainly acceptable figures. But we must nevertheless ask, Are the men who were reinterviewed representative of all those men whom we meant to interview? Can we generalize safely to the larger universe? We attempt to answer these questions by two types of analysis.

The first type of analysis involves systematic comparison of the social and psychological characteristics of the men who were reinterviewed with those of a truly representative sample of the male working population under the age of 65. We are in an optimum position to do this, for the men who were randomly excepted from the follow-up study constitute a representative subsample of the overall sample and thus are an appropriate comparison group. The differences between the two subsamples are few and small: the men reinterviewed were, as of the time of the original interviews, a little more intellectually flexible, somewhat more trustful, slightly less self-confident, and somewhat more "liberal" in their religious backgrounds than were those in the comparison group. But the two groups do not differ significantly in most of the characteristics important to our analyses—for example, education, social class, major occupational characteristics, age, and even urbanicity.

Our second method of assessing the representativeness of the follow-up sample is to repeat the major substantive analyses, again using the 1964 data but this time limiting the analyses to those men reinterviewed in 1974. The rationale is that, insofar as we get the same results from analyses of the follow-up subsample as from the total sample, we can be confident that the subsample provides a good base from which to generalize to the larger population. We have repeated all the principal analyses of the relationships among social class, occupational conditions, and psychological functioning. The smaller size of the subsample means that several secondary avenues cannot be explored and that some findings are no longer statistically significant. But the main findings hold up uniformly well. Thus we can proceed to analyze the longitudinal data with confidence that whatever we find can be generalized to the larger population of men employed in the United States.

UNRELIABILITY AND CHANGE

We now face the core technical problem in longitudinal analysis—how to separate unreliability of measurement from real change in the phenomena studied. In principle, unreliability in the measurement of the independent variables is especially likely to confound causal analysis (Duncan 1975, p. 117). In the present analysis, both substantive complexity and intel-

lectual flexibility serve as principal independent variables vis-à-vis each other; it is the relative magnitude of their effects that is centrally at issue. Therefore we need to assess how much of any apparent change in substantive complexity represents real change in men's job conditions over the 10-year interval between the two surveys and how much is a function of unreliability in the measuring instruments. Similarly, we need to assess how much of any apparent change in intellectual flexibility represents real change in the men's cognitive functioning and how much is a result of measurement error.

Recently Jöreskog and his associates have introduced powerful new procedures for using maximum-likelihood confirmatory factor analysis to separate unreliability from change (see Jöreskog 1969, 1970, 1973*a*, 1973*b*; Jöreskog, Gruvaeus, and van Thillo 1970; Jöreskog and van Thillo 1972; Jöreskog and Sörbom, 1976*a*, 1976*b*; Werts, Jöreskog, and Linn 1973; Werts, Linn, and Jöreskog 1971).[4] The essence of these methods is the use of multiple indicators for each principal concept, inferring from the covariation of the indicators the degree to which each reflects the underlying concept that they all are hypothesized to reflect and the degree to which each reflects anything else, which for measurement purposes is considered to be error. The test of our success in differentiating "true scores" on the underlying concept from errors in the indicators is how well the hypothesized model reproduces the original variance-covariance matrix of the indicators.

The first step in the use of these procedures is to develop measurement models for the principal concepts—models that will later form the basis for a causal analysis. The measurement models must specify the relationships of indicators to concepts, take account of unreliability (or measurement error) in all the items that measure a concept, and allow for the possibility that measurement errors are correlated in repeated measurements of the same phenomena. For example, any errors in the information obtained in 1964 about the complexity of men's work with "things" or in our coding of this information might well be correlated with errors in the same type of information in 1974. Such correlated error in any constituent item might make our index of substantive complexity seem more or less stable than it really is (see Borhnstedt 1969). Before assessing changes and the reasons for such changes in the substantive complexity of work, we must remove the effects of correlated errors in measurement of the indicators of this concept.

[4] Other instructive discussions of the issues involved in separating unreliability from change are found in Alwin 1973, 1976; Blalock 1969; Burt 1973; Hauser and Goldberger 1971; Heise 1969, 1970, 1975; Heise and Bohrnstedt 1970; Lord and Novick 1968; and Wheaton et al. 1977. Informative applications of the technique are provided in Alwin 1973; Bielby, Hauser, and Featherman 1977; Mason et al. 1976; and Otto and Featherman 1975.

A MEASUREMENT MODEL FOR SUBSTANTIVE COMPLEXITY

By the "substantive complexity of work" we mean the degree to which the work in its very substance requires thought and independent judgment. Substantively complex work by its very nature requires making many decisions that must take into account ill-defined or apparently conflicting contingencies. Although in general work with data or people is likely to be more complex than work with things, this is not always the case, and an index of the overall complexity of the work should reflect its degree of complexity in each of these three types of activity. Work with things can vary in complexity from ditch digging to sculpting; similarly, work with people can vary in complexity from receiving simple directions or orders to giving legal advice; and work with data can vary from reading instructions to synthesizing abstract conceptual systems.

Our information about the substantive complexity of men's work is derived from detailed questioning of each respondent about his work with things, with data or ideas, and with people. Early in the 1964 interview—immediately after learning the title of the respondent's principal occupation and its institutional locus—the interviewer asked these questions:

> One thing we would like to be able to pin down particularly accurately is how much of your working time is spent reading and writing, how much working with your hands, and how much dealing with people. We realize, of course, that you can be doing two or even all three of these at the same time.
>
> 1. First—reading or writing. Here we should like to include any type of written materials—letters, files, memos, books, or blueprints. About how many hours a week do you spend reading, writing, dictating, or dealing with any kind of written materials on your job? (if any time at all.)
>   a) What do you do?
>   b) What are they [the materials] about?
>
> 2. Second—working with your hands, using tools, using or repairing machines. We should like to include everything that involves working with your hands—operating a lathe or a dentist's drill, moving furniture, playing the piano. About how many hours a week do you spend working with your hands on your job? (if any time at all.)
>   a) What do you do? [Probe: What operations do you perform?]
>   b) What materials do you work on?
>   c) What tools or equipment do you use?
>   d) [If relevant.] What do you do to set up and maintain your equipment?
>
> 3. Third—dealing with people. Here we do not mean to include passing the time of day but only conversations necessary for the job: for example, talking to your boss, teaching, supervising, selling, advising clients. About how many hours a week does your job require you to spend dealing with people? (if any time at all.)
>
> What kinds of things do you do—do you teach students, supervise subordinates, receive instructions from the boss, sell to customers, advise

clients, discuss the work with coworkers, or what? [Be sure to ascertai what he does and to whom.]

[If more than one such activity.] At which one of these do you spend th most time?

The foregoing questions provided the basis for seven ratings: appraisal of the complexity of each man's work with things, with data, and wit people; an appraisal of the overall complexity of his work, regardles of whether he works primarily with data, people, or things; and estimate of the amount of time he spends working at each type of activity. In earlie analyses, we subjected these seven ratings to a one-dimensional explorator factor analysis, which we then used as the basis for creating factor scores

Now, instead of using exploratory factor analysis to create a single com posite score, we treat all seven ratings of the 1964 job as "indicators" o the underlying but not directly measured concept, the substantive com plexity of that job (see fig. 1).[5] Each indicator is understood to reflect th underlying concept, which it measures only imperfectly, and some degre of error in measurement (see Alwin 1973, p. 259 and n. 2; and Lord an Novick 1968, p. 72). The follow-up survey asks the same questions an makes the same seven ratings. Again, we treat these ratings as indicator of the underlying concept, in this instance the substantive complexity o the job held at the time of the 1974 interview. Again, we conceive of eac indicator as reflecting the underlying concept, together with some degree o measurement error. We also allow for the possibility that errors of measure ment are correlated over time—that whatever errors there may be in th measurement of complexity of work with things in the 1964 job, for exam ple, may be correlated with errors in the measurement of complexity o work with things in the 1974 job.

We also have information about the complexity of each man's work i two of his earlier jobs, the first job he held for six months or longer an the job held immediately before his 1964 job. Both of these measures ar approximate scores, based on extrapolations from limited job-history in formation (see Kohn and Schooler 1973, pp. 111–12 and n. 21). In ou earlier analyses, we treated these as two separate variables. In the presen analysis, we have no real need for measures of substantive complexity a two separate times before 1964. But the logic of our measurement mode calls for using multiple indicators of important concepts whenever it i possible to do so; it is the multiplicity of indicators that enables us t differentiate unreliability of measurement from change in true score

[5] In fig. 1, as in all subsequent figures and in the text, we present standardized value These are more easily comprehended than metric values, and using them makes i possible to compare indicators in the measurement models and causal paths in th structural equation models (see Blalock 1967). But all computations have been base on unstandardized variance-covariance matrices.
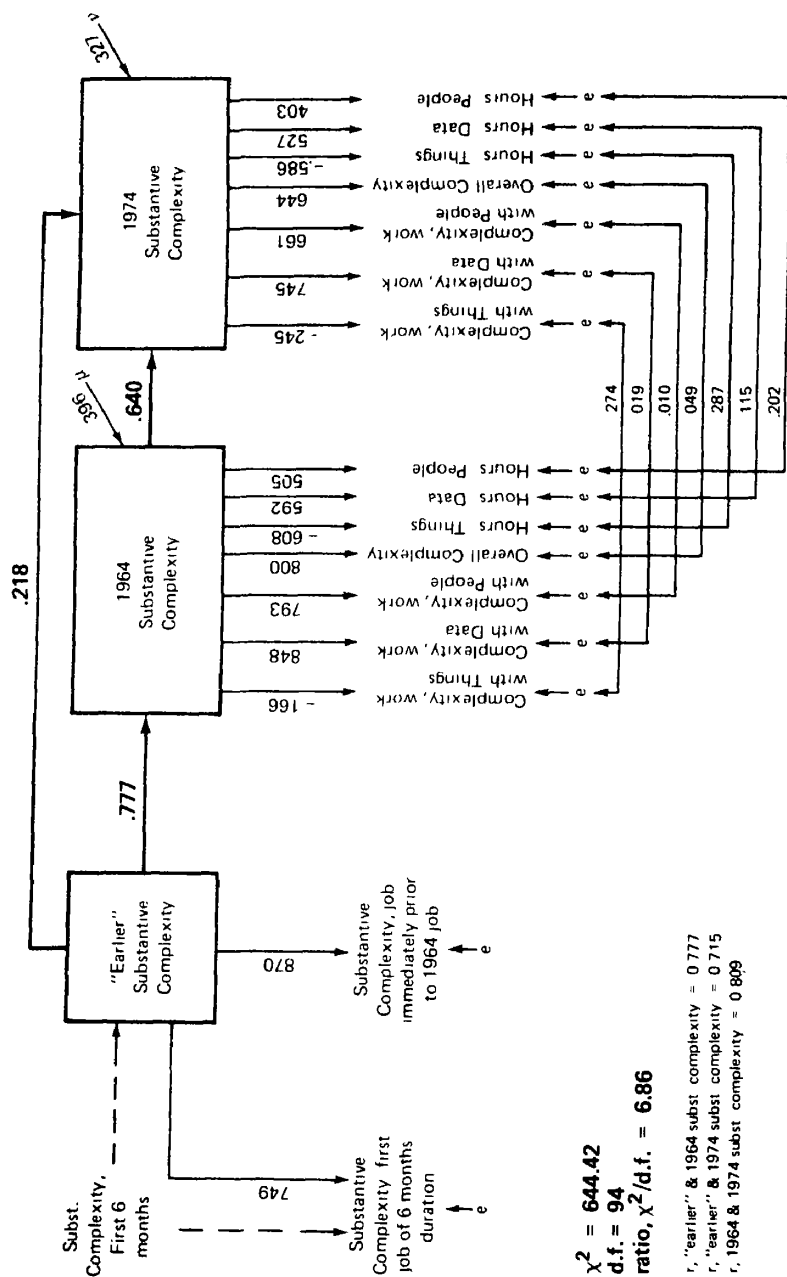
FIG. 1.—Measurement model for substantive complexity (preliminary); figures shown are standardized

We therefore treat these two measures as indicators of a single concept, "earlier" substantive complexity.

The overall fit of this model to the data, based on a χ^2 goodness-of-fit test, is fairly good: the total χ^2 is 644.42, with 94 degrees of freedom, for a ratio of 6.86 per degree of freedom. (In this test, χ^2 is a function of the discrepancies between the actual variance-covariance matrix and the variance-covariance matrix implied by the measurement model. Therefore the better the fit, the smaller the χ^2 per degree of freedom.) But an examination of the first-order partial derivatives of the maximum-likelihood function (see Costner and Schoenberg 1973; Sörbom 1975) indicates that the fit of model to data could be improved by taking into account other correlated errors of measurement—notably correlations between errors in our overall rating of the complexity of each man's work and errors in our specific ratings of the complexity of his work with things, with data, and with people. Allowing these errors to correlate reduces the χ^2 to 439.34, with 88 degrees of freedom, for a ratio of 4.99. (The difference between the initial χ^2 and that in this modified model is 205.08. At 6 degrees of freedom, which is the difference between the degrees of freedom of the two models, this is clearly a statistically significant improvement.) All the parameters of this model are consistent with our earlier analyses. In particular, the complexity of work with data and with people and the overall complexity of the work are shown to bear a strong positive relationship to the underlying concept, while the amount of time spent working with things is shown to be strongly negatively related to the concept in both 1964 and 1974. In these respects, the model accurately reflects both our theoretical intent (see Kohn 1969, pp. 139–40) and the factor loadings of the earlier exploratory factor analysis (see Kohn and Schooler 1973, n. 15).

There is, however, one flaw in this model, as well as in the earlier exploratory factor analysis: the complexity of men's work with things is depicted as being negatively related to the substantive complexity of their work. In principle, complexity of work in any realm—with things as well as data or people—should contribute to the overall substantive complexity of the job. What our model actually reflects is that we had classified men who do not work with things into the lowest category of complexity of work with things; thus men who work entirely with data or with people are classified together with ditch diggers in the lowest category and contrasted with men who do complex work with things.

To achieve a model fully congruent with our theoretical intent, we exclude all men who do not work with things from the computation of correlations involving this indicator, on the rationale that not working with things is qualitatively different from working with things at a low level of com-

plexity.[6] (Concretely, we treat not working with things as "missing data" and use pairwise deletion in computing the correlations.) Again, we allow errors in each indicator to correlate with errors in that same indicator 10 years later. As with the earlier model, the fit of model to data is improved by taking into account other correlated errors. In this model the error correlations that best improve the fit of model to data are those between the complexity of work with things and the amount of time spent working with things, data, and people, both intratime and over time.[7] With these errors allowed to correlate (see fig. 2), the overall χ^2 is 413.81, with 82 degrees of freedom, for a ratio of 5.05, which is nearly the same as that provided by the alternative model. What is more important, this model matches our theoretical intent exactly. The difference in the fit of model to data being minimal and the fit of model to theory much better, we shall employ the model depicted in figure 2 in our causal analyses. But to be certain that the causal inferences we draw are not somehow an artifact of the choice of measurement model, we also compute correlations for the alternative measurement model. Using this model would make no difference in the inferences we draw about the reciprocal effects of substantive complexity and intellectual flexibility.[8]

[6] It is neither necessary nor desirable to follow a similar procedure for men who do not work with data or people, because the logical implications for the measurement of substantive complexity of not working with things are different from those of not working with data or people. Not working with things does not necessarily imply anything about the substantive complexity of the job; certainly it is possible to do substantively complex work that does not involve the direct manipulation of physical objects. On the other hand, not working with written materials or people does imply a low level of substantive complexity; it is hard to imagine a substantively complex job that does not require some reading (e g., of blueprints or specifications) and some discussion with supervisors, co-workers, or others. Moreover, it is rare for a job to require complex work with written materials and not even minimal interaction with people or complex work with people and no reading or writing.

[7] It seems reasonable that these errors might be correlated, because descriptions or evaluations of men's work with things might well affect or be affected by the estimates made of how much time the men spend working in each type of activity. Moreover, measurement errors may be similar for both interviews.

[8] A third approach to measuring substantive complexity, suggested by an *AJS* referee, is to split the sample into the logically possible subgroups defined in terms of whether the men work with things and also with data and people, with specified combinations of two of these three, or with a specified one of these three; then to estimate separate measurement models of substantive complexity for each subgroup for which there is an adequate number of cases; for each of the subgroups, to compute the covariances between the "true scores" for substantive complexity and for other pertinent concepts; and finally to see whether these variance-covariance matrices are essentially the same. If so, we can construct a common variance-covariance matrix for estimating the structural equation models. If not, we would have to estimate different structural equation models for the various subgroups. In fact the only subgroup large enough to provide reliable estimates is that of the men who engage in all three activities—who work with things, data, and also people. We have reestimated the measurement models of substantive complexity and intellectual flexibility for this crucial subgroup and have

FIG. 2.—Measurement model for substantive complexity (final); figures shown are standardized

The most interesting information provided by the measurement model depicted in figure 2 is the estimate of the overall stability in job complexity. The stability, as expected, is substantial; for example, the path from the substantive complexity of the 1964 job to that of the 1974 job is .61, and the correlation between the two is .77. The actual effect of earlier jobs on later jobs, as compared with the effect of other variables, can only be assessed in the causal analyses to come, when other independent variables are simultaneously considered. What the measurement model does tell us is that there has been considerable stability in the substantive complexity of the men's jobs over the course of their careers.

A MEASUREMENT MODEL FOR INTELLECTUAL FLEXIBILITY

Our index of intellectual flexibility is meant to reflect men's actual intellectual performance in the interview situation. In the 1964 interview, we sampled a variety of indicators—including the men's answers to seemingly simple but highly revealing cognitive problems involving well-known issues, their handling of perceptual and projective tests, their propensity to agree when asked agree-disagree questions, and the impression they made on the interviewer during a long session that required a great deal of thought and reflection. None of these indicators is assumed to be completely valid; but we do assume that all the indicators reflect, in some substantial degree, men's flexibility in attempting to cope with the intellectual demands of a complex situation.

We claim neither that this index measures innate intellectual ability nor that intellectual flexibility evidenced in the interview situation is necessarily identical with intellectual flexibility as it might be manifested in other situations; we do not have enough information about the situational variability of intellectual functioning to be certain. We do claim that our index reflects men's actual intellectual functioning in a nonwork situation that seemed to elicit considerable intellectual effort from nearly all the respondents. That our index is not artifactual and that it measures an enduring characteristic is attested to by the evidence—to be presented shortly—of its remarkably high stability over time. Spaeth's (1976) analysis adds to the credibility of the index by showing that the correlations between an earlier variant of our index and various social phenomena are similar to those for more conventional indices of intellectual functioning.

More concretely and specifically, our index (see fig. 3) is based on seven indicators of each man's intellectual performance. These are (1) the Goodenough estimate of his intelligence (see Witkin et al. 1962), based on a

carried out a structual equations causal analysis comparable to that discussed later in this paper for the sample as a whole. This analysis fully supports the conclusions we derive from the causal analysis of the entire sample.
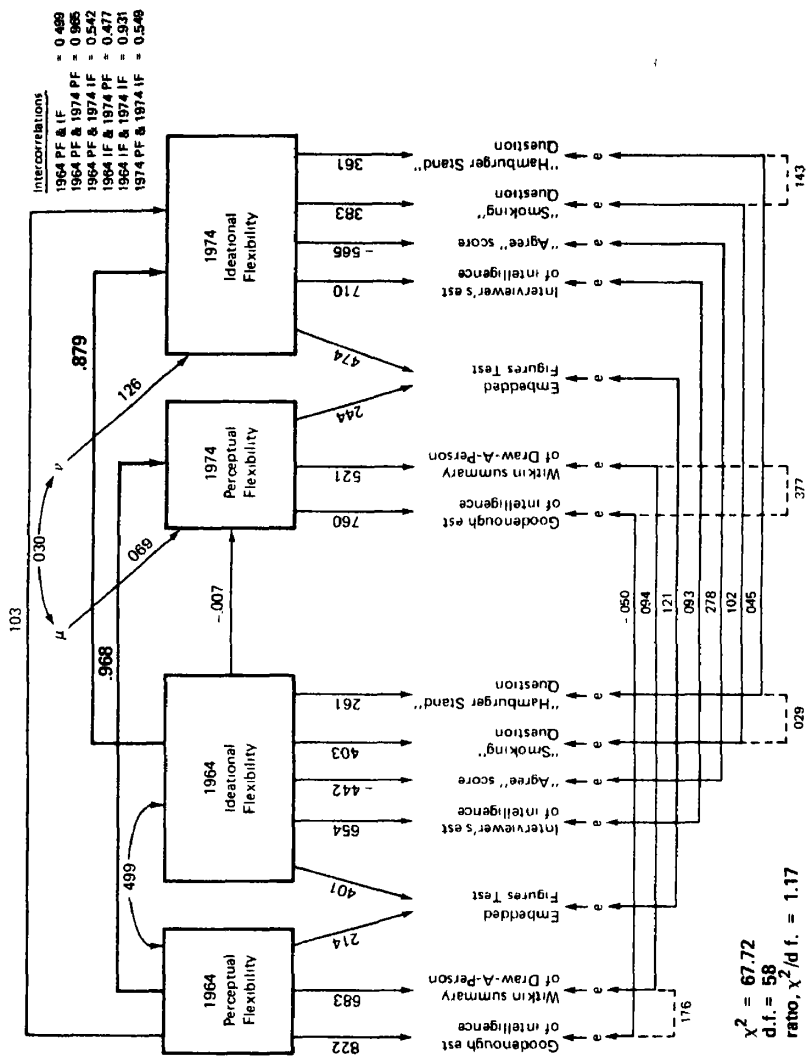
FIG. 3.—Measurement model for intellectual flexibility; figures shown are standardized

detailed evaluation of the Draw-a-Person Test; (2) the appraisal of Witkin et al. (1962) of the sophistication of body concept in the Draw-a-Person Test; (3) a summary score for his performance on a portion of the Embedded Figures Test (see Witkin et al. 1962); (4) the interviewer's appraisal of the man's intelligence; (5) the frequency with which he agreed when asked the many agree-disagree questions included in the interview; (6) a rating of the adequacy of his answer to the apparently simple cognitive problem, "What are all the arguments you can think of for and against allowing cigarette commercials on TV?" and (7) a rating of the adequacy of his answer to another relatively simple cognitive problem, "Suppose you wanted to open a hamburger stand and there were two locations available. What questions would you consider in deciding which of the two locations offers a better business opportunity?"

In the earlier analyses of the 1964 data, we performed an orthogonal principal components factor analysis of these various manifestations of intellectual flexibility. This analysis yielded two dimensions, one primarily perceptual, the other ideational. Since the ideational component of intellectual flexibility is of much greater theoretical interest, our analyses have focused on that dimension.

In the follow-up study, we secured entirely comparable data after elaborate pre-testing to be certain that the cognitive problems had the same meaning in 1974 as in 1964. The measurement model we now employ for intellectual flexibility is similar to that for substantive complexity in most respects, with the following exceptions. First, following the logic of the two-factor model derived from the earlier exploratory factor analysis, we posit two concepts underlying the seven indicators. Second, we have no assessments of intellectual flexibility prior to 1964, so there is nothing comparable to "earlier substantive complexity." Third, to take into account that two of our indicators are based on the same task, the Draw-a-Person Test, we allow those errors to be correlated. Finally, following a lead provided by the first-order partial derivatives, we also allow for the possibility of correlated error between the two cognitive problems.

We get slightly different estimates of the parameters of the measurement model, depending on whether we posit (as in the earlier analysis) that the two underlying concepts are necessarily orthogonal to each other or that they are possibly correlated with one another. The nonorthogonal model provides a significantly better fit to the data, so we shall employ it in the causal analyses that follow. (In any event, estimates of the reciprocal effects of substantive complexity and ideational flexibility prove to be virtually identical, whether based on orthogonal or on nonorthogonal measurement models of intellectual flexibility.)

Figure 3 depicts the measurement model for intellectual flexibility, with

the two underlying concepts allowed to correlate. The model shows tha some of the indicators of intellectual flexibility are not especially reliable this is the very reason we thought it necessary to construct a measuremen model that would differentiate unreliability of measurement from actua intellectual functioning. Judging by the goodness-of-fit test, the model i successful in achieving this objective: the overall χ^2 is 67.72, with 58 de grees of freedom, for a ratio of 1.17, which means that the model provide a very good approximation to the actual variances and covariances of th indicators.

From this model, we learn that the path from men's levels of ideationa flexibility in 1964 to their levels in 1974, shorn of measurement error, is a very substantial .88 and that the correlation between the two is an eve higher .93. We conclude that there has been great stability in men's level of ideational flexibility over the 10-year period.[9] The question for causa analysis is whether, despite this overall stability, there has been enoug individual change for job conditions to have had much of an effect.

CAUSAL ANALYSIS: THE RECIPROCAL EFFECTS OF SUBSTANTIVE COMPLEXITY AND INTELLECTUAL FLEXIBILITY

To do structural equation causal analyses, we have computed the variance and the (unstandardized) covariances between the "true scores" for jo complexity at the various stages of career, intellectual flexibility in 196 and 1974, and all the other variables that will enter into the analyses.[10] These variances and covariances are the data on which the causal model of the reciprocal relationship between substantive complexity and intellec tual flexibility are based.[11]

[9] Parenthetically, the correlation between an index of ideational flexibility based o factor scores derived from exploratory factor analysis of the 1964 data and a simila index based on the same factor loadings for 1974 is a much lower .59. This findin dramatically illustrates the moral that methodologists have long been preaching—tha correlations may be radically understated when they are not corrected for the attenua tion that results from unreliability of measurement.

[10] Our procedure has been to develop measurement models independently for eac concept, compute the covariances among "true scores," and use these covariances a the data for causal analysis. Computing the covariances of the concepts can be don with either the ACOVS or the LISREL computer program. Using either procedure, on develops models combining the principal features of two or more measurement model In developing these combined models, the crucial requirement is to fix, at the value derived from the measurement models, those parameters that define the relationship between concepts and their indicators (e.g., the paths from concepts to indicators, th residuals for the indicators, and the correlations among those residuals) while not inad vertently constraining the interrelationships of the concepts (e.g., when using LISRE by fixing causal paths at zero or some other value instead of allowing them to be fre or by fixing the unexplained variance of any of the concepts).

[11] In developing the causal (i.e., linear structural equation) models, we employed th LISREL computer program (Jöreskog and van Thillo 1972), as subsequently modifie

A reciprocal relationship can occur contemporaneously (albeit not necessarily instantaneously) or more gradually over time. Our earlier analyses, using cross-sectional data, could consider only contemporaneous effects. But there is no reason in principle why substantive complexity and intellectual flexibility might not affect each other both contemporaneously and more gradually over time. We shall therefore assess causal models that allow the possibility of both contemporaneous and lagged reciprocal effects (see fig. 4). Since our concern in these analyses is entirely with the ideational component of intellectual flexibility, we shall henceforth use the terms "ideational flexibility" and "intellectual flexibility" interchangeably and limit the analyses to ideational flexibility.[12]
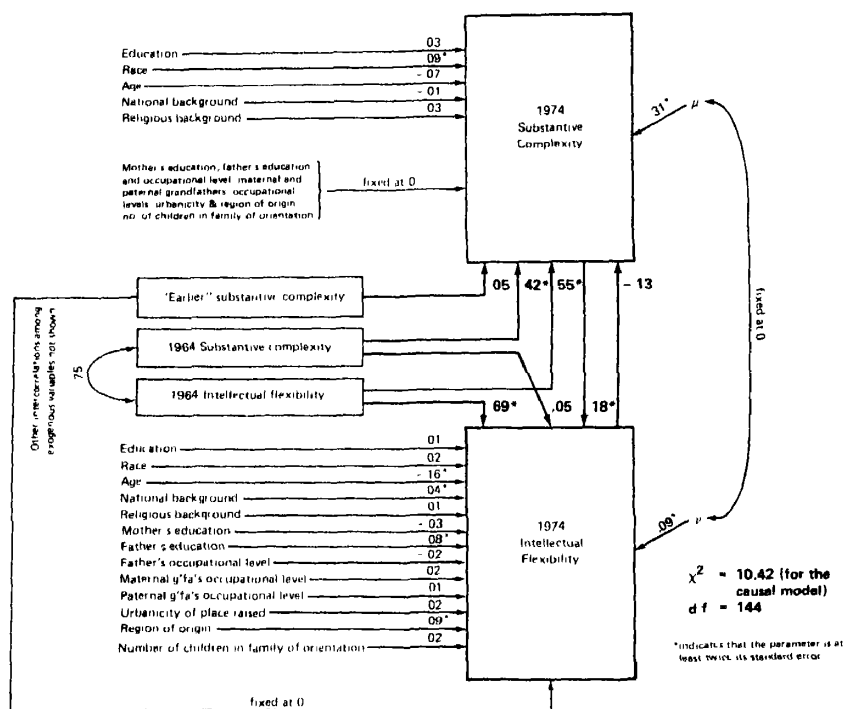
FIG. 4.—Reciprocal effects of substantive complexity and intellectual flexibility: full model; figures shown are standardized.

by Ronald Schoenberg. Pertinent writings on the strategy of this type of causal analysis are those of Burt 1976; Duncan 1975; Heise 1970, 1975; Jöreskog 1973*b* and in press; Jöreskog and Sörbom, 1976*a*; and Werts et al. 1971, 1973.

[12] In fact, the effect of substantive complexity on perceptual flexibility is of approximately the same magnitude as its effect on ideational flexibility. But we cannot assess the reciprocal effect of perceptual flexibility on substantive complexity, because the 1964 and 1974 measures of perceptual flexibility are so highly correlated (at .96) that we encounter insuperable problems of multicollinearity.

The model depicted in figure 4 includes as potentially pertinent exogenous variables all social characteristics that prior research literature and our own earlier analyses give us any reason to believe might affect either substantive complexity or intellectual flexibility. We thus include in the model the respondent's own age and level of education, parents' levels of education, father's occupational level, maternal and paternal grandfathers' occupational levels, race, national background and religious background, urbanicity and region of the country of the principal place where the respondent was raised, and even number of brothers and sisters.[13] We also include as exogenous variables the respondent's 1964 levels of substantive complexity and intellectual flexibility as well as the substantive complexity of his earlier (pre-1964) jobs.

For the model to be adequately identified, the direct effects of one or more exogenous variables on 1974 intellectual flexibility must be assumed to be zero; similarly, the direct effects of one or more exogenous variables on 1974 substantive complexity must also be assumed to be zero; otherwise there will be too little empirical information to solve the equations unequivocally.[14] Therefore, we posit that background characteristics that would not be interpreted as job credentials by employers (even by discriminatory employers) do not directly affect the substantive complexity of the 1974 job; these variables are thus used as instruments to identify the equation. The rationale is that these variables—maternal and paternal education, paternal occuptional level, maternal and paternal grandfathers' occupational levels, urbanicity and region of origin, and number of children in the parental family—may very well have affected men's job placement earlier in their careers. By the time that men are at least 10 years into their careers, however, these variables should no longer have any direct effect on the substantive complexity of their jobs, certainly not when the

[13] The indices of national background, region, and religious background are linear approximations to these nonlinear concepts. In our present use, these linearized indices represent slight underestimates of what would be shown in a more complicated dummy-variable analysis. The rationale for these linearizations is given in Schooler 1972, 1976. Essentially all three indices are ordered in terms of environmental complexity: national background, on the basis of how long it has been since the social organization of the nation's agriculture passed beyond feudalism; region of the United States, on the basis of industrialization and expenditures for education; and religion, on the basis of fundamentalism.

[14] In contemporaneous-effects models (and other "nonrecursive" models), the number of parameters to be estimated will be greater than the amount of information provided by the intercorrelations among the variables, unless some assumptions are imposed on the model, usually by setting some path(s) to zero. (Alternatively, one can impose other restrictions on the model, e.g., as we have in fact done, by not allowing the residuals to be correlated.) The problem of insufficient information is generally referred to as the "identification" problem, and a variable used to help solve the identification problem is called an "instrument." For lucid discussions of this complex topic, see Duncan 1967, 1975, pp. 81–90; and Heise 1975, pp. 160–81. Still pertinent is Simon's (1957) now classic essay on the subject.

substantive complexity of their 1964 and earlier jobs are statistically controlled. Similarly, we posit that the substantive complexity of earlier jobs should have no direct effect on the men's intellectual flexibility in 1974, when the substantive complexity of their 1964 and 1974 jobs are statistically controlled.[15]

As figure 4 shows, a very important determinant of the substantive complexity of the jobs the men held in 1974 is of course the substantive complexity of the jobs they held 10 years before; an even more important determinant of their intellectual flexibility in 1974 is their intellectual flexibility at the earlier time. As we learned from the measurement models, both phenomena, particularly intellectual flexibility, are stable. Nevertheless, the reciprocal effects of substantive complexity and intellectual flexibility are considerable.

The effect of substantive complexity on intellectual flexibility is approximately one-fourth as great as the effect of men's 10-year earlier levels of intellectual flexibility. This effect is essentially contemporaneous:[16] The lagged path from 1964 substantive complexity to 1974 intellectual flexibility is a statistically nonsignificant .05, while the contemporaneous path from 1974 substantive complexity is a more substantial and statistically significant .18.

A path of .18 might not in ordinary circumstances be considered especially striking; but a continuing effect of this magnitude on so stable a phenomenon as intellectual flexibility is impressive, for the cumulative impact is much greater than the immediate effect at any one time. Continuing effects, even small-to-moderate continuing effects, on highly stable phenomena become magnified in importance. The effect of the substantive complexity of work on intellectual flexibility is especially noteworthy when we take into account that we are dealing with men no younger than 26 years of age, who are at least 10 years into their occupational careers.

The reciprocal effect of intellectual flexibility on substantive complexity

[15] These overidentifying restrictions raise a question as to whether the number of degrees of freedom computed by LISREL and presented in figs. 4 and 5 is correct. Some contend that the correct number of degrees of freedom for an overidentified model based on a covariance matrix of "true scores" should exclude those degrees of freedom deriving from the overidentifying restrictions. We do not find this argument convincing. In any event, the number of degrees of freedom in structural equation models is important primarily for giving a comparative basis for evaluating the magnitudes of the χ^2s of alternative models. What really matters in making these comparative judgments is that the procedures be consistent.

[16] A cautionary note is in order here: our analysis does not take into account the length of time the men have been in their present jobs; thus all we mean by "contemporaneous" is that the effect results from the job currently held (for however long, short of 10 years), not from any previous job. A more exact appraisal of the timing of job effects would be exceedingly difficult to accomplish without measurements of both substantive complexity and intellectual flexibility at more frequent intervals than we have made.

is even more impressive—surpassing that of the substantive complexity of the 1964 job. This effect is entirely lagged; that is, it is the men's intellectual flexibility in 1964, not their contemporaneous flexibility that significantly affects the substantive complexity of their 1974 jobs. The longitudinal analysis thus demonstrates something that no cross-sectional analysis could show—that over time the relationship between substantive complexity and intellectual flexibility is truly reciprocal. The effect of substantive complexity on intellectual flexibility is more rapid: current job demands affect current thinking processes. Intellectual flexibility, in contrast, has a delayed effect on substantive complexity: current intellectual flexibility has scant effect on current job demands, but it will have a sizable effect on the further course of one's career. The cross-sectional analysis portrayed only part of this process, making it seem as if the relationship between the substantive complexity of work and psychological functioning were mainly unidirectional, with work affecting psychological functioning but not the reverse. The longitudinal analysis portrays a more intricate and more interesting, truly reciprocal process.

Have we somehow misspecified the equations, leaving out some important variables that might alter the overall picture, or in some other way misconceived the true picture? Since structural models can never be "proved" (Ducan 1975) but only compared with other plausible models, all reasonable alternatives must be considered.

One indication that the equations have not been seriously misspecified is that, if we allow the residuals for 1974 substantive complexity and intellectual flexibility to be correlated, the correlation proves to be nonsignificant and the estimates of all parameters remain essentially unchanged. Thus it seems unlikely that some important variable affecting both substantive complexity and intellectual flexibility has been left out of the model. Moreover, the results do not depend on our choice of instruments, for using a variety of other instruments does not appreciably change the results. Nor do our findings result from the presence of statistically nonsignificant background variables in the model, for deleting the nonsignificant background variables from the predictive equations does not affect our conclusions. In particular, the effect of substantive complexity on intellectual flexibility is slightly strengthened by the deletion of background variables that do not have statistically significant effects. The model is robust, whatever reasonable modifications we try.

Finally, our findings do not result from having fixed the values of the measurement models before estimating the causal model (see Burt 1973). We have confirmed the causal model by developing a "full-information model," in which both measurement and causal parameters are estimated simultaneously. This model confirms both the measurement models and

the causal model depicted in figure 4. In particular, it shows the effect of substantive complexity on intellectual flexibility to be contemporaneous and of the same magnitude as previously shown. The effect of intellectual flexibility on substantive complexity is again shown to be lagged. The magnitude of this path, too, is exactly the same as in figure 4.

The one anomaly in the model shown in figure 4 and the corresponding full-information model is that the path from 1974 intellectual flexibility to 1974 substantive complexity is not just statistically nonsignificant; it is negative. Despite its statistical nonsignificance, the existence of such a negative path suggests a problem of multicollinearity (see Blalock 1963; Farrar and Glauber 1967; Gordon 1968), probably resulting from the very high correlation ($r = .93$) between 1964 and 1974 intellectual flexibility. When we use indices of both 1964 and 1974 intellectual flexibility as independent variables vis-à-vis substantive complexity, we probably exaggerate the importance of 1964 intellectual flexibility while creating an artificially negative effect for 1974 intellectual flexibility. A proper assessment of the effect of intellectual flexibility on substantive complexity requires dropping the statistically nonsignificant contemporaneous path from the model. Similarly, a proper assessment of the effect of substantive complexity on intellectual flexibility requires dropping the statistically nonsignificant lagged path from the model. Therefore in figure 5 we delete these (and all other) nonsignificant paths and reestimate the model. This, we believe, represents the most accurate assessment that can be made of the overall effects of substantive complexity and intellectual flexibility on each other.

This model shows the contemporaneous effect of substantive complexity on intellectual flexibility to be of virtually the same magnitude (a path of .17) as that shown in figure 4. Even with a slightly higher estimate of the stability of intellectual flexibility (at .71), the effect of substantive complexity on intellectual flexibility remains nearly one-fourth as great as that of the men's 10-year earlier levels of intellectual flexibility.

The lagged effect of intellectual flexibility on substantive complexity (a path of .45) is not quite as great as it appeared to be before we removed the nonsignificant negative contemporaneous path in figure 4, but by any other standard it is very large. Intellectual flexibility surpasses even the substantive complexity of men's 1964 jobs as a determinant of the substantive complexity of their 1974 jobs.

As we did for the model depicted in figure 4, we have confirmed figure 5 by developing a full-information model, in which measurement and causal parameters are simultaneously estimated.[17] All parameters of the measure-

[17] The χ^2 for the full-information model comparable to fig. 4 is 1,035.23, with 546 degrees of freedom, for a ratio of 1.90, a remarkably good fit of model to data for so complex a model based on so large a number of cases. The χ^2 for the full-information model comparable to fig. 5 is nearly the same—1,036.68, with 548 degrees of freedom, for a ratio of 1.89. Nothing is lost in the fit of model to data in simplifying the model.
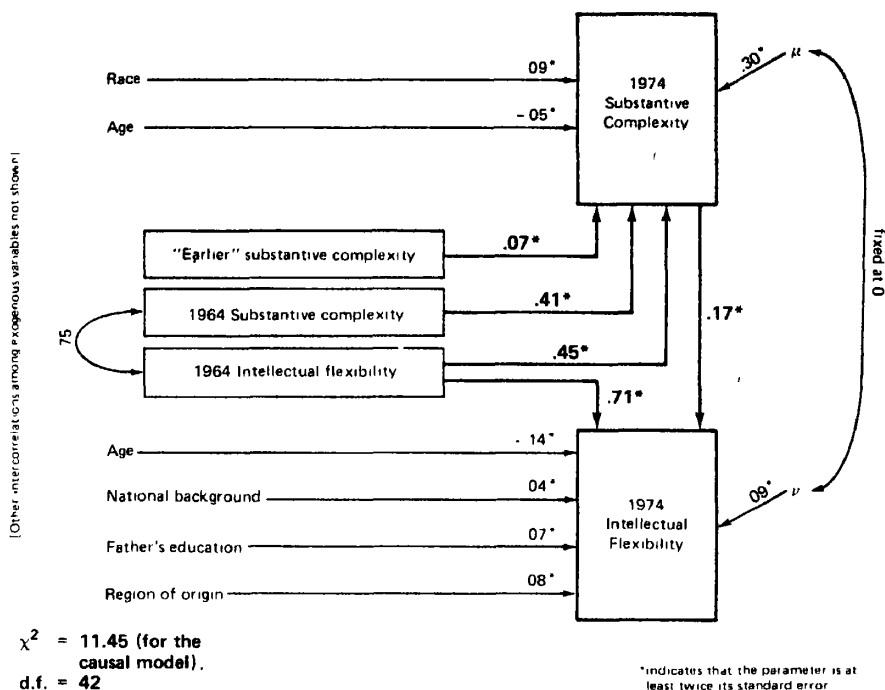
FIG. 5.—Reciprocal effects of substantive complexity and intellectual flexibility: significant paths only; figures shown are standardized.

ment models for both substantive complexity and intellectual flexibility are very close to those shown in figures 2 and 3, most of them nearly identical, none differing by more than .03. The causal model, too, is confirmed, the contemporaneous path from substantive complexity to intellectual flexibility being exactly as we had found it to be in figure 5 (.17), the lagged path from intellectual flexibility to substantive complexity being slightly lower (.41 vs. .45). All other causal parameters are very close to those shown in figure 5.

The data thus demonstrate beyond reasonable doubt what heretofore could be stated as only a plausible thesis buttressed by presumptive evidence—that the substantive complexity of men's work both considerably affects and is considerably affected by their intellectual flexibility.[18]

[18] Although our primary interest is the reciprocal effects of substantive complexity and intellectual flexibility, the model is also instructive in telling us about the effects of social background on men's jobs and cognitive functioning in mid- and later career. By the time men are at least 10 years into their occupational careers, only race and age continue to have direct effects on the substantive complexity of their jobs; other aspects of social background have certainly affected the substantive complexity of earlier jobs, but by this career stage not even education has a statistically significant direct effect. Age is negatively related to 1974 substantive complexity when the com-

DISCUSSION

There are several limitations to the analyses reported in this paper: some we hope to remedy in further analyses; the others may be beyond our ingenuity or the scope of our data.

One obvious limitation is that our analysis has been restricted thus far to substantive complexity and intellectual flexibility. In our further work, we intend to develop measurement models for other facets of occupational structure and other facets of psychological functioning. Our hope is ultimately to develop a causal model of the overall relationship between occupational structure and psychological functioning.

Second, the present analysis, as was true of all our previous analyses of occupational conditions, deals only with men. We are now analyzing data for women and intend in the near future to report on the relationship between women's occupational conditions and their psychological functioning.

A third limitation is that, although our measurement models take account of unreliability in indicators of the two central concepts, they have not dealt with possible unreliability of measurement for education, race, age, or any other aspect of social background. Our own data do not provide any solid basis for assessing reliability of measurement of these variables, and we are dubious about using reliability coefficients derived from other bodies of data as the basis for correcting correlations for attenuation. It would probably not make any real difference in the causal analysis. But without evidence, the issue must be left unresolved.

Fourth, conspicuously lacking in our treatment of these data is a systematic analysis of "career" patterns. We have treated prior jobs (and even the same job, held 10 years earlier) as if all series of jobs were equally continuous or discontinuous along some meaningful career line. But a more realistic conceptualization would have to take into account that some job changes represent logical progression in a meaningful sequence, while others represent shifts out of one career sequence, perhaps into another. We know of no really satisfactory way of dealing with this issue, despite early efforts by Wilensky (1961) and several more recent efforts by others (see Ladinsky 1976) at classifying career patterns.

plexity of prior jobs is statistically controlled. Since the correlation in 1964 was positive, this probably indicates that over the 10-year period the younger men in the sample have been closing the gap. It is possible, however, that our finding reflects a curvilinear relationship between age and substantive complexity or a cohort effect. Similarly, the data suggest that blacks are catching up with whites in the substantive complexity of their jobs, probably a reflection of lessening discrimination (see Featherman and Hauser 1976; Farley 1977). In their effects on intellectual flexibility, age, national background, father's education, and region of origin (not education, race, size of parental family, or urbanicity) continue to be statistically significant when prior intellectual flexibility is statistically controlled. None of these variables, not even age, has as large an effect on intellectual flexibility as the substantive complexity of work.

Fifth, both our measurement and causal models assume that relationships among variables are essentially the same for all segments of the work force. The assumption is obviously testable, and we hope to test it, although we recognize that a thorough assessment may require a much larger body of data than ours. It would be especially desirable to examine these models separately for workers at different ages and different stages of career.

Sixth, as must be apparent to the reader, changes in occupational circumstances are not the only ones that people experience in a 10-year interval: some marry, divorce, or become widowed; traumas and joyous events occur; these and other occurrences may exacerbate, mitigate, or deflect the processes our models depict. We have information about these events, but it is too early in the analytic process to tell whether the depth of the data and the size of the sample are adequate for analyzing the part they play in the ongoing process.

Despite these limitations, we believe that we have shown, more definitively than has ever been done before, that the relationship between occupational conditions and psychological functioning is reciprocal: people's occupational conditions both affect and are affected by their psychological functioning.

These findings come down solidly in support of those who see occupational conditions as affecting personality and in opposition to those who see the relationship between occupational conditions and personality as resulting solely from selective recruitment and job molding. We do not deny that personality has great importance in determining who enter what types of jobs and how they perform those jobs; in fact our analyses underline the importance of these processes. But that has never been seriously at issue. What has been disputed is whether the reverse phenomenon—of job conditions molding personality—also occurs. The evidence of our study unequivocally supports the position that it does occur. Thus our findings bear directly on an issue central to the field of social structure and personality—whether social-structural conditions affect personality only during childhood socialization or continue to do so throughout adulthood. Here is clear evidence that one important facet of social structure—the substantive complexity of work—directly affects adult personality.

In particular, this study adds to and helps specify the growing evidence that the structure of the environment has an important effect on cognitive development (see Rosenbaum 1976) and that cognitive processes do not become impervious to environmental influence after adolescence or early adulthood but continue to show "plasticity" throughout the life span (see Baltes 1968; Horn and Donaldson 1976; Baltes and Schaie 1976). Our findings reinforce this conclusion by showing that intellectual flexibility continues to be responsive to experience well into mid-career and probably

beyond. In fact the remarkable stability of intellectual flexibility appears to reflect, at least in part, stability in people's life circumstances. Intellectual flexibility is ever responsive to changes in the substantive complexity of people's work; for most people, though, the substantive complexity of work does not fluctuate markedly.

This study also demonstrates the importance of the impact of intellectual flexibility on substantive complexity. We think it noteworthy that this effect appears to be lagged rather than contemporaneous. The implication is that the structure of most jobs does not permit any considerable variation in the substantive complexity of the work: job conditions are not readily modified to suit the needs or capacities of the individual worker. Over a long enough time, however—certainly over a period as long as 10 years—many men either modify their jobs or move on to others more consonant with their intellectual functioning. Thus the long-term effects of intellectual flexibility on substantive complexity are considerable, even though the contemporaneous effects appear to be negligible.

Our models, of course, start in mid- or later career. There is every reason to believe that men's levels of intellectual flexibility in childhood, adolescence, and early adulthood may have had an important effect on their educational attainments, and our data show that educational attainment has had an extremely important effect on the substantive complexity of the early jobs in men's careers. Since such complexity is a primary determinant of the substantive complexity of later jobs, it seems safe to infer that intellectual flexibility's long-term, indirect effects on the complexity of later jobs is even greater than our analysis depicts.[19]

In the broadest sense, our findings support our general strategy for studying the relationship between social structure and personality. We have consistently argued that, in interpreting the relationship between social structure and individual psychological functioning, one should always ask how a person's position in the larger social structure affects the conditions of life that directly impinge on him (see Kohn 1963, 1969, 1977; Kohn and Schooler 1969, 1973; Schooler 1972, 1976; see also Olsen 1974).

[19] Our analysis thus has implications for that part of the process of social mobility left unanalyzed in most discussions—the paths from the occupational status of the first job to that of later jobs. The reciprocal relationship between substantive complexity and intellectual flexibility implies an internal dynamic by which relatively small differences in occupational status at early stages of career may become magnified into larger differences later in the career. If two men of equivalent intellectual flexibility were to start their careers in jobs differing in substantive complexity, the man in the more complex job would be likely to outstrip the other in further intellectual growth. This in time would probably lead to his attainment of jobs of greater complexity, further affecting his intellectual growth. As a result, small differences in the substantive complexity of early jobs might lead to increasing differences in the substantive complexity of later jobs. Since substantive complexity is closely tied to occupational status, it is probable that differences in the status of the men's jobs would also be magnified by this process.

Thus, in attempting to interpret the relationship between social class and values and orientation, we saw class-correlated differences in occupational conditions as a potentially important bridge between position in the hierarchical ordering of society and conceptions of reality (Kohn 1969; Kohn and Schooler 1969). Substantive complexity is particularly important, for on one hand the substantive complexity of work is closely linked to the job's location in the stratificational system, and on the other the substantive complexity of people's work is correlated with their values and orientation. Our past research demonstrated that the relationship between social class and values and orientation could reasonably be attributed, in large degree, to class-correlated differences in such occupational conditions as substantive complexity. But not even our two-stage least-squares analysis (Kohn and Schooler 1973) demonstrated conclusively that substantive complexity has an actual causal effect on values, orientation, or any other psychological phenomenon. The present analysis buttresses our analytic strategy by showing that substantive complexity actually does have a causal impact on one pivotal aspect of psychological functioning, intellectual flexibility.

Admittedly, our research has not yet demonstrated that substantive complexity directly affects values or self-conception or social orientation—in fact anything other than intellectual flexibility. Still, intellectual flexibility is the crucial test. Because of its remarkable stability, intellectual flexibility offers the most difficult challenge to the hypothesis that substantive complexity actually affects some important aspect of psychological functioning. Moreover, intellectual flexibility is tremendously important in its own right. Finally, we see intellectual flexibility as intimately related to values, self-conception, and social orientation. It is in fact an important link between social class and self-directed values and orientation (Kohn 1969, pp. 186–87; Kohn and Schooler 1969). Thus demonstrating the causal impact of substantive complexity on intellectual flexibility gives us every reason to expect substantive complexity to have a causal impact on values and orientation, too. In our further analyses, we shall assess the hypothesized causal impact of substantive complexity—and of other structural imperatives of the job—on values, self-conception, and social orientation. For now, one crucial causal link in the relationship between social structure and psychological functioning has been conclusively demonstrated.

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Social Class and Self-Esteem among Children and Adults[1]

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The literature on the relationship of social class to self-esteem is riddled with contradictions, showing positive, null, and inverse relationships. Two studies examining this relationship are compared—one, a sample of children aged 8–18; the other, a sample of adults aged 18–65. The results indicate virtually no association for younger children, a modest association for adolescents, and a moderate association for adults. Four principles of self-esteem development are advanced to account for these conditional relations—social comparison processes, reflected appraisals, self-perception theory, and psychological centrality. It is suggested that these principles apply equally to adults and children and that the identical principles help to explain why social class should have different effects on the self-esteem of children and adults.

After decades of research on both social class and self-esteem, it is somewhat surprising to find so little firm knowledge about their relationship. Perhaps this is because social class has commanded the attention of sociologists while self-esteem has primarily concerned psychologists; or it may be that investigators have considered it pointless to attempt to establish a conclusion too obvious to require confirmation. And, indeed, the argument that the individual's social status should be related to his personal feeling of worth seems so self-evident as to represent a virtual triviality. Although there certainly are successful people consumed with self-doubts and unsuccessful ones entirely satisfied with themselves, nevertheless, as William James ([1890] 1950, pp. 306–7) long ago noted: "One may say, however, that the normal *provocative* of self feeling is one's actual success or failure, and the good or bad actual position one holds in the world. . . . he who has made one blunder after another, and still lies in middle life among the failures at the foot of the hill, is liable to grow all sicklied o'er with self-

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distrust, and to shrink from trials with which his powers can really cope."

James's impressions are entirely consistent with Mead's (1934) theory of reflected appraisals. If the essential characteristic of social class, in the Weberian sense of "status group" (Gerth and Mills 1946), is unequal prestige, then one would expect those looked up to by society to develop a high level of self-respect, and vice versa.

Obvious though these expectations are, the data are surprisingly inconsistent, at times showing no relationship (or even inverse relationships), at other times modest relationships, and at still other times moderate ones. The aim of this paper is to suggest that beneath this diversity is an underlying consistency which can be understood on theoretical grounds. The pattern of relationships, we believe, has been obscured by failing to take account of the factor of age and, hence, by overlooking the fact that a social structural variable, such as social class, may signify a radically different set of social experiences and may be endowed with entirely different psychological meanings for individuals of unequal maturity. The most obvious demonstration of this point is that, for the adult, social class is achieved (at least in principle), whereas for the child it is unequivocally ascribed. From a sociological perspective, then, the fundamental meaning of social class differs for children and adults. The bulk of this paper will be devoted to advancing certain theoretical reasons for expecting the relationship of social class to self-esteem to vary across the age span and to examining these theoretical ideas in the light of empirical data. But, first, it is necessary to consider the empirical basis for the assumption that this relationship is stronger among adults than among children.

SOCIAL CLASS AND SELF-ESTEEM: CHILDREN, ADOLESCENTS, ADULTS

The data for this paper are drawn primarily from two sources. The first is a sample of Baltimore City public school children. A random sample of 2,625 pupils distributed among 25 schools was drawn from the population of pupils in grades 3 through 12. Each school in Baltimore was initially stratified by two variables: (1) proportion of nonwhite and (2) median income of census tract. Twenty-five schools falling into the appropriate intervals were randomly selected, and 105 pupils chosen within each school by random procedures. Interviews were conducted with 1,917 pupils, that is, 79% of the sample still registered in school and 73% of those originally drawn from the central records. Sampling considerations obliged us to assign double weight to one combined elementary and junior high school; as a result, the basis of our analysis amounts to a sample of 1,988 pupils in 26 schools.

The study of adults is based on information gathered through scheduled

interviews with a sample of 2,300 people, aged 18–65, representative of the census-defined urbanized area of Chicago (U.S. Bureau of the Census 1972), which includes sections of northwestern Indiana as well as some of the suburban areas of Chicago. The sample was drawn in clusters of four households per block and used a total of 575 blocks, one-fourth the total sample of 2,300. The 1970 census reports that there are 2,137,185 households in the Chicago urbanized area (U.S. Bureau of the Census 1972); when this total is divided by the total number of blocks in which households were to be chosen (575), the result, 3,716, is the skip factor for the selection of households. That is, every 3,716th household was selected; the three additional households of the block cluster were then chosen by dividing the total number of households on each block by four and using the result as the factor for counting from the initially selected address. Among those contacted, 30% refused to be interviewed. In anticipation of refusals and to make allowance for households where contact could not be established within three callbacks, substitute addresses in each block were also prelisted. The targeting of households thus was entirely separated from the interviewing.

In the school pupil study, self-esteem is measured by a 6-item Guttman scale (coefficients of reproducibility and scalability are 90% and 68%, respectively). Issues of reliability and validity of measurement are dealt with in the Appendix. The adult study uses the 10 items of the Rosenberg Self-Esteem Scale (Rosenberg 1965), scoring them according to the Likert method. Because race operates as a suppressor variable (Rosenberg 1968, 1973*a*; Davis 1971) in the school pupil data,[2] its effect is statistically controlled for both children and adults in table 1.

Table 1 shows virtually no association between social class, measured by the Hollingshead Index of Social Position (Hollingshead and Redlich 1958, pp. 387–97), and self-esteem among the 8–11-year-olds (partial gamma = +.029) and a modest association for early adolescents (for 12–14-year-olds, partial gamma = +.072) and for later adolescents (for those 15 or older, partial gamma = +.102). In the adult data, on the other hand, the relationships are considerably stronger: controlling for race, the relationship (partial gamma) of education and self-esteem is +.197, that of occupation and self-esteem, +.160, and that of income and self-esteem, +.233. In order to determine whether the associations in these various groups are

[2] A suppressor variable is one which operates to conceal the true strength of the zero-order relationship. Whereas extraneous and intervening variables reduce the strength of the original zero-order relationship, suppressor variables increase it. In the pupil study, the black children were more likely to be in the lower class but also more likely to have higher self-esteem. The fact that this test factor was negatively related to the independent variable but positively related to the dependent variable obscured the true relationship between social class and self-esteem in this sample. Hence, we have partialed out the effects of race.

TABLE 1

SOCIAL STATUS AND SELF-ESTEEM AMONG BALTIMORE SCHOOL PUPILS AND CHICAGO ADULTS (CONTROLLED FOR RACE)

	BALTIMORE SCHOOL PUPILS (Social Class and Self-Esteem, by Age)			CHICAGO ADULTS		
	8–11	12–14	15+	Education and Self-Esteem	Occupation and Self-Esteem	Income and Self-Esteem
Partial gamma . .	+.029	+.072	+.102	+ 197	+.160	+.233
N . . .	(699)	(571)	(468)	(2,288)	(1,689)	(1,988)

significantly different from one another, Pearson r's have been compared (Blalock 1972, pp. 405–7). For the 8–11 group, the correlation is significantly different from all three adult correlations at the .01 level; for the 12–14 group, the differences are all significant at the .08 level; and for the 15+ group, they are all significant at the .05 level. The three age groups, however, are not significantly different from one another.

Results of earlier research have been too inconsistent to permit simple comparisons with our findings. Among younger children, several studies show inverse relationships between social class and self-esteem (Clark and Trowbridge 1971; Trowbridge 1970, 1972; Piers 1969; Soares and Soares 1969, 1970, 1972), and some show null or close to null relationships (Coleman et al. 1966; Long and Henderson 1970; St. John 1971). Among adolescents, Bachman (1970), Rosenberg (1965), and Jensen (1972) show weak associations between social class and self-esteem, Epps (1969) shows null or close to null relationships, and Soares and Soares (1972) show an inverse relationship. Finally, among adults, Weidman, Phelan, and Sullivan (1972) and Yancey, Rigsby, and McCarthy (1972) report clear positive relationships, Luck and Heiss (1972) find positive associations within educational groups, and Kaplan (1971) finds no overall association (although relationships appear under certain conditions).

To these findings must be added the results of two unpublished studies of adults. One of these is Middleton's investigation of over 900 black and white adult men (Middleton 1977), which showed the following zero-order correlations of social class with self-esteem: education, $r = .359$; occupational status, $r = .366$; and income, $r = .378$. Although these relationships decline somewhat when controls are introduced, they remain reasonably strong—certainly stronger than in any of the data concerning children.

The second unpublished study is Kohn and Schooler's (Kohn 1969; Kohn and Schooler 1969) nationwide sample of 3,101 working men. Socioeconomic status was measured by the Hollingshead scale and self-esteem by a Guttman scale of global self-esteem. The correlation, which is highly significant, is $r = .1929$.[3]

Since studies of adults and children often use different measures of self-esteem, different measures of social class, different types of samples, different measures of association, and different statistical controls, one cannot compare the exact strengths of the relationships. Nevertheless, we believe the available evidence is reasonably consistent with the Baltimore and Chicago findings, which show virtually no association between social class and self-esteem among preadolescents, a modest relationship among adolescents, and a moderate relationship among adults.

[3] We wish to thank Melvin Kohn for preparing this and several other special tabulations for the purposes of this paper.

The most compelling explanation of this relationship among adults is that advanced by Kohn (1969) and Kohn and Schooler (1969). In their nationwide study of 3,101 working men, they demonstrated that members of different social classes differ radically in the types of occupational activities in which they are actually engaged. The job imperatives are such that the work of those in higher status positions is characterized by a high level of occupational self-direction—an opportunity to make one's own decisions, to exercise independent judgment, to be exempt from close supervision—in large part because of the substantive complexity of the work. Kohn (1969, p. 184) showed that when occupational self-direction was controlled, the originally significant relationship of social class to two self-esteem factors vanished almost completely.[4] From these findings, we may deduce that one reason social class has little effect on the self-esteem of children is that children are not yet exposed to the class-related occupational conditions that help to shape self-esteem.

This paper seeks to add to our understanding by advancing additional theoretical reasons for expecting the observed variations across the age span, exemplified in table 1, to occur. Two general explanations will be suggested: The first is that social class organizes the interpersonal experiences of children and adults in different ways; the second is that social class is interpreted within different frameworks of meaning by children and adults. These two factors, we believe, can help us to understand why the identical social structural variable (social class) can produce different psychological outcomes (self-esteem) for different social groups (children and adults).

This is not to suggest that different general principles of self-esteem formation operate among children and adults. On the contrary, we shall attempt to show that the principles are identical. Indeed, it is the operation of these identical principles which help us to understand why social class, on theoretical grounds, should have different effects on the self-esteem of children and adults. Four theoretical ideas relevant to self-esteem formation will be considered: social comparison processes, reflected appraisals, self-perception theory, and psychological centrality.

[4] Kohn's (1969) and Kohn and Schooler's (1969) two factors of self-confidence and self-deprecation are based on six of the 10 Rosenberg self-esteem items. These items also satisfy the requirements of a Guttman scale and may, for present purposes, be treated as a single self-esteem measure. This has the advantage of making the data more comparable with most of the other studies of adults which have combined the Rosenberg items into a single scale. As noted earlier, the original association of social class and the Guttman scale of self-esteem is $r = .1929$. When this relationship is controlled on the index of occupational self-direction, however, the relationship reduces to partial $r = .0470$—a proportional reduction of 76%. One cannot, of course, be absolutely certain that this reduction in the social class–self-esteem relationship is due solely to occupational self-direction and not at all to some related variable; but there can be little doubt that the job imperatives play a major role.

SOCIAL COMPARISON PROCESSES

In 1954 Leon Festinger set forth his theory of social comparison processes, holding that "there exists, in the human organism, a drive to evaluate his opinions and his abilities. . . . To the extent that objective, non-social means are not available, people evaluate their opinions and abilities by comparison respectively with the opinions and abilities of others" (p. 117–18). One could add that it is not only opinions and abilities but also social identity elements—groups, statuses, and social categories—that are evaluated. The individual compares his own group or position with that of other people. Nowhere is this more apparent than with reference to social class. The very meaning of social class is founded on the idea of inequality—of superior and inferior, higher or lower, better or worse. Since social class involves the vertical ordering of members of society along a prestige continuum, it is inherently comparative and invidious. The special relevance of social class for self-esteem obviously rests in the comparison of one's prestige with that of other people.

But with whom does the individual compare his social status? Does he compare it with some general social average or with a narrower population subgroup? This must depend, we believe, on the homogeneity or heterogeneity of the socioeconomic environment in which the individual is ensconced. Under conditions of visible social class heterogeneity, the higher classes may pride themselves on their superior status, and the lower classes may be made painfully aware of their relative inferiority (Kaplan 1971, p. 46). In the company of peers, on the other hand, status neither enhances nor diminishes self-esteem. In other words, in a completely classless—or single class—environment, class will have no effect on self-esteem because no one is below or above anyone else.

One reason social class makes less difference for the self-esteem of the child, we suggest, is that the SES of his interpersonal environment is more homogeneous. In examining this proposition, two questions will be considered. First, is the younger child more likely than his seniors to perceive his environment as socioeconomically homogeneous? Second, is the socioeconomic environment of the young child actually more homogeneous?

The Baltimore data provide rather clear evidence that children tend to perceive their socioeconomic environments as homogeneous. When asked, "Would you say that most of the kids in your class at school are richer than you, the same as you, or poorer than you?" fully 93% of the subjects said "the same." Only a miniscule proportion of subjects of any class considered themselves either above or below most of those in their school environments.

Furthermore, subjects were asked, "Do you know any kids whose families are richer than your family?" and "Do you know any kids whose fami-

lies are poorer than your family?" "Any" is a large umbrella, indeed. It turns out that the younger the child, the less likely is he to say that he knows either richer children or poorer children. Among the youngest, 24% said that they knew richer kids and 32% that they knew poorer ones. Among the oldest, 66% said that they knew richer kids, and 71% that they knew poorer ones (Simmons and Rosenberg 1971). (In fact, the association between age and knowing "richer kids" is gamma = .5189 and between age and knowing "poorer kids" is gamma = .4933.) As the child grows older, he apparently becomes increasingly conscious of economic inequality.

Although the relative obliviousness of younger children to economic inequality is due in part to limited social learning, the interesting point is that it also reflects reality. Children are more likely to perceive their environments as socioeconomically homogeneous because these environments are in reality more homogeneous. Two items of evidence point to this conclusion.

First, most children, of whatever SES level, attend schools whose average socioeconomic level is similar to their own. Irrespective of age, the great bulk of lower-class children attend schools whose average socioeconomic status is low, higher-class children, ones where it is high. Among the younger children, only 3% of the lower-class (Hollingshead Class V) children, compared with 50% of the higher-class children (Hollingshead Classes I and II), attend higher-class schools (school SES mean 3.0 or above); in the 12–14 age group, the corresponding figures are 5% and 53%; and among those 15 or older, the figures are 13% and 82%. Expressed another way, the relationships between the individual's class and the social class mean of his school for the various age groups are as follows: 8–11 years, gamma = .6173; 12–14 years, gamma = .6395; 15+ years, gamma = .5891.

When children of whatever age say their SES is pretty much "the same" as that of those in their environments, then, they are simply describing reality. But what about the fact that adolescents are more likely to be aware of some socioeconomic heterogeneity in their environments, to say that they know at least some children who are richer or poorer than themselves? Is greater socioeconomic heterogeneity more likely to be a part of their experience? If we look at the SES standard deviations of schools attended by younger and older children, we find a second item of evidence: The standard deviations tend to be larger for older subjects (table 2). For example, while 47% of the youngest children attended schools with a relatively small standard deviation (under 0.7), this was true for only 20% of the oldest subjects. Nor is this surprising. High schools are not only much larger than elementary schools but also, drawing from a much wider geographical area, tend to recruit children of more diverse socioeconomic

TABLE 2

AGE BY STANDARD DEVIATION OF SCHOOL SES (%)

School SES (SDs)	Age 8–11	12–14	15+
Less than .7	47	35	20
.7 to 1.09	46	61	51
1.1 or more	7	4	29
$N = 100\%$	(821)	(649)	(514)
	$\chi^2 = 324$; $df = 4$; $P < .001$; gamma = .2809		

backgrounds. Hence, it is understandable that the older child should be more likely to encounter a wider range of poor and well-to-do in his school.

The younger the child, then, the greater the perceived and actual homogeneity of his school (and probably neighborhood) environment. If children's interpersonal environments are such that most SES comparisons are with equals (or perceived equals), it is understandable that, feeling neither above nor below others, their social class should neither raise nor lower their self-esteem.

The adult experience, to be sure, is not entirely dissimilar: a good deal *of adult interaction also goes on within socioeconomically homogeneous* environments.[5] Yet it is clear that adults are more conscious of, and more exposed to, socioeconomic inequality.

It will be recalled that 93% of the children, asked whether they were richer, poorer, or the same as most schoolmates, replied "the same." In the Chicago adult study, subjects were asked, "Would you say your total family income is higher, lower, or about the same as the following groups: Most of your friends? Most of your relatives? Most of your neighbors?" As one might expect, many adult subjects also considered themselves average in terms of these comparable membership groups. Nevertheless, 43% saw themselves as either richer or poorer than most of their friends, 61% thought they were above or below most of their relatives, and 49% considered themselves economically superior or inferior to most of their neighbors.

Furthermore, it is this feeling of economic superiority or inferiority that partly accounts for the relationship between social class and self-esteem among adults. Table 3 shows that the zero-order gamma of income and self-esteem is .2485; controlling for whether they consider themselves richer or poorer than friends, the partial gamma = .1509. For relatives, the corre-

[5] Although Max Weber (Gerth and Mills 1946) stressed the contrast between classes and status groups, in the long run, he suggested, people in the same tax bracket end up dancing with one another.

TABLE 3

INCOME AND SELF-ESTEEM, CONTROLLING FOR COMPARISON WITH PEERS, AMONG ADULTS PERCEIVING THEIR INCOME AS HIGHER OR LOWER THAN OTHERS

Controlling for Whether Subject Considers Self Richer or Poorer Than . . .	Income and Self-Esteem			
	N	Zero-Order Gamma	Partial Gamma	Reduction (%)
Friends. . .	(421)	.2485	.1509	39
Relatives. . .	(594)	.2594	1834	29
Neighbors	(423)	2462	1598	35

sponding figures are .2594 and .1834; for neighbors, .2462 and .1598. I other words, among adults who consider their income unequal to that o their "peers," between 29% and 39% of the relationship between incom and self-esteem is apparently attributable to social comparison processe Invidious economic comparisons thus appear to play some part in ac counting for the relationship between income and self-esteem among adult although other factors are also involved.

These data, however, do not adequately reflect the degree of socioeco nomic heterogeneity that actually characterizes the adult environment. Th reason is that it is not among friends, neighbors, and relatives, after all that the individual experiences the greatest SES heterogeneity (by an large, these are his peers) but rather in the world of work. Within thes institutional structures—the worlds of business, industry, and governmen —life is organized in hierarchical fashion. In such environments, there i virtually no way to evade the brute reality of differences in power, prestige and possession. In these organizations, everyone knows who gives th orders and who takes them; who commands great respect and who little who is paid well, who poorly. The adult certainly knows, at least in a gros way, where he stands along the continuum. It is thus the experience of a adult in the world of work which calls to his attention his place in a recog nized stratification system. Although the environment and experience o the school child is such that he may be relatively oblivious to the system o social stratification, nothing is more omnipresent in the life of the adult. I it any wonder that, comparing himself with others he encounters, th adult's social class should show a stronger relationship to his self-esteen than the child's?

The general principle of social comparison, of course, applies equally t children and adults. The self-esteem of both groups is influenced by com paring themselves with those around them. But, given their social roles the interpersonal environments of children and adults differ radically

Schooling is, after all, the main business of the child's life; working, the main business of the adult's. In school, children (especially younger ones), whatever their own SES levels, tend to be more or less equal to most of those around them; there is nothing in this to raise or lower their self-esteem. In the world of work, on the other hand, the social comparisons adults make place them above or below others and may understandably affect their feelings of self-worth. Both children and adults make social comparisons, but the differing structures of interpersonal relations in school and work place make SES irrelevant to the child's self-esteem, relevant to the adult's.

REFLECTED APPRAISALS

Since the process of human communication obliges the individual to respond to himself from the standpoint of others (Mead 1934), it is generally assumed that people come to see themselves as they believe others—particularly significant others (Sullivan 1953)—see them. In this simplified form, the theory of reflected appraisals—the theory that we come to see ourselves as we perceive others as seeing us—has been clearly supported by empirical research (Miyamoto and Dornbusch 1956; Reeder, Donohue, and Biblarz 1960; Manis 1955; French 1968; Quarantelli and Cooper 1966; Sherwood 1965; Rosenberg 1965). The results are clear and unequivocal: There is a stong and definite relationship between the "perceived self" (Miyamoto and Dornbusch 1956) and the individual's own picture of what he is actually like. The Baltimore data showed that 62% of the children who believed their significant others thought well of them but only 20% of those who thought they did not had high global self-esteem. It is thus what we believe others think of us that usually counts.

Assuming that the attitudes of others toward us affect our self-esteem, the critical question is, Do those people with whom we predominantly interact and about whose opinions we most care judge us by our social class position? Do they respect us because we are in a higher class or lack respect for us because we are in a lower class?

In order to deal with this question in a nomothetic rather than an idiographic sense, we must ask which categories of people characteristically enter the individual's experience by virtue of his location in the social structure. Who has opinions of us and whose opinions we care about are not so much matters of accident as of our "role-sets," that is, "the complement of role relationships *which persons have by virtue of occupying a particular* social status" (Merton 1957, pp. 368–84). To use Merton's example, the status of the public school teacher involves patterned relationships with incumbents of a number of other statuses—with pupils, with colleagues,

with the school principal, with the superintendent of schools, with the board of education, with parent-teacher associations, and so on. The child also has his characteristic role-set. The child has patterned relationships with his mother, father, siblings, other relatives, teachers, friends, and classmates; in fact, the overwhelming bulk of the child's interactions are with people with whom he has primary relations.

The relevance of these observations is this: Whereas the adult's role-set consists largely of status unequals, almost all of the individuals who constitute the child's role-set are of his own socioeconomic status. This is true of his mother, father, and siblings; probably true of his school and neighborhood peers; and possibly true of his teachers or more remote relatives. This is not to say that these others do not judge and evaluate the child. On the contrary, they may rank him high in looks, medium in intelligence, and low in cleanliness; they may admire his athletic ability, but have a poor opinion of his charm; they may approve of his friendliness, yet deplore his laziness. But one thing that parents, siblings, and probably peers neither admire nor deplore, approve nor disapprove, respect nor disrespect is his social class. No father looks up to his child as a member of the social elite; no brother looks down on him because of his working-class origins. Yet mothers, fathers, siblings, and peers are precisely those people whose opinions count most to him and who, as research (Rosenberg 1973*b*; Rosenberg and Simmons 1972) indicates, most powerfully affect his self-esteem. Among possible status unequals, teachers alone are likely to have comparable influence. In sum, if significant others neither look up to nor down on the child by virtue of his socioeconomic status, then, taking the role of these others, his objective SES should have little impact upon his feeling of self-worth.

Contrast this situation with the adult's status-set, particularly his occupational role-set. To return to Merton's example of the teacher, he or she may be treated deferentially by the illiterate parent, but imperiously by the highly educated parent; may be condescended to by the school principal, yet addressed respectfully by the janitorial staff. Not all of these people, of course, are significant others, but neither are their views of the self irrelevant. Since work is such an important part of the adult's life, what those who enter his work experience think of him is likely to have a bearing on his self-attitudes.

Furthermore, consider the adult's family statuses. A father cannot consider his child an economic failure, but a child can consider his father an occupational or economic failure. A wife may express satisfaction with her husband's occupational success or dissatisfaction with his failure, and so, in fact, may the adult's own father, mother, brothers, sisters, and other relatives. Unlike the child, many of the individuals who enter the adult's

fe by virtue of his structural location in the society—his various role-sets -do view him in social status terms.

In addition, the adult's status-set is obviously much broader than that f the child. For the child, most interactions with others are exhausted by ie categories of others who enter his role-set as child; for the adult, role-:ts beyond occupation and family are involved. Hence, the adult's status-:t brings him into contact with many more people who view him in status :rms; to the extent that he sees himself from their viewpoints, his social .atus should affect his self-esteem.

Again, we see that an identical principle—reflected appraisals—helps xplain why social class should have different effects on the self-esteem of iildren and adults. A child's self-attitudes are at least as influenced by thers' attitudes toward him as an adult's; but if others do not judge him ı status terms, then status will have little effect on self-esteem.

Thus far, in calling attention to social comparison processes and reflected ppraisals, we have suggested that the significance of social class for self-steem depends on the interpersonal context in which it is embedded. The nmediate environments and role-sets of children and adults are such that ocial class enters the interpersonal experiences of children and adults in ifferent ways, with corresponding effects on self-esteem.

But the differential impact of social class on self-esteem must also take ito account how social class enters the phenomenal fields of children and dults. In other words, the significance of social class for the feeling of self-orth also depends on the subjective meaning assigned to the objective fact f social class and on the position of social class in the individual's structure f awareness and values. Two additional principles of self-esteem forma-on will therefore be invoked: self-perception theory and psychological entrality.

ELF-PERCEPTION THEORY

n advancing self-perception theory as an alternative explanation for cogni-ive dissonance phenomena, Bem (1965, 1967) set forth the radical be-avioristic view that we come to know ourselves not by introspection but y observation of our overt behavior. In this view, we learn about ourselves ı much the same way that others learn about us—by observing and inter-reting our own behavior and its outcomes. If this is true of such inner tates as attitudes, motives, and intentions, it is still more obviously true f more readily recognizable dispositions, such as abilities. Someone who ries skiing for the first time and does well at it may come away with a ew view of himself as a skier. We thus draw conclusions about ourselves

in part by observing our actions or their outcomes (e.g., successes or failures).

But our self-regard depends primarily on what we have done, secondarily on what our ego-extensions have done.[6] As noted earlier, for the adult, socioeconomic status is achieved; for the child, ascribed. For the former, this status (high or low) is earned—the outcome of his efforts and his actions; for the latter, it is conferred—the product of another's accomplishments. If people evaluate themselves largely in terms of their own behavior, it is certainly easy to understand why social class should be more closely tied to the self-esteem of adults than of children.

Children's self-esteem, it should be stressed, is probably just as dependent as adult self-esteem on achievement; but this achievement is their own, not their parents'. Several items of evidence support this assertion. First, many studies (summarized by Purkey 1970) show a consistent (though not always strong) relationship between school marks and self-esteem. Second, there is evidence that sociometric status and self-esteem are related (Rosenberg 1965). Third, the student's election as an officer of a high school club has been found to be related to his self-esteem (Rosenberg 1965; Rosenberg and Simmons 1972). What the child himself has wrought, then, appears to bear on his self-esteem; his father's achievements, on the other hand, are not his own.

But the identical principle applies to adults. What the adult has achieved may affect his feeling of self-worth, but what his father has achieved is largely irrelevant. For example, the Chicago study showed a relationship of gamma = .18 between the individual's own occupation and his own self-esteem and of gamma = .07 between his father's occupation and his own self-esteem. The point is that the partial gamma of the father's occupation and one's own self-esteem, controlling on own occupation, is only gamma = .01. Even if the father's status helped one to get ahead (or otherwise) in the first place, it is the proximal fact of one's own success or failure, not the distal fact of paternal prestige, that directly affects the adult's self-esteem.

Like the other principles of self-esteem formation, self-perception theory applies equally to children and adults. The reason social class affects the self-esteem of adults more than of children is that different interpretations are assigned the same structural facts by members of these age categories. The general implication is that the impact of a structural arrangement on

[6] Except insofar as the ego-extension's achievements are viewed as a product of one's own efforts. A mother may take pride if her child achieves professional success, an athletic coach if his pupil wins an Olympic victory, a professor if his former assistant writes a successful book, because the achievements of these ego-extensions are felt to represent the outcomes of one's own efforts and energies. In the present context, a parent may feel responsible for what the child accomplishes, but a child will scarcely feel responsible for what the parent accomplishes.

a psychological disposition depends on the meaning of that objective fact to the individual involved.

PSYCHOLOGICAL CENTRALITY

Unlike self-esteem, which is a comparatively straightforward idea (a global positive or negative attitude toward the self), the self-concept is an extremely intricate, complex, multifaceted structure. The chief elements or components of the self-concept are dispositions (e.g., intelligence, kindness, morality, optimism) and social identity elements (e.g., race, sex, religion, age, social class). (For more elaborate and sometimes differing views, see Kuhn and McPartland 1954; Gordon 1968, 1976; Brissett 1972; Franks and Marolla 1976; Rosenberg 1976.)

But we cannot understand the self-concept without considering the relationship among its components. In the words of Combs and Snygg (1959, p. 126): "The phenomenal self is not a mere conglomeration or addition of isolated concepts of self, but a patterned interrelationship or Gestalt of all these." Some elements of the self-concept are at the center of attention, at the heart of the individual's major concerns, others at the periphery; some self-values are critically important, others are of the utmost triviality. Thus, the impact of any given component on global self-esteem will depend on its importance or unimportance, centrality or peripherality, in the individual's cognitive structure.

The relevance of this point has been demonstrated in several empirical investigations. A study of adolescents (Rosenberg 1965) showed that a high self-rating on a trait was most closely related to global self-esteem when the trait was one that the youngster considered very important to himself. The principle of psychological centrality applies not only to dispositions but to social identity elements as well. One study (Rosenberg 1976) showed a small relationship between the individual's pride in his race or religion and his self-esteem. But, again, the relationship proved to be stronger among those who felt their race or religion to be an integral part of the self than among those who did not.

The question we shall raise here is whether the same principle applies to social status, that is, Does social status have a particularly powerful bearing on the self-esteem of those who value status highly?

Take the matter of income. Those with higher income, we have seen above, are more likely to have high self-esteem than the economically less successful. But the strength of this relationship, it turns out, depends on how much importance the individual attaches to money, how prominent it is in his scale of values. For example, the Chicago adults were asked to agree or disagree with the statement, "One of the most important things

about a person is the amount of money he has." Table 4 shows that the more strongly the individual agrees with this statement, the more powerful is the relationship between his income and his self-esteem. Among those who strongly agree, the relationship of income to self-esteem is gamma = .52; for those who agree somewhat or disagree somewhat, the gammas are .39 and .37; for those who strongly disagree, gamma = .21.

As noted earlier, the Chicago study showed a clear though moderate association between adult occupational status and self-esteem. But this association, we find, depends also on the importance attached to status. Subjects were asked, "How important is it to you to move to a higher prestige class?" Among those who considered it very or somewhat important, the association is gamma = .25; but among those considering it unimportant, gamma = .11 (table 5).

The impact of social class or status on global self-esteem, then, depends in large measure on its psychological centrality for the individual.[7] The importance of this point for our argument lies in the fact that social status is more psychologically central to the adult than to the child. Adults are more aware of, attuned to, concerned with social status than children; the adult is more likely to perceive his world in terms of stratification, to be alert and sensitive to his own and others' social rank. Let us consider some of the evidence bearing on this point.

One indication of whether class plays a major role in the individual's cognitive structure is simple familiarity with the vocabulary of stratification. Although it is possible to be class conscious without knowing the term, in general we would expect awareness of the term to be associated with awareness of the concept. In the Baltimore study, children were asked, "Have you ever heard about 'social class,' or haven't you ever heard this term?" The results (Simmons and Rosenberg 1971, p. 244) show that only 15% of the elementary school subjects had ever heard the term, compared with 39% of those in junior high school and 75% of those in senior high school.

The identical question was asked of a sample of younger and older English and American school subjects (Stern and Searing 1976). (The younger English children were 11–12, the older, 15–16; the younger American children were 12-13, the older, 17–18.) In both countries, the results were the same. The older subjects were much more likely to have heard of the term "social class." Although we are aware of no comparable data

[7] Such a valuation of self-concept components, of course, is no mere accident. People not only attempt to be successful at those things they value, but they also elect to value those things at which they are successful. The point is that whatever factors operate to make a disposition or social identity element psychologically central in the first place, once it acquires that location in the individual's phenomenal field, its impact on the individual's global feeling of self-worth is amplified.

TABLE 4

SELF-ESTEEM, LEVEL OF FAMILY INCOME (in Thousands) AND IMPORTANCE ATTACHED TO MONEY BY ADULTS

	CONSIDER MONEY "ONE OF THE MOST IMPORTANT THINGS"											
	Strongly Agree			Somewhat Agree			Somewhat Disagree			Strongly Disagree		
SELF-ESTEEM	16+	8–16	8–	16+	8–16	8–	16+	8–16	8–	16+	8–16	8–
High	72	26	16	35	24	17	42	23	15	51	47	32
Medium	6	40	22	38	24	13	30	22	18	24	22	24
Low	22	35	62	27	51	70	28	55	67	25	31	43
$N=100\%$	(18)	(43)	(69)	(48)	(115)	(83)	(81)	(224)	(123)	(325)	(563)	(292)
	$\chi^2=30$; $df=4$; $P<.001$; gamma $=.5222$			$\chi^2=23$; $df=4$; $P<.001$; gamma $=.3868$			$\chi^2=31$; $df=4$; $P<.001$; gamma $=.3688$			$\chi^2=31$; $df=4$; $P<.001$; gamma $=.2140$		

TABLE 5

SELF-ESTEEM, OCCUPATIONAL STATUS, AND THE IMPORTANCE OF STATUS AGGRANDIZEMENT FOR ADULTS

	IMPORTANCE OF MOVING TO HIGHER PRESTIGE CLASS											
	Great or Some				Little				None			
SELF-ESTEEM	Low and High Professionals	Clerks, Technicians, and Sales	Skilled	Un- and Semi-skilled	Low and High Professionals	Clerks, Technicians, and Sales	Skilled	Un- and Semi-skilled	Low and High Professionals	Clerks, Technicians, and Sales	Skilled	Un- and Semi-skilled
High	43	34	24	21	45	38	36	29	53	42	49	38
Medium	25	26	32	19	24	25	23	26	22	29	21	20
Low	32	40	44	60	31	37	41	45	25	29	29	42
$N=100\%$	(47)	(129)	(82)	(99)	(97)	(191)	(152)	(144)	(108)	(197)	(160)	(123)
	$\chi^2=17$; $df=6$; $P<.01$; gamma $=.2498$				$\chi^2=8$; $df=6$; $P<.21$; gamma $=.1413$				$\chi^2=13$; $df=6$; $P<.04$; gamma $=.1074$			

among adult samples, it seems unlikely that the sharp rise in awareness demonstrated in adolescence should not continue into adulthood; probably few adults have never heard the term "social class."

There are other indications that the younger the child, the less aware he is of a social class system. In the Stern and Searing (1976) study, younger subjects were less likely to believe that "there are social classes in England (the United States)," to deny that "there are no such things as social classes nowadays," and to deny that "in England (the United States) we are all middle class really." Similarly, when respondents in the Baltimore study who said they had heard of social classes were asked which of four classes (upper, middle, working, or lower) they belonged to, 11% of the younger but only 3% of the older said "don't know."

Equally important from the self-esteem viewpoint is the accuracy of the individual's perception of his own location in the stratification system. If, for example, there was a random association between objective-class and subjective-class identification, then an objectively high position would not generate feelings of pride nor an objectively low position, feelings of shame.

We shall draw upon data from three studies to examine whether children are in fact less likely to be aware of their actual status in society.

Table 6 presents the following data: (1) the association of objective-class (Hollingshead scale) and subjective-class identification among those Baltimore children and adolescents who had heard the term "social class" and who identified with one of the social classes; (2) two measures of class association among Chicago adults: that between occupational status and prestige-class identification and that between income and income-class identification; and (3) the association between objective-class (Hollingshead scale) and subjective-class identification among Kohn and Schooler's (1969) national sample of adult working men.

The differences are striking. Among the youngest children, there is vir-

TABLE 6

OBJECTIVE-AND SUBJECTIVE-CLASS IDENTIFICATION

Association of Objective and Subjective Class	Baltimore Children* (by Age)			Chicago Adults		Nationwide Male Adults§
	8–11	12–14	15+	Prestige Class†	Income Class‡	
Gamma...	−.0184	.1972	.4412	.4912	.6935	.3954
N...	(88)	(163)	(304)	(1,234)	(1,935)	(3,074)
$P <$..	142	.193	001	.001	.001	.001

* Among those who had heard of social class and who identified with a class.

† Occupational level and prestige-class identification.

‡ Income and income-class identification.

§ Kohn and Schooler nationwide sample of 3,101 men using the Hollingshead SES measure.

tually no association (gamma = —.0184) between their objective-class and their social class identification. In the 12–14 age group, gamma = .1972; in the 15 or older group, gamma = .4412.[8] Three measures of association appear for adults. In the Chicago study, the occupation–prestige-class identification association is gamma = .4912, and the income–income-class identification association is gamma = .6935. Finally, the Kohn and Schooler study of 3,101 working men shows an objective-class–subjective-class identification association of $r = .3954$. If the correlations of objective and subjective class among each of the three school age groups are compared with each of the three adult correlations, they all prove to be significantly different at the .01 level. The 8–11 and 12–14 age groups are also significantly different from the 15+ age group at the .02 level, but they are not significantly different from one another.

Thus, adults appear to be far more accurate than children about their relative status position in society. In particular, they are highly sensitive to income differences; the rich know they are richer, the poor, poorer. Although there is variation in results depending upon the samples and indicators used, in general among adults objective position does bear a strong and clear relationship to where one sees oneself in the stratification system.

Compared with their seniors, then, the data suggest that younger children are less likely to have heard of the term "social class"; if they have heard of it, they are less likely to have formed an opinion of where they belong; and if they have formed such an opinion, they are more likely to be mistaken. It does not seem rash to conclude that social status is less psychologically central to children than to adults. And, in the light of our earlier discussion, it is understandable that objective social status should have less impact on a child's self-esteem than on an adult's.

Like the other principles enunciated above, the principle of psychological centrality is just as significant for children as for adults. The objective fact of social class, however, has a different personal relevance for adults and children and, hence, different self-esteem consequences. To the child, the social stratification system and his place in it is relatively vague, blurred, inaccurate, and peripheral; hence, objective class has little bearing on his self-esteem. For the adult, the reverse is the case. The significance of the objective demographic fact thus depends on the place of that fact in the individual's psychological field.

[8] The developmental trend observed in Baltimore is reproduced in other research. Stendler (1949) showed that younger children had less accurate perceptions of others' statuses than older ones. About two-thirds of the first graders' estimates of their peers were wrong, in contrast to only a fourth of the eighth graders'. And Stern and Searing (1976, p. 185) note: "By the end of secondary school, family social class correlates with self-selected social class labels at gamma = .46 . . . in England and gamma = .58 in the United States"—results consistent with gamma = .44 among Baltimore adolescents.

DISCUSSION

If social science is to go beyond the level of description, it is imperative to understand how a demographic variable enters the individual's life, is converted into interpersonal experiences, is processed by a particular cognitive structure, and reflects the individual's relationship to his environment. Ultimately, all experience must be filtered through the sieve of the individual mind; what is essential is to distinguish which objective experiences constitute an actual part of the individual's life and how these are interpreted within his own phenomenal field. We have attempted to illustrate this point by comparing the impact of social class on the self-esteem of children and of adults.

It is important to stress that we are not speaking of differences in the cognitive processes of children and adults. On the contrary, the general principles governing self-esteem among children and adults are, we believe, identical. Two of these—social comparison processes and reflected appraisals—deal with interpersonal processes. Both children and adults learn their worth, in part, by comparing themselves with others; for social structural reasons, such comparisons are more likely to be socioeconomic among adults than among children. Again, both children's and adults' self-attitudes are influenced to a large degree by the attitudes of others toward themselves. Again, for social structural reasons, the others with whom the adult interacts are much more likely than those with whom the child interacts to judge him in social class terms.

The other two general principles—self-perception theory and psychological centrality—pertain to the psychological treatment of social structural facts. It is not simply the objective fact but what one makes of the fact that determines its impact on the individual. Both children and adults learn their worth by observing their behavior and its outcomes; by the adult, social class is interpreted as an outcome of one's own behavior, by the child, it is not. Again, for both children and adults the impact of a given self-concept component (disposition or social identity element) on global feeling of self-worth depends on the awareness and importance of that element to the individual. For structural reasons, adults are more aware of their actual location in the stratification system and probably attach more importance to it.

In sum, one cannot understand the significance of a social structural variable for the individual without learning how this variable enters his actual experience and is processed within his own phenomenal field. As both the phenomenologists and the symbolic interactionists remind us, facts must be interpreted within their "meaning contexts." In emphasizing the importance of an interactional or subjective interpretation of objective reality, we are not reducing sociological forces to psychological ones but

specifying intervening variables which themselves may be shaped by objective forces.

Although the primary purpose of this paper has been to show why social class *should* affect the self-esteem of adults, we have found it equally important to attempt to demonstrate why social class should *not* have this effect on children. If we hope to appreciate the meaning of social class for the child, it is essential to see social class from his viewpoint, to adopt the child's eye view of stratification, to understand how it enters his experience and is processed internally. To the sociologist, social class means differential prestige, respect, possessions, and power, with obvious self-esteem implications. But from the viewpoint of the child, the matter appears entirely different. First, the child, as he looks around himself in the actual world of the school and neighborhood in which he lives, finds that most of the children he meets are socioeconomically much like himself—neither richer nor poorer. His effective interpersonal environment, which provides the primary social experiences which enter his phenomenal field, is largely a classless society, a world in which status plays little or no role. (This is not to say that it is unstratified—age and authority stratification is sharp and strong—but it is not socioeconomically stratified.) Second, the child sees himself through the eyes of others, and what he believes they think of him largely affects his self-esteem. And, indeed, significant others may think well or ill of him in many ways but rarely with reference to socioeconomic status. Third, even if the child knows where he stands, he is surely aware that this position has nothing to do with his own efforts or accomplishments. It is certainly understandable that he should feel greater pride or shame in his school marks or athletic skill than in paternal accomplishments. Fourth, he is relatively oblivious to the social class system. Since his actual social class position has only a modest connection with the class with which he identifies, one can understand why the young child is unlikely to take pride in high status or experience shame in low status. The differential association of social class to self-esteem for children and adults stems from the different social experiences and psychological interpretations associated with this structural fact in these age groups.

APPENDIX

Since a weak association of social class and self-esteem among children might be due to measurement error, it is important to consider issues of reliability and validity. Guttman (1950, chap. 8) has shown that a high coefficient of reproducibility necessarily insures instrument reliability. Since age comparisons are involved, however, it is necessary to consider whether this reliability level is the same among children of different ages or in dif-

ferent groups. For the Baltimore self-esteem scale, the coefficients of reproducibility among black children of different ages are as follows: 8–11, 90%; 12–14, 90%; 15 and older, 91%. For white children, the corresponding figures are 91%, 90%, and 90%. There is also evidence of convergent validity of the Baltimore measure, although it is limited to pupils in the seventh grade or higher. These subjects were administered seven of the 10 items on the Rosenberg Self-Esteem Scale (RSE). (A discussion of the validity of this measure appears in Rosenberg 1965, chap. 2; Wylie 1974; Robinson and Shaver 1973; Silber and Tippett 1965.) The relationship of the Baltimore and the truncated RSE measure was gamma = .6119. (The RSE items were not considered suitable for use with those in the lower grades.) Construct validity (Cronbach and Meehl 1955; American Psychological Association 1954) was assessed by examining the relationship between the self-esteem measure and other variables with which it should, on theoretical grounds, be associated. The relationship of the Baltimore self-esteem measure to the following variables was examined (figures in parentheses are gammas): depression scale (.31); anxiety score (.34); "perceived self" (Miyamoto and Dornbusch 1956), that is, how the child believes significant others see him (.31); school marks (.10); and, among secondary school club members, club presidency (.17). These relationships are all in the expected direction and, with the exception of club presidency (due to the small number of club members), statistically significant. In addition, the relationships of the Baltimore measure and the RSE measure to these other variables are very similar. (Further details on the internal reliability, convergent validity, and construct validity of the Baltimore self-esteem scale appear in Rosenberg and Simmons 1972, chap. 2.)

As far as social class measurement error is concerned, the data from secondary school pupils were obtained from the pupils themselves, but the data for elementary school pupils were obtained from the parents (precisely because young children are often unaware of the objective facts relevant to social class). There is no particular reason to expect any difference in social class measurement error between the adult sample and the parents of children, although it is possible that adolescent reports are less accurate.

There is evidence to indicate that the sample is representative of the population from which it is drawn (the public school population of Baltimore from grades 3 through 12). A comparison of sample data with population data shows an impressive similarity in terms of race, grade level, size of school, racial context of school, and median family income of census tract of school. For example, the population is 64.1% black, the sample 63.6% black. The sole deviation is some overrepresentation of elementary and underrepresentation of senior high school pupils (for details, see Rosenberg and Simmons 1972, app. A).

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Models of Relevance: The Social Effects of Sociology[1]

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Most sociologists predicate their work on the conviction that it will have at least some beneficial social effects. But ideas vary greatly as to how such effects may come about. Perhaps the most difficult problem for proponents of any such "model of relevance" is to show how social insight can serve the general good when it addresses conflict situations in which help to one set of interests necessarily entails damage to others. Several different models of relevance are considered, as manifested in the writings of both classical and contemporary writers, including Sumner, Merton, Marx, Mills, and Barrington Moore, Jr. None of the formulations is found altogether satisfactory in resolving the problems inherent in building such models.

"You shall know the truth," the Scriptures tell us, "and the truth shall make you free." Some version of this doctrine seems to sustain most systematic social inquiry. True, not all students of society would necessarily regard freedom as the most important ultimate aim of their activities. Many would give first importance in this regard to the alleviation of human suffering, or the resolution of grave social conflicts, or the promotion of enlightened official policies. But nearly all would want to argue that their work has some usefulness which goes beyond their own satisfaction in carrying it out. These notions of how social inquiry can and should redound to some benefit for society as a whole are not just vague intimations; potentially they represent sociological models in their own right.

This study deals with such models, which I term models of relevance. I hold that these models play an extremely influential role in shaping sociological work—not only applied sociology, but also purely academic studies.[2] Perhaps everyone has noticed that some sociological writings read as though

[1] A fuller version of this article will appear as a chapter in my forthcoming book, *Insight and Social Betterment: A Preface to Applied Social Science* (New York· Oxford University Press, 1978). I wish to thank Lewis Coser, Forrest Dill, George Homans, Robert Keohane, Thomas Luckmann, Terry Rosenberg, and Theda Skocpol for their interest and assistance in this study. As always, the shortcomings are solely the responsibility of the author.

[2] For convenience, I am couching this argument in terms of sociology. Actually, I believe that many of the same points would apply, *mutatis mutandis*, to other social science disciplines.

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addressed to potential activists or an aroused public, while others seem intended for a policymaking elite. Often the first resound with ringing judgments and moral indignation, while the latter tend more toward technical understatement and emotional reserve. These differences show the impact of differing models of relevance, as they guide sociologists' expectations of the long-term effects of their work. Other manifestations of such models include not only the style of organization and presentation of social inquiry but also, and perhaps even more important, the selection of research problems and strategies. But though the influence of these models may be profound, sociologists have rarely subjected them to critical examination.

In these pages I aim to identify and analyze several models of relevance which have played an influential role in American sociology. I mean to take such models seriously as interrelated hypotheses in their own right about the workings of social systems. For models of relevance must deal with such issues as the relations between beliefs and action, the conditions of stability and change in social systems, the interactions between abstract ideas and material interest, and the role of power relations in shaping patterns of social change. All of these are eminently sociological questions. It would be surprising if the assumptions embodied in sociologists' models of relevance did not fit with their thinking on other matters.

Superficial consideration of these issues has often led to gross underestimation of their complexity. For from a strictly technical perspective, the usefulness of social insight is easily established. Any manager aiming to raise a firm's production will profit from the greatest possible understanding of the firm and its employees. Any would-be revolutionary, by the same token, will profit from the most thorough understanding of the institutions to be subverted. In calculations like these, the insights of social science facilitate social manipulation just as natural science makes possible the work of the engineer.

This realization has led some thinkers to regard social science insight as a sort of universal solvent for the stickiest social problems. George Lundberg (1947, pp. 64–65) epitomizes this simplistic view in his book *Can Science Save Us?* The answer to the question in the title was of course a ringing affirmative. He wrote:

> Why is it that we find ourselves with thousands of research laboratories devoted to the application of the scientific method to physical problems, and almost none, comparatively speaking, devoted to research in human relations? Is it because human relations are functioning so smoothly that they need no attention? A glance at industry, our minority problems, and the international situation should be sufficient answer. Is it because, while physical research pays a tangible dividend in profits, improvement in human relations does not? Ask any employer what labor troubles, strikes, lockouts, slowdowns, and loafing is [*sic*] costing him, and in the end, costing all of us. Then why don't we risk a few million dollars in scientific re-

> search on human relations the way we risk hundreds of millions on industrial research . . . ?

The superficiality of Lundberg's assumptions is only too apparent. He simply ignores the fundamental fact that the conditions which he wishes to see improved, like most situations which sociologists and others might regard as ripe for improvement, entail conflicts of interest.[3] "Labor troubles, strikes, lockouts, slowdowns, and loafing" are troublesome precisely because they entail incompatible attempts to gain advantage. It may be easy to imagine how added insight might help either one contestant or another in these situations. But it is much more problematic to develop a formula for "rational solutions" to these difficulties which all concerned would acknowledge as such.

More generally, it is nearly always easier to assess the social effects of specific items of insight in specific situations than it is to judge the effects of whole categories of inquiry upon wide areas of social life. An industrial sociologist may determine, for example, that working conditions in a particular plant lead to greatly increased mortality among its workers. Disclosure of the finding may be certain to provide advantage for workers or to place management on the defensive. The sociologist may assess such an outcome as satisfactory or not, depending upon his or her values or interests. But much more complex sociological judgments are required to assess the long-term social effects of studying occupational safety or industrial relations in general. And models of relevance demand precisely these kinds of assessments.

The following discussion deals with five models of relevance. The aim is to make the models explicit and to evaluate them as sociological hypotheses in their own right. To be sure, we are some way from being able to test such models against specific data. But it is by no means impossible to assess their internal consistency and to weigh their plausibility as assertions about the working of the social world. This will mean posing the following questions about each: (1) Does the model embody a coherent idea of what beneficial results for society at large it seeks to promote? Is this version one which other students of society might share? (I do not mean to ask here whether the underlying notion of social good is metaphysically sound, but simply whether it represents a plausible and workable basis for consensus among researchers.) (2) What intellectual products are supposed to bring about such results? What form of sociological research, in other words, is to have the beneficial effects? (3) What category of social actors is seen as the "consumers" of the relevant insight? Whose behavior, in

[3] I have developed this observation at greater length in an earlier study, "The Problem with Social Problems" (Rule 1971).

other words, is supposed to be shaped by the insight in question? (4) Is it reasonable to expect the actions inspired by such insight to take the desired form? Do these actions have a chance of attaining their intended ends?

The five models treated below are distinguished first as ideal types. Then passages from the writings of influential sociologists are presented to illustrate the actual manifestations of such types and the strengths and weaknesses which they entail. As with all ideal types, none of the five is embodied in pure form in any empirical instance. Each writer departs from or embellishes the ideal type at some point in his writings. Careful and coherent exposition of any model of relevance is rare, and many statements discussed below are fragmentary assertions of principles which may not be followed faithfully in practice. Still, the examples given here should illustrate how models of relevance shape expectations concerning sociological inquiry and should demonstrate the difficulties inherent in such expectations.

Nor do these five ideal types represent all the models of relevance which are either logically possible or empirically influential. These discussions confine themselves to models which follow the Weberian assumption that empirical analyses cannot themselves yield prescriptions of ultimate value. Thus I do not deal here with the doctrines of the Frankfurt school, whose studies of social antagonisms aim at revising the values of the antagonists. Those models entail serious difficulties in their own right, I feel, but very different ones from those of the models discussed here. While the models discussed below are hardly exhaustive, however, they are probably the most influential ones in the development of American sociology.

MODEL 1: NO NET EFFECTS

Perhaps the simplest model of relevance is one which posits no net input at all of sociological insight to any greater social good. For anyone espousing such a model, the inherent rewards of enhanced understanding would be the only possible justification for the efforts of the sociologist. I suspect that this model is especially unlikely to find defenders in anything like its pure form, at least among working sociologists. A few social scientists, however, have espoused variants of the doctrine.

For obvious reasons, those who despair of the possibility of social betterment through social insight do not develop any very clear concept of what such betterment would entail. Writers in this vein do, however, provide various accounts of the social conditions which preclude such change. One line of argument to this effect is to view all important social outcomes as governed by forces too powerful to be influenced by human ideas. A good example of such an argument comes from William Graham Sumner (1934,

1:105):

> It is at any rate a tough old world. It has taken its trend and curvature and all its twists and tangles from a long course of formation. All its wry and crooked gnarls and knobs are therefore stiff and stubborn. If we puny men by our arts can do anything at all to straighten them, it will only be by modifying the tendencies of some of the forces at work, so that, after a sufficient time, their action may be changed a little and slowly the lines of movement may be modified. . . . In the meantime spontaneous forces will be at work, compared with which our efforts are like those of a man trying to deflect a river, and these forces will have changed the whole problem before our interferences have time to make themselves felt. . . . Everyone of us is a child of his age and cannot get out of it. He is in the stream and is swept along with it. All his sciences and philosophy come to him out of it. Therefore the tide will not be changed by us. It will swallow up both us and our experiments. It will absorb the efforts at change and take them into itself as new but trivial components, and the great movement of tradition and work will go on unchanged by our fads and schemes.

Pareto and Spencer, among the other early sociologists, also sometimes took positions like Sumner's; perhaps they, like he, found this argument an opportune way of emphasizing the importance of the causal forces which made up their newly designated subject matter.

Not many present-day social scientists argue that their efforts are indifferent in their overall effects on social well-being. One partial exception is furnished by Edward Banfield and James Q. Wilson, who propose a subtler argument in favor of this model in their well-known book *City Politics* (1963, p. 3):

> The reason why knowledge about politics (whether in the form of general propositions or as practical wisdom) will not lead to better solutions of social problems is that the impediments to such solutions are a result of disagreement, not lack of knowledge. Knowing how disagreements arise, how the parties to them act *vis a vis* each other, and the rules and practices by which certain institutions mediate them is not likely to be of use either in preventing disagreements from arising or in bringing them to quicker or more satisfactory resolution. Thinking that a general increase in the level of knowledge about politics will promote better and faster solutions of social problems is something like thinking that a general increase in the skill of chess players will lead to shorter games or to a "solution of the problem of chess."

Banfield and Wilson, then, arrive at their position by taking quite seriously the issue which Lundberg ignores in his statement from *Can Science Save Us?* Their recognition of the antagonistic social interests in the "consumption" of social science insight leads them to the conclusion that increase in such insight will be self-canceling in its effects on social well-being. Unlike Sumner, Banfield and Wilson seem to believe that social science insight may in fact help people to pursue their own interests more effectively. But

the interests so promoted must necessarily be, as they see it, opposed to other interests facilitated by the same sort of insights.

These remarks by Banfield and Wilson are tantalizingly brief. The fact that they are offered only in passing leaves it to us to speculate about their exact interpretation. Probably Banfield and Wilson would not want to insist that every single insight provides precisely the same measure of advantage to every interested party—as though every fact useful for fueling insurgents' criticisms would serve just as well in the defense of entrenched interests. More likely they mean that the balance of advantage to all concerned would somehow sum to zero over the longer run.

Again, probably no social scientist would espouse the present model in its pure form, either on the Banfield-Wilson grounds or on any other. For arguing that understanding counsels skepticism in itself implies that understanding is beneficial. Skepticism, after all, promises to save us the wasted efforts which might result if we mistakenly believed that understanding could lead to active steps to betterment. A judicious limit on expectations represents a significant social benefit in its own right.

Actually all three of these writers take this position. Banfield and Wilson (1963, pp. 3–4) write: ". . . the spread of knowledge about politics may also reduce the amount of well-meant but often harmful interference by citizens in the workings of political institutions. A public which understands the nature and necessity of politics may perhaps be more willing than one that does not to allow politicians to do their work without obstruction. . . . And it may be more aware of the risks it runs of damaging, or perhaps even of destroying, a tolerable system by attempting reforms the full effects of which cannot be foreseen." In this view, the consumers of social science understanding would be a wiser general public who would know better than to run the risk of making things worse by trying ineptly to improve them. In fact, Sumner (1914, p. 37) and many other conservative thinkers have taken this position.

MODEL 2: DIRECT AND POSITIVE EFFECTS

A second obvious possibility is to attribute to social science insight a direct and positive role in enhancing social well-being. Unlike the view suggested in the first quotation from Banfield and Wilson, this approach does not picture all social interests as standing in zero-sum relation to one another. Unlike certain models discussed below, this one does not look to any one group of social actors as the main consumers of insight. Instead, this model posits areas of possible improvement in social relations where change would be to the interest of all. It is thought that social science, by showing how to realize these possibilities of a "better" social life for everyone, may lead to ameliorative change.

In its many versions, this model has probably been the most influential of any in the development of American sociology. The earliest volumes of the *American Journal of Sociology*, for example, abound with expressions of faith in the role of sociology in pointing the way to a more rational social world. These rather touching expressions, however, do not spell out in any detail how these ameliorative changes are to come about. But such accounts have become somewhat more common in the present-day social problems literature. Probably the most sophisticated and fully elaborated of these is Robert Merton's detailed theoretical statement in his *Contemporary Social Problems* (1971). Indeed, it is among the most complete and thoughtful defenses given for any model of relevance. It warrants examination here in detail.

Perhaps the first question confronting any model of relevance is how to locate those conditions considered ripe for improvement in some sociologically coherent category. For Merton, that category is "social problems," states of society which contravene commonly held standards of acceptability (1971, p. 799). But this view obviously entails all the difficulties of those doctrines lumped under the designation of "value-consensus" theories. Are not disputes over what should be regarded as acceptable, after all, at the very heart of the conflicts which often pass for social problems?

Merton seems to acknowledge the validity of these objections. He writes (1971, p. 804):

> We must . . . be prepared to find that the same social conditions and behaviors will be defined by some as a social problem and by others as an agreeable and fitting state of affairs. For the latter, indeed, the situation may begin to become a problem only when the presumed remedy is introduced by the former. What is loosely described as "socialized medicine," for example, was defined as a social remedy by Walter Reuther and many others in his constituency of the AFL-CIO just as it was defined as a social problem by the successive presidents of the AMA and many others in their constituency.

Is the designation of social problems then completely arbitrary, nothing more than a whim of the one who defines? Merton thinks not; there is, he claims, at least a core of conditions which nearly everyone holds undesirable, no matter what his political identifications. No one, in other words, is really in favor of violent crime, epidemics, or poverty. In such cases, he holds, there is a consensual, and hence independent, criterion according to which these conditions represent subjects for improvement.

But there are difficulties. Certainly there exist categories of social conditions which no one would regard as good things in themselves. But even there, the significance of an offending condition, and the details as to what would constitute its betterment, are matters of partisan difference. Probably no one really finds industrial strikes, to take Lundberg's example, a pre-

ferred mode of social activity; all concerned would be pleased to have industrial disputes settled without strikes if the settlement could be on their terms. But what people agree on is that they would prefer things to be different, not necessarily *how* they would like things to change.[4] These facts create problems for any model of relevance.

Acknowledging the difficulties of his first position, Merton turns to another, related argument. What marks social problems as a distinct sociological category is not only their contravention of shared social standards, but also their actual role in society. Consistent with his functionalist principles, Merton regards as "problematic" those conditions which fail to make their proper contribution to the working of the social whole. Cases of multiple dysfunctions represent, on purely objective grounds, social problems. The role of the sociologist is to point out such objective failures of social systems, even when they are not publicly recognized or proclaimed as social problems.

But who defines the "proper" functions of any institution? Does such a definition not once again entail representing the standards of the observer as objective sociological reality? Merton (1971, p. 820) denies this:

> When we say that a particular group or organization or community or society is disorganized in some degree, we mean that the structure of statuses and roles is not as effectively organized as it, then and there, might be. This type of statement, then, amounts to *a technical judgment about the workings of a social system*. And each case requires the sociological judge to supply competent evidence that the actual organization of social life can, under attainable conditions, be technically improved to make for the more substantial realization of collective and individual purposes.

It is of course essential for Merton to insist that such judgments are "technical"—that is, objective—since the alternative is to define them in terms of the personal preferences of the observer. But who judges the "collective purposes" of a particular social element? The issue is no clearer here than in the "commonly shared standards" argument. Is the purpose of a welfare institution to provide maximum services to the poor, or to keep the latter in a position of regimented and well-disciplined subsistence? Is the purpose of a firm to provide jobs for its employees, to aggrandize the power of its top management, or to generate profits for its stockholders?

But Merton has a defense: "A social dysfunction refers to a *designated* set of consequences of a *designated* pattern of behavior, belief, or organization that interfere with a *designated* functional requirement of a *designated* social system. . . . It must be noted . . . that the same social pattern

[4] A number of writers, including some explicit critics of Merton, have recently rejected the notion of any strictly technical or purely objective definition of social problems (see Blumer 1971; Kitsuse and Spector 1973; Ross and Staines 1972).

can be dysfunctional for some parts of a social system and functional for other parts . . ." (1971, pp. 839–40).

But if this formulation solves a conceptual problem, it at the same time reintroduces the problem of objectivity: the injunction to look for designated consequences to a designated social unit once again introduces the very relativism which Merton deplores. *Which* consequences of *which* social conditions should be assessed in defining a social problem? Featherbedding, for example, may be dysfunctional for the management of a firm but functional and indeed vital for the union organization. Where, in this case, does the social problem lie?

There should be no question of the importance of these matters in terms of the requirements of any model of relevance. Without some criterion for identifying problematic social conditions, the model lacks the coherent objective required of it. Lack of such a clear-cut goal of efforts at ameliorative change also casts into doubt the identity and role of "consumers" of social insight. For if people really would disagree in principle as to what constitutes "problems," then we cannot expect added insight to result in a coherent and harmonious program of change. We cannot expect scholars to act consistently in identifying conditions for study or in choosing insights concerning such conditions which might represent spurs to ameliorative action.

As we have seen, Merton believes that exposure of conditions which are both generally deplored and objectively dysfunctional will lead to a movement of public feeling toward eliminating the offending condition. But are not many designated social problems, including poverty, racial injustice, and the like, rediscovered periodically, without permanent steps toward their alleviation? Acknowledging the argument, Merton answers (1971, p. 807):

> Sociologists do not claim that knowledge of the consequences attendant on current social beliefs and practices will automatically lead people to abandon the beliefs and practices that frustrate their own basic values. Man-in-society is not as strictly rational a creature as all that. The sociological truth does not instantly make men free. It does not induce a sudden rupture with demonstrably dysfunctional arrangements in society. But by discovering consequences of accepted practices and by making these known, the sociologist engaged in the study of social problems provides a basis for substantial reappraisals of these practices in the long run, if not necessarily at once.

"The long run"; it has the ring of Marxist pronouncements on the ultimate effects of social class relations on the course of history. But even assuming a modicum of consensus in designating some social condition as undesirable in principle, can Merton really show that the force of opinion in these cases is likely to prevail over other forces in the situation? To do

so would be to ignore the considerable vested interests which profit from the existence of those things designated as social problems. Whether the force of "commonly shared standards" or the disapproval of "dysfunctional" elements can necessarily prevail over these seems problematic at best.

Take poverty as an example. Herbert Gans, in an article entitled "The Positive Functions of Poverty and Inequality" (1973), cites some 15 positive social benefits stemming from the existence of poverty. These range from insurance that "dirty" jobs will be done to the contribution of the poor, through their political apathy, to the stability of the American political process. Gans's arguments seem to me thoroughly persuasive. Poverty and "social disorganization," though seen as quintessential social problems, are actually a boon to important interests in American society. One wonders whether any such "undesirable" social condition could continue to exist, were this not the case.[5]

Merton's arguments, then, do not fully succeed in developing an objective standard for the existence of social problems. This difficulty in turn sets off a chain reaction of other troubles with his model of relevance. It casts into doubt, for example, the nature of the insights which might lead to social change. For if observers disagree as to what constitutes a social problem, they will hardly be able to agree as to what forms of insight cry out for change. And given such lack of consensus, it is unclear how any coherent program of action for change could arise from sociological inquiry. Different consumers of social insight would draw different and perhaps conflicting implications for action from the analyses of problematic conditions. In short, it is difficult to believe that Merton's formulations, if followed, would lead to a coherent pattern of movement toward what people would agree was enhanced social well-being. And while one cannot exclude the possibility that other arguments in favor of this model may succeed where this one fails, Merton's position appears to be the most sophisticated we have on the subject.

MODEL 3: SPECIAL CONSTITUENCY—THE PROLETARIAT

The difficulties of Merton's argument should at least be instructive. They demonstrate how hard it is to develop a program of enlightenment which promises to serve directly the interests of all elements in society. Vested

[5] Consider, for example, the case of poverty and unemployment, both of which represent classical "social problems." Economists generally concede that a drop in unemployment in the United States tends to raise the rate of inflation. Government planners thus have accepted substantial chronic unemployment, at levels higher than would be politically acceptable in other industrial countries, as a "necessary price" for the control of inflation. So, unemployment and the resulting poverty may represent social problems, but they are not so problematic as the alternative, that is, full employment.

interests in keeping problematic conditions as they are, and contests over what constitutes problems in the first place, threaten to plague any version of the previous model.

The difficulties of approaches like Merton's have led many writers to seek an altogether different strategy. Their solution is to accept as inevitable the conflicting interests in programs for social betterment, and hence in the uses of social insight. Instead of attempting to develop a form of research which promotes immediately the interests of all, they propose to generate forms of insight useful at least initially to just one category of social actors. These "special constituency" models, as I call them, are predicated on the assumption that this one category of consumers of social insight has the power to act effectively in the longer-run interest of all of society. No doubt the most famous proponent of such an approach is Karl Marx. His program of social inquiry was strictly oriented to the proletariat, the consumers who, he felt, could make best use of any insight in the long run.

Marx—like most students of society, but more openly and candidly than most—saw his work as designed to bring about a better world. He considered it his mission to extend the realm of rational understanding over the workings of the social world. At the same time, he explicitly identified himself as a partisan of a specific set of embattled interests. Evidently he felt no conflict between these two positions. He based his research on openly available sources, and published his findings and conclusions for anyone to read. Yet he assumed that his labors would have their beneficial social effects through the actions of only one element of the social structure. No one ever appreciated more keenly the fact that social problems stemmed from oppositions of interests among different elements of society; no one would have more quickly discounted the possibility of providing insight which would work for the immediate benefit of all groups. And yet, crucially, Marx felt that the social groupings favored by his inquiries in the short run would reap from them an advantage which would in the long run work to the best interests of all humanity.

What form of social betterment does this model of relevance aim at? Marx's answer was a particularly rich one. In the very long view, his vision of the good society was one of a world of bounty, free of the coercion of the capitalist state and the irrationalities of the capitalist economy. But the immediate goal, of course, was a revolutionary overthrow of the capitalist system which embodied these irrationalities. It would be difficult to underestimate the significance of this combination of assumptions for Marx's model of relevance. For it made it reasonable to assume that a better, more beautiful, less conflict-ridden world in the longer run required action to produce more partisanship and strife in the shorter run. As Marx

put it in *The Communist Manifesto* (see Feuer 1959, p. 29),

> If the proletariat during its contest with the bourgeoisie is compelled, by the force of circumstances, to organize itself as a class, if, by means of a revolution, it makes itself the ruling class and, as such, sweeps away by force the old conditions of production, then it will, along with these conditions, have swept away the conditions for the existence of class antagonisms and of classes generally, and will thereby have abolished its own supremacy as a class.
>
> In place of the old bourgeois society, with its classes and class antagonisms, we shall have an association in which the free development of each is the condition for the free development of all.

Marx believed, as we all know, that insight as to the true workings of capitalism could aid the proletariat to hasten the break with the old system. Such insights represented the essential intellectual "products" in his model of relevance. Marx felt that his critiques of the capitalist system served only the interests of the proletariat, that his ideas were incapable of being turned to the use of antagonistic social groups. True, members of other classes, including intellectuals, students, dispossessed capitalists, and others, might identify themselves with the proletariat and thus become part of Marx's special constituency. But Marx held that the proletariat as a class represented the only historical force with the ability necessary to make significant changes. And only it could profit from insight into the ultimate defects of capitalism.

One implication of these views was the rejection of all moderate social reform measures, such as attacks on discrete social problems. For Marx, the problems of capitalism were of a piece; reformist measures would tend to delay the death of the terminal patient instead of hastening the birth of a newer, better system. This conclusion, like those on the consumption of Marxist insight, obviously depends for its validity on Marx's historical prophecies about the future of capitalism.

Do Marx's statements provide a coherent view of the state of social betterment to which his model of relevance is attuned? Would others share this view? It would be difficult to fault the ultimate vision of a world without want or coercion. But the short-term elements of Marx's scenario of social improvement, exacerbation of the conflicts of capitalism, leading to sharp break with the political and economic system are obviously much more controversial. Whether such a sharp break is essential to any meaningful improvement in social conditions continues to stand as a point of contention between Marxists and their more moderate colleagues in social science.

One reason for skepticism on this point is the failure of key Marxist prophecies on the future of capitalism. It is now a truism that advanced capitalist societies have become, contrary to Marx's expectation, less po-

larized rather than more so. The bulk of the populace in these societies both better off materially and more effectively integrated into political li than in Marx's time. These changes make it difficult to maintain that tl only really significant improvement in capitalist societies can come abo through their destruction. One may of course hold that destruction capitalism is preferable to its perpetuation, but that is a different matte

Second, it is far from clear that political action inspired by Marxi precept has always contributed to social betterment or to the alleviatio of human suffering. Surely even the most sympathetic Marxist observer communist and socialist regimes would want to concede that Marxist do trine, as interpreted by the leaders of these states, has at least sometim led to very serious human costs. And it goes without saying that no oi has as yet seen anything like a radical break with the irrationalities whi Marx attributed to capitalism—political repression, economic scarcit oppressive state bureaucracy—in any Marxist regime.

Finally, part of the inadequacy of Marx's model of relevance has to with the patterns of consumption of insight which he posits. It is by means clear that the industrial proletariat has represented the main coi sumers of Marxian insight and analysis. In fact, intellectuals, peasant students, and other groups have often figured much more prominently th industrial workers in this connection. Nor can one reject the possibili that resourceful elite groups may be able to use Marxian insight to furth their own decidedly nonrevolutionary interests. The very proliferation Marxist thinking—its currency, in differing versions, in so many differe contexts—belies any certainty about its overall effects on social bettermen

It would be easy to mistake these reservations about Marx's model relevance for an attack against Marxist analysis in general. That is def nitely not my intent. One can accept all of these misgivings and still reta a belief in many of the key Marxist insights. What I criticize here is n so much the main sociological content of Marx's ideas as his finely tune assumptions relating social analysis to improvement of social condition For these assumptions are so precise and so closely related to one anoth that change in any one is apt to leave in doubt Marx's assurance of th historical results of his own social analysis.

MODEL 4: SPECIAL CONSTITUENCY—THE UNCO-OPTED

Difficulties with the Marxist model of relevance, particularly those arisin from historical events since Marx's time, have not been lost upon lef oriented thinkers. Even those sympathetic to the working class may n now find this special constituency the most promising consumer of insigh for social innovation. Perhaps as a result, a genre of research has arise which is informed by a somewhat different, though closely related mode

What distinguishes this model is its orientation to a category of consumers identified as those uncompromised by allegiance to or manipulation by entrenched power. Adherents to the various versions of this model can clearly trace their intellectual lineage to Mannheim. But Mannheim was much more explicit in identifying his special constituency of unattached intellectuals than more recent exponents of this model have been in identifying theirs.

This model seems to have informed most of the critical sociological studies of American society in the 1960s and 1970s. These studies picture America as dominated by certain selfish established interests, interests which most of the time succeed in squelching protest and innovation from below. The rather diffuse grouping at the bottom of the social heap—including possibly the working class, but also the poor, the deviant, racial minorities, and the alienated more generally—represents the special constituency of the various versions of this model. Members of these groups or their representatives, it is supposed, will respond to critical insights on the true workings of American society by coalescing their latent strength and seeking change. Such action, the model assumes, will represent overall betterment of social conditions.

No spokesman for this view seems to have given as detailed an account of it as that given by Merton for his model. One must thus piece together the underlying assumptions from briefer remarks made in passing. But the model seems more than merely implicit in statements like the following one by Gary Marx in the Introduction of his *Muckraking Sociology* (1972, pp. 4–5):

> . . . I wish to argue that an important and little acknowledged potential for change lies in the educative role it [social science] can play in raising public issues. Such data and analysis can give us a clearer picture of our world, stripped of protective verbiage and without the usual selective perceptions (and misperceptions). If this picture involves a striking contrast between values and practices, it can be politically useful to those seeking change . . . social science can help articulate the value claims and suffering of ignored and powerless groups. . . . The social researcher can of course also be instrumental to change by studying sources of resistance and how advocates of change can be mobilized, by designing and helping implement new programs and conceptions of society and by evaluating the consequences of innovations.

The differences between the various versions of this model and the positions of Merton, on the one hand, or of Karl Marx, on the other, are nuanced and significant. Like Karl Marx's model, this one envisages the relevant intellectual products as critical insights as to the true, rather than the ostensible, workings of society. Like Merton, on the other hand, Gary Marx and other writers in this genre stress the dramatization of discrep-

ancies "between values and practices." But adherents to the present model lack the Marxist faith in the ultimate historical triumph of their special constituency, while they also lack Merton's theoretical assurance that the conditions which they find unacceptable will meet with nearly universal disapproval.

This latter difference manifests itself in the portrayal of themselves by these writers not as technical, objective commentators, a la Merton, but rather as engaged activists who do not hesitate to commit themselves in favor of those groups and values which they favor. A good example comes from the statement of Jerome Skolnick and Elliott Currie (1970, p. 13), who define their position in self-conscious contrast to Merton's: "Social scientists, then, study social problems from the vantage point of committed people striving to make sense of the society in which they find themselves. There is nothing wrong with that, but it should not be mislabeled 'disinterested' inquiry."

Statements like this point to an important difficulty in this model of relevance. By rejecting the Mertonian role of dispassionate interpreter of society's own standards, writers like Skolnick and Currie gain in dramaturgy but lose in the theoretical coherence of their model of relevance. The preceding statement suggests that the sociologist's values or interests may not be the same as those of the larger society, that they may be "partisan" sensitivities on behalf of embattled groups. But this assumption provides no assurance whatsoever that the sociologist's insights into the circumstances of oppressed groups will be sympathetically received by most people. The sociologists may illuminate the suffering caused, say, to homosexuals by the attitudes and practices of the "straight" world, only to find that most straight people regard such oppression as no more than fitting. The researcher may of course resolve to tell the story just the same, to throw in his or her lot with the oppressed whether the world sympathizes or not. This possibility is always latent in the role of the engaged sociologist. But it fits badly into a model of relevance, which requires some account of how insight is likely to succeed in making things better.

A few pages after the passage cited above, Skolnick and Currie portray the desired role of the sociological investigator as that of holding those in "positions of authority, power and influence accountable for their actions" (1970, p. 16). Such a view has much in common with that of C. Wright Mills, who developed the most influential arguments for this model of relevance (e.g., 1959, pp. 185–86). For Merton, such a position would be consistent; it implies that sociological insight should foster confrontation by the public at large of conditions which offend its own standards. If Skolnick and Currie or Mills were to give this interpretation to their approaches, their positions would be virtually indistinguishable from that of Merton. But though they are not explicit, I do not believe this to be the

intent of Skolnick and Currie. And Mills certainly never offers such consensual arguments. Yet without some assumption of general support for their critical judgments, these three authors seem to lack grounds for faith in the long-term effects of their efforts. By addressing their insights to embattled groups at the bottom of the heap of social privilege, they may solve a moral problem of not wishing to identify with the standards of the establishment. But their intent to serve the weak then risks becoming unconvincing simply because of the weakness of those they would serve.

Mills makes himself especially vulnerable to such objections when he writes, "There is no necessity for working social scientists to allow the political meaning of their work to be shaped by the 'accidents' of its setting, or its use to be determined by the purposes of other men. It is quite within their powers to discuss its meanings and decide upon its uses as matters of their own policy" (1959, p. 177).

Given his combative political stance, Mills really must take a position like this one. And yet, within the framework of his own analysis of American society—as embodied, for example, in *The Power Elite* (1956)—it is difficult to find grounds for such confidence in the effects of his insights. For one must finally ask to whom Mills is addressing his critical analyses. Not, one supposes, to those at the top levels of the power hierarchy, since they are presumably so implicated in the rotten order that they will not heed; nor, one imagines, to the middle levels, which Mills describes as a "drifting set of stalemated, balancing forces" (1956, p. 324). And still less, one supposes, to those at the bottom of the heap, who represent a "politically fragmented . . . increasingly powerless . . . mass society" (1956, p. 324). But what audience is left? If publics, in Mills's sense, have really been extinguished by the growth of mass society, there is really little hope of a responsive audience anywhere.

Placing hopes for consumption of "relevant" social insight on a special constituency of unco-opted actors, then, solves some problems but raises others. It forestalls the necessity of defending the complicated historical predictions which burden Karl Marx's model of relevance. It obviates the need to rest one's hopes for change on the sensibilities of the majority or upon established institutions—both of which may seem responsible for the unsatisfactory state of things in the present. But the result is the lack of any special rationale indicating how the unco-opted consumers of insight may be able to assert their interests effectively. Karl Marx could rely on the historical growth of proletarian power; Merton could put his faith in an appeal to the normative standards and interests of society as a whole. But this model has neither historical predictions nor appeal to a universal standard to guarantee its results. The account of why the insights addressed to this special constituency should necessarily prevail in action has yet to be written.

MODEL 5: SPECIAL CONSTITUENCY—GOVERNMENT OFFICIALDOM

The final model of relevance to be considered here is that oriented to agencies of the state as consumers of sociological insight. One might view this model as the mirror image of the previous one. Instead of seeing those most disaffiliated from power and institutional connections as the most promising vehicle of enlightened understanding, this model identifies established power holders as most likely to use social science insight in the interests of society as a whole.

No doubt this is one of the oldest models of relevance; it has had especially many variants. The 1960s brought a resurgence of interest in this view—precisely at the moment when other enthusiasts began to take special interest in the previous model. As one might expect, however, exponents of the two models have not generally been friendly to each other's programs. Consider, for example, the following statement from Daniel Patrick Moynihan (1965, pp. 15–16):

> All in all, the prospect is for a still wider expansion of knowledge available to governments as to how people behave. This will be accompanied by further improvement of the now well-developed techniques of determining what they think. Public opinion polls are already a daily instrument of government decision-making. . . . The day when mile-long petitions and mass rallies were required to persuade a government that a popular demand existed that things be done differently is clearly drawing to a close. Indeed, the very existence of such petitions and rallies may in time become a sign that what is being demanded is *not yet* a popular demand.

The message is clear: Those in power, aided by the proper social science techniques, can determine the needs of society more efficiently than ever. More important, they can be counted on to meet those needs without unauthorized dissidence from below.

This model at least avoids the implausibility of placing faith in the actions of special constituencies which are inherently weak. Unlike the proletariat or unaffiliated actors, the government represents a special constituency which ought to have the power to act on social science insight if any group can.[6]

On the other hand, one may well wonder about the interest of government bodies in social science insight. The modern state, after all, represents a complex and often contradictory bundle of potentials for action. Which of these potentials is social science insight most likely to serve? Are its most important uses in the hands of state actors likely to represent steps which would widely be regarded as in the interest of "society as a whole"? Or is the state more likely to mobilize such insight on behalf

[6] However, some writers have taken the position that social problems are inherently so complex as to defy even the full application of state power. Moynihan himself has adopted a version of this position in his more recent writings (1973, p. 278).

of partisan or exploitative interests? Obviously, these questions raise all the long-standing debates over the nature and function of the modern state.

Again, we have few explicit attempts to deal with the complexities of this model of relevance. But most variants of the model seem to deal with these questions by recourse to a modified version of an approach like Merton's. Consider the words of the influential report on the public role of social science published by the National Science Foundation (NSF) (1969, pp. 3–4):

> The commission believes that among the American people there are a number of widely shared national and community objectives, and that social scientists can contribute to their achievement. Interethnic and interracial conflict; alienation of much of America's youth from society; multiple problems of man living in urban settings; various pollutions of man's environment; crime; mental illness rates; problems of alcoholic and narcotic addiction; problems of the educational process in the nation's schools —these are among the most visible and publicized concerns. . . .
>
> At present, the social sciences, like the humanities, are a relatively unused national resource. Much of America's effort to achieve its aims makes little attempt to use this fundamental resource. The Commission asserts its doubt that this country can successfully solve its challenging and diverse social problems unless it draws upon the increasing capabilities of the social science community.

The closeness to Merton's position hardly requires emphasis. The NSF authors believe that the problems addressed by government action are everybody's problems, problems of the whole of society. These writers seem to differ from Merton only in focusing their hopes for enlightened action specifically on agencies of the state.[7]

And certainly this position suffers from the difficulties of Merton's. For the assumption that the state stands fully and unambivalently committed to the eradication of offending social conditions is dubious at best. Conditions like those cited in the NSF statement—racial injustice, pollution, and alienation, for example—represent results of government action as much as they represent situations which the government is committed to eradicating. To be sure, no one would regard such conditions as desirable in their own right. But the alienation of youth noted by the NSF writers stemmed largely from the Vietnam War and other harsh government policies. Those at the centers of power no doubt regarded these results as undesirable side effects of the war, but nevertheless worth the political price. Environmental pollution is likewise deplorable in itself, but it may often prove more acceptable than the political costs of effective state action against the polluters.

[7] While Merton by no means rules out state agencies as consumers of insight into social problems, the implication of his statements seems to be that any and all elements of society may potentially be able to act on such insight.

To oversimplify only slightly: This model can be persuasive only on th assumption that state agencies stand ready to act "in the interest of sc ciety as a whole" and are prevented from doing so only by ignoranc Were this the case, the statements by Moynihan and the NSF writer would be justified. But as a blanket principle, such an affirmation implie an unacceptably optimistic statement of the government's willingness t do the "right" thing.

CONCLUSION

The past 10 years have seen a remarkable development among America sociologists of reflective concern with the workings of sociological inquiry Some expressions of this concern have come in the form of semihistorica accounts of the unfolding of sociological thought. Perhaps the best know examples in this genre are Gouldner's *The Coming Crisis of Western Soci ology* (1970) and Friedrichs's *A Sociology of Sociology* (1970). Othe manifestations of the same concern have been quantitative studies of cita tions and other recognition of published work—for example, S. Cole (1978) A major theme in all of these writings has been "paradigms," "domai assumptions," and other immanent principles in the organization of socio logical inquiry.

Clearly models of relevance represent yet another such principle, on with its own force in shaping sociological theory and research. The pre ceding discussion should demonstrate just how far ranging the influenc of these models is. Yet the remarkable thing is how little developed mos versions of these ideas are as coherent sociological accounts. All of th models examined above have proved to embody some very serious am biguities or implausibilities.

These deficiencies, however, are instructive. In every case, the most seri ous difficulty seemed to arise in the attempt to take account of the con flicting interests which surround those social conditions deemed ripe fo improvement—and which therefore bear upon the insight that might lead to improvement. To oversimplify slightly: accounts of how social arrange ments could be "better" do not necessarily provide grounds for reshaping the concrete interests and prerogatives of those who oppose such change.

For anyone who has entertained hopes that sociological inquiry will somehow speak to the needs of society as a whole, these ambiguities are unpleasant to confront. I suspect that most working social scientists have entertained such hopes, at least at some point. And I know of virtually no attempt to assess these ambiguities and contradictions directly without attempting to explain them away. The unique exception is Barrington Moore's recent work, *Reflections on the Causes of Human Misery*, sub-

titled *and upon Certain Proposals to Eliminate Them* (1972). The ability to confront painful uncertainties without flinching is the outstanding feature of this book.

Early in the book, for example, he explicitly rejects the idea that the social analyst can, as Mills would have it, remain in control of the social effects of his writings. Moore remarks, ". . . one has to let the political chips fall where they will in serious social analysis. There is no innate guarantee that valid social analysis will always yield conclusions favorable to the humanitarian impulse" (1972, p. 9). Then, most significantly, Moore adds in a footnote to this remark (1972, p. 9): "I used to think that there might be some general drift in that direction because the dominant groups in any society are generally the ones who have more to hide about the way the society works. Hence critical exposure would generally favor the Left. Further investigation into the character of lower-class oppositional and revolutionary movements has made me more aware that they too have a great deal to hide (and to exaggerate) for their own political reasons. On that account I have become skeptical of any such drift."

This is a telling observation. Here Moore rejects the notion that any model of relevance can provide assurance of the ameliorative effects of social inquiry. In honestly confronting this ambiguity, which Moore surely does not regard with equanimity, he parts company with Merton, Mills, and a wide variety of others.

Models of relevance will continue to have their influence on sociological practice. Appreciation that the social effects of sociology are less than certain should not deter people from embracing models of relevance as matters of faith, if not as received wisdom. But the bases of choice even among uncertain alternatives need not be wholly irrational. Careful analysis of competing models of relevance should at least help distinguish between more and less promising possibilities. Thus we ought to entertain such questions as: How are the results of different programs of social investigation likely to serve major interests in society, in foreseeable social circumstances? How is the consumption of sociological insight likely to be shaped by its usefulness to various social contestants in ongoing social conflicts? Are there limited areas in which sociological understanding might be useful to all or nearly all elements of society, and if so what sorts of social insight would be most useful here? These questions may be extremely difficult, but they are at least empirical, sociological questions, ones which we ought to be able to attack if anyone can. And they have not received the attention which they demand.

If I were more certain of any one model, perhaps I could close with an expression of confidence about the ultimate social benefit which might stem from these remarks. But perhaps it would be more appropriate to cite

the epigram chosen by Barrington Moore for his book on the causes of human misery—from Joan Robinson's *Freedom and Necessity:* "Anyone who writes a book, however gloomy its message may be, is necessarily an optimist. If the pessimists really believed what they were saying there would be no point in saying it."

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Changing Public Policy: The Impact of Public Opinion, Antiwar Demonstrations, and War Costs on Senate Voting on Vietnam War Motions[1]

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A new approach to the study of legislative change enables us to deal directly and quantitatively with questions about how long-term changes in public policy come about. The approach is applied to the aggregate change of mind by the U.S. Senate as it moved from support of the Vietnam war to opposition from 1964 to 1973. Substantively, cumulative war costs, public opinion, and antiwar demonstrations all had significant effects on Senate roll call outcomes, but they were so highly intercorrelated that their separate effects could not be disentangled. In addition, demonstrations taking place in the months before a vote had a slight positive impact on the number of dovish votes received by motions. The 1970 invasion of Cambodia seems to have led to a significant turning point in the way the Senate dealt with the war. The general strengths and weaknesses of the new approach are assessed. It opens a new area to statistical inquiry and generates a number of novel questions that should lead to additional research.

n this paper, we present a new approach to the study of legislative change. This approach enables us for the first time to deal directly and quantitatively with questions about how long-term changes in public policy—as lefined in legislation—come about. To demonstrate the utility of the approach for dealing with issues of genuine importance, we have chosen to ipply it to the Senate's voting on the Vietnam war.

THE STUDY OF LEGISLATIVE CHANGE

The Problem

Like many other issues, this was one on which Congress voted many times over a period of years; some motions were passed, others were rejected, and he legislation on the books at the end of the period was substantially dif-

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ferent from what it had been at the beginning. What happened in between, and why?

Anyone interested in democratic politics would like precise answers to many questions about legislative change, for it is a key link in the democratic process—the point at which political preferences are (or are not) translated into new policy directions for a society. What is the impact of public opinion, the actions of interest groups and elected officials, and significant events? Does the importance of such factors vary over time? Why do some motions pass and others fail in a given session, and why do some fail one year only to be passed later? Answers to these and related questions would greatly increase our understanding of democratic political change, but unfortunately they are not now available.

We claim that this is the case because no one has previously developed a satisfactory way to study statistically the determinants of roll call outcomes over time. Scholars have made noteworthy progress in studying public policy, political change, and legislatures, but the following review of the literature will show that none of the advances really allows us to confront directly a central question: What leads to changes in legislative outcomes over time?

Past Work

Even the two bodies of work most relevant here—first, work on how political "inputs" are transformed into "outputs" and, second, studies of legislatures—do not deal directly with legislative change.

1. Of central importance is the literature which attempts to show how inputs into the political process—social, economic, and political characteristics of states or nations, party activities, etc.—are related to political outputs, including government adoption of policies, expenditure levels, and support for various programs. Such work has greatly contributed to our understanding of how such characteristics of political units influence public policy, and controversies generated by the work have provided a substantial research agenda (see, e.g., Dye 1966; Fry and Winters 1970; Wilensky 1975; Silberman and Durden 1976).[2] But although this body of work is important and helpful, it has three weaknesses which make it less than satisfactory for dealing with legislative change.

First, the statistical work on the correlates of public policy does not

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[2] Works dealing with the factors that determine aggregate election results can be considered part of the same input-output paradigm (see, for example, Kramer 1971; Tufte 1975; Bloom and Price 1975; see also Nordhaus 1975).

deal with the alternatives considered by the legislature. Typically, some consequences of legislation—most often expenditures—are studied; sometimes legislative provisions are studied as well. But almost never studied is the process by which the legislature, faced with many alternative proposals, accepts some while rejecting others.

Second, most available work on policy outputs is based on cross-sectional data rather than time-series data. The work is thus inherently static in design, and conclusions drawn from it must be treated very cautiously when trying to draw inferences about political change over time (see Hofferbert 1974, chap. 6, for a discussion). Several recent articles show, in fact, that the substantive conclusions that can be drawn from time-series analysis will often be different from those drawn from cross-sectional analysis (see Erikson 1971; Gray 1976; Winters 1976).

Third, as many have noted, past works have often employed unsatisfactory measures of public policy. Most often, policy outputs are measured in terms of expenditures (e.g., Dye 1966; Jacob and Vines 1971; Hofferbert and Sharkansky 1971), although there are a significant number of exceptions (e.g., Dye 1969; Walker 1969; Ginsberg 1976). Prominent scholars in the area have long been aware of the problems involved in using expenditure data, however (e.g., Hofferbert 1974, chap. 6): for example, many types of legislation involve primarily regulative activities and are only poorly gauged by expenditure data. In addition, one recent article shows that substantive conclusions about the determinants of public policy can be very different if nonexpenditure data rather than expenditure data are used (LeMay 1973).

2. Similarly, past work on Congress is both important and helpful here, but it has several deficiencies for those concerned with how legislation changes over time. A great deal of work, for example, focuses on the determinants of voting by individual legislators, assessing the relative importance of constituency, party, the president, committee chairmen, and so on (e.g., Turner 1970; Kingdon 1973; Clausen 1973; Jackson 1974; Froman 1963; Matthews and Stimson 1975; cf. Mayhew 1974; Fiorina 1974). Unfortunately, since the emphasis in past work has been on predicting the behavior of individuals—nearly always at one point or over a short period—the work bears only indirectly on change in (aggregate) roll call outcomes over time. This is especially true when substantial changes in personnel must be taken into account.

Also common are case studies of the passage of particular bills and types of bills (e.g., Bauer, Pool, and Dexter 1963; Berman 1962; Nadel 1971). Case studies can be informative with regard to the passage of particular items and can point to factors generally important in the passage of legislation. But they tend to focus on the specifics of particular bills rather than

developing general analytic approaches which would be useful in studying enough motions simultaneously to determine the causes of long-term legislative change. In addition, there is little attempt in such studies to quantify the relationships among the variables, so conclusions are imprecise and not easily translated into more formal models (on these points, see Jackman 1975, chap. 1; and Hofferbert 1974, chap. 4).[3]

Thus past statistical work on policy does not deal with alternatives considered by the legislature, does not consider changes in policy over time, and has not developed satisfactory measures of policy. Past work on Congress has tended to focus on the cross-sectional analysis of individual behavior. What is needed, and what we attempt to provide in this paper, is an approach that deals with the collective voting of a legislative body over time, using satisfactory quantitative measures of policy.

DESIGN

If we want to explain why a legislative body votes the way it does on a series of roll calls on an issue, we must be clear about what we are explaining—the dependent variable—and about the independent variables and their effects. In attempting to specify the dependent variable, however, we immediately confront a problem no one has satisfactorily handled to date. If we want to deal with legislative outcomes, we need to consider both the content of a bill and the number of votes it receives. The problem is that these two factors—content and number of votes—are generally intertwined in our intuitive way of thinking about legislation. If someone were to say, "The Senate is more dovish this year than last year," that might mean that equally dovish motions were getting more votes or that increasingly dovish bills were getting the same number of votes, or both. To analyze legislative change, however, content and number of votes must be treated separately.

In principle, our solution is simple, though perhaps counterintuitive: the dependent variable will be the number of votes motions receive on a given

[3] Two other bodies of work on Congress will come to mind but are not directly relevant. Studies of the internal operation of Congress are numerous but will be ignored for the most part. The approach to be developed is most closely tied to the statistical work on public policy, which deals relatively little with the details of legislative activity. It is not claimed that the internal operations of Congress are unimportant; it is just that the approach will focus on the links between certain variables, without considering all the intervening mechanisms. The literature on roll call analysis is voluminous but not helpful for understanding legislative change. One of the leading roll call analysts, Aage Clausen, implies that the dimensions of voting discovered in such analyses are likely to be of little use in studying changes in policy (Clausen and Cheney 1970, p. 139; on related points, see Leik and Matthews 1968).

side of an issue—in this paper, the number of dovish votes.[4] The content of the motions will be among the variables predicting the number of votes. It certainly seems reasonable to suggest that the number of votes a motion gets depends to some extent upon the content of the motions; legislators respond to content when deciding how to vote, as they respond to other stimuli.

This focus on aggregate roll call outcomes and their determinants over time represents a major reconceptualization of the study of voting in legislatures. The utility of the design is best shown in applying it to a specific issue.

ENDING THE VIETNAM WAR: SOME HYPOTHESES

The Vietnam war was an extremely important and divisive issue in recent American political history. Although a list of the causes of the end of American involvement could be quite long, the various factors were important only insofar as they affected the decisions of Congress and the president: American involvement ended after Congress "changed its mind" and cut off all funding for the war. The Senate played a particularly important role in ending the war (Bernstein and Anthony 1974; Kanter 1972; Laurance 1976; cf. Hinckley 1971, pp. 138–48), and it is the Senate's change of mind, from almost unanimous support to strong opposition, that is the focus of this paper.

Our basic idea is extremely simple: we assume that, in the aggregate, members of Congress are rational political actors who often calculate costs and benefits in simple ways. Specifically, we predict that Congress will turn against past policies as the costs of the policies rise without compensating benefits and that members of Congress will respond to public opinion. We are not sure how they will respond to protest demonstrations. By taking these few factors into account, we expect to explain a fairly high proportion of the variance in Senate voting on the Vietnam issue.[5]

[4] The reduction of voting to a single pro-anti dimension may seem like an oversimplification but is a fair representation of reality: senators are actually faced with complex choices reduced to yea-nay-abstain on given roll calls, but it is nevertheless fairly clear on which side of a conflict a given choice falls.

[5] Given the importance of the Vietnam war issue, one would think that we would know with some accuracy what caused the Senate to change its mind and turn against the war. But we do not. Very little quantitative work has been done on the end of the Vietnam war, and most of what exists concentrates on public opinion or the interplay of personalities. See Page and Brody (1972); Verba et al. (1967); Converse and Schuman (1970); Hahn (1970); Brody and Verba (1972); Verba and Brody (1970). There are two attempts to predict aggregate public opinion from the war situation, but they do not deal with congressional voting; see Milstein (1973); Mueller (1973, chap. 3); cf. Sullivan and O'Connor (1972). One study deals with Senate voting but does not deal

Public Opinion

Given the amount of discussion devoted to public opinion and representative government, we know very little about the relationship between public opinion and legislative change. Relatively few works empirically relate public opinion to legislative activity on specific issues, and even fewer attempt to gauge precisely the impact of public opinion on changes in legislation over time. The only firm conclusion one can draw from the literature is that sometimes legislatures are responsive to public opinion and sometimes they are not.[6]

Thus, with regard to Vietnam, we have less than we would like to draw on in formulating hypotheses about the relationship between public opinion on the war and Senate voting. Congress can be expected to be responsive to public opinion in general, given the assumption that legislators are motivated by the desire to be reelected and that, everything else being equal, responsiveness to public opinion is perceived as a good way to enhance one's electoral prospects (see Mayhew 1974; Downs 1957). Whatever the truth, congressmen believe that their stands on issues influence their electoral chances (Mayhew 1974, pp. 70–71). Not all issues are equal, of course. In general, the public is not very attentive to foreign policy questions, and legislators are not likely to pay much attention to public opinion on issues that are not salient to the public (Miller and Stokes 1966). It is possible, therefore, that the Senate paid little attention to public opinion very early in the war. The cost of the war increased fairly quickly, however, and the issue became more important. It became clear fairly soon that voters could be influenced by candidates' stands on the war.[7] In addition, voluminous data on public opinion at the national level were available to legislators, who would therefore have information to guide their votes, should they choose to pay attention to it.[8] Given that the issue was salient

with public opinion or war costs; see Burstein and Freudenburg (1977). Relevant studies will be cited below, but we have found none examining how war costs, public opinion, and demonstrations, either separately or together, affected policy change in Congress. For a theoretical discussion, see Andrews (1976).

[6] On public opinion and legislation or legislators, see Devine (1970); Miller and Stokes (1966). Cohen (1973, chap. 1) provides an excellent review of the literature on public opinion and foreign policy, which explicitly notes our ignorance about the relationship between public opinion and Vietnam policy. He emphasizes that, although we often think we know about the impact of public opinion on policy, the literature does not support such a claim.

[7] See Page and Brody (1972); for discussions of the circumstances under which foreign policy and defense issues enter domestic politics, see Zimmerman (1973) and Rosenau (1967).

[8] Senators would be primarily concerned with their own states but would probably still be interested in national trends. On the difficulties senators have in gauging constituency opinion, particularly in foreign policy, see Jewell (1962, chap. 8).

and that some information about public opinion was available, we would expect that, in the aggregate, the Senate would respond positively to public opinion.

How strong is the relationship likely to be? This is an uncharted area, and there is little theory to draw on. The simplest viewpoint is probably that, although the Senate would be inclined to follow public opinion, everything else being equal, the body was also under a variety of other pressures, including strong proinvolvement pressures from Presidents Johnson and Nixon. Some pressures might reinforce public opinion, but others would oppose it and tend to weaken its influence. Thus the relationship between public opinion and Senate voting can be expected to be moderate, not strong.[9]

Antiwar Protest Demonstrations

If senators saw antiwar protest demonstrations as representing the opinions of large numbers of constituents, Senate voting could be expected to be positively related to the incidence and magnitude of such demonstrations. We do not really know what senators thought about the demonstrations, however, nor do we have much idea about the impact of such demonstrations.

Gamson (1975) has recently provided some evidence that organized social protest can be effective in producing political change, despite the disfavor with which protest is often viewed. Despite the magnitude of anti–Vietnam war protest, however, we do not know whether it was effective; some thought it helped end the war, others thought it was counterproductive, while still others thought the demonstrations might have had an indirect effect by helping to get the antiwar point of view before the public.[10] There have been no studies of the impact of demonstrations on congressional voting.

Given the lack of past evidence and the diversity of hypotheses, we make no predictions about the impact of demonstrations on Senate voting. The attempt to assess the impact, however, will contribute to our knowledge about a phenomenon much discussed at the time.

[9] We know that here and elsewhere we are making predictions about aggregate-level outcomes which depend upon assumptions about individual behavior not tested here. The findings are merely expected to be consistent with the assumptions.

[10] Converse and Schuman (1970) and Berkowitz (1973) discuss these possibilities. As far as we can tell, Berkowitz's study is the only one which attempted a quantitative assessment of the impact of demonstrations. After examining the impact of a number of demonstrations on public opinion and American troop strength and munitions expenditures, he concluded that demonstrations might have had small, short-term counterproductive effects, but that they could easily be said to have had no impact in the long run.

War Costs

Our hypothesis is extremely simple and obvious: there should be a strong positive relationship between war costs—defined in terms of battle deaths and monetary costs—and Senate opposition to the war.

Although the hypothesis is obvious, it has not been tested. What we are really testing is the possibility that legislators assess the effectiveness of policies they voted for in the past and adjust future votes accordingly. This possibility has seldom been considered explicitly in past work on voting in legislatures, perhaps because researchers so seldom analyze change over time.[11] The impact of the past upon the present is considered explicitly in work on budgeting; a major controversy, in fact, revolves around whether legislators are often concerned with the substantive content of programs when they vote on budgets.[12] But the actual impact of past policy decisions on present voting has not been examined empirically, so we have little sense of the impact of past success or failure on present voting.

Change over Time

We were also interested in seeing whether the hypothesized relationships remained the same throughout the war. For example, the Senate might have been slow to respond to public opinion, or it might have responded to antiwar demonstrations only late in the war, when it became relatively clear that participants were not merely part of a radical fringe.

Our main focus was on 1970. Popular and congressional response to the American invasion of Cambodia in May was widely seen as a turning point in the history of American involvement. Senate outcomes on Vietnam-related roll calls certainly underwent a crucial change during 1970. Before the invasion of Cambodia, almost no dovish proposals received a majority of votes in the Senate, and the mean number of dovish votes received on proposals was 32. During 1970, however, dovish proposals began to receive a majority of votes fairly often, and the mean number of dovish votes

[11] This is not to imply that it has not occurred to others that legislators examine past policy in an area when deciding how to vote; it merely seems that such considerations are seldom incorporated explicitly into statistical analysis. In analyzing the influences on voting in Congress, the literature most often examines the impact of various political actors, such as constituents, lobbyists, party leaders, the president, and so on, often attempting to weigh the relative impact of various kinds of actors; see, for example, Jackson (1974). Ideology and party are often examined as well; in the defense realm, see Bernstein and Anthony (1974).

[12] The seminal work in the area claims that the congressional focus is mostly fiscal; see Wildavsky (1974); Davis, Dempster, and Wildavsky (1966). More recent work tries to show that programmatic concerns are often crucial; see Natchez and Bupp (1973) and Kanter (1972).

received by motions was 53; for the entire period from 1970 through 1973, the mean was 48. We decided, therefore, to see whether the furor over the invasion of Cambodia led to a significant transition point, by determining whether the relationships examined were the same after 1970 as they were in 1970 and before.

DATA

Our dependent variable is the total number of dovish votes received by each of 91 bills and amendments related to the Vietnam war voted on by the Senate between the passage of the Gulf of Tonkin resolution in 1964—the vote commonly associated with the beginning of major American involvement—and the passage of the War Powers Act in 1973, which is taken to be symbolically and concretely an important point in the end of American involvement.[13] The roll calls included are those dealing with American military forces in Vietnam; military and economic aid to Vietnam; American military forces in, and military aid to, other nations in Indochina; and those defining the war-making powers of the president. Dovishness was defined within the context of each series of roll calls.

Our independent variables are measures of the content of the motions, war costs, public opinion, and antiwar demonstrations.

The content of the motions was measured along three dimensions: (1) a dimension gauging the hawkishness of motions; (2) a funding dimension, with the motions most generous with regard to funding at one extreme; and (3) a war-powers dimension, with motions favoring presidential initiative at one extreme and those limiting the president at the other.[14]

Our basic measures of war costs were total battle deaths and the incremental monetary cost of the war through the fiscal year during which each vote was taken.

Public opinion was measured in terms of the only questions asked consistently by polling organizations over the whole course of the war. One measure gauged the proportion of the population which felt our entry into the war had been a mistake, while the other was the proportion of the population that approved of the way the president was handling the situation in Vietnam.[15]

[13] In a preliminary analysis, we tried using dovish votes as a proportion of votes actually cast; since the results were virtually identical with those that follow, however, we will use only the more straightforward dependent variable—the number of dovish votes—throughout this paper.

[14] Information about the construction of these and other variables can be found in the Appendix.

[15] Though neither of these questions is a satisfactory measure of position on a hawk-dove dimension, they do give some indication of American reaction to our Vietnam policy, and, as stated, they were the only questions asked throughout the war. A major

The basic measure of antiwar demonstrations was a monthly count of demonstrations reported in the *New York Times*. Although this is a crude measure, the number of demonstrations reported in the *Times* is probably highly correlated with the number actually taking place, and the number of demonstrations is probably highly correlated with the number of participants.[16] Given that we are concerned with national trends and their impact upon Senate voting in the aggregate, rather than upon individual senators, our measure should not be biased in any significant way.

For the time-series analysis, the data were organized on a monthly basis for all variables except monetary cost of the war, for which fiscal-year data were used. When data were not available on a monthly basis, values of the variables were estimated for the months needed for the analysis. The procedures are specified in the Appendix.

We decided to include the following variables in our equations: DOVE-YEA, the dependent variable, is the number of dovish votes cast in each of the 91 roll calls; DIM1 is the dimension which gauges the hawkishness of motions; DIM2 is the funding dimension; DIM3 is the war-powers dimension; COST is the incremental monetary cost of the war through the fiscal year of the vote, in billions of current dollars; DEATHS is the total number of battle deaths up to the time of the vote, in hundreds; RCNTDETH is the number of deaths during the six months preceding the vote, in hundreds; MISTAKE is the percentage thinking entry into the war was a mistake; APPROVE is the percentage approving presidential handling of the Vietnam situation; DEMONS is the total number of antiwar demonstrations up to the time of the vote, in tens; and RCNTDEMS is the number of antiwar demonstrations during the six months preceding the vote, in tens.

We included RCNTDETH and RCNTDEMS because we thought that recent events might have an especially great impact on Senate voting; the use of a six-month period is somewhat arbitrary, but changing the length of time involved did not materially affect the results when we tried it.

STATISTICAL MODELS

We are interested in examining the relationships between the independent variables, including content, and our dependent variable, the number of

problem with the "mistake" question is that a significant proportion of those who thought our entry was a mistake were hawks rather than doves; see Converse and Schuman (1970). Nevertheless, the question has proven useful in previous analyses; see Mueller (1973).

[16] Snyder and Tilly (1972, p. 523) found a correlation of 0.84 between the number of demonstrations and the number of participants in their much more elaborate study.

dovish votes:

$$\text{DOVEYEA} = b_1 + b_2\,\text{DIM1} + \ldots + b_i\,\text{DEATHS} + \ldots + b_n\text{RCNTDEMS}\,. \tag{1}$$

The analysis is complicated, however, by the fact that we want to determine whether the relationships were the same in the 1971–73 post-Cambodia period as they were earlier. To do so, we define a dummy variable with a value of zero for roll calls taking place through the end of 1970 and a value of one for roll calls taking place after that. This procedure is described in detail in Johnston (1963, pp. 221–28; for a political application, see Jackson 1974, chap. 5); in essence, it allows us to see whether there is a turning point between 1970 and 1971 at which some of the relationships change. Equation (1) is rewritten, using the same variables, but adding a series of variables, each of which is a variable from equation (1) multiplied by the dummy variable D:

$$\text{DOVEYEA} = b_1 + \ldots + b_n\text{RCNTDEMS} + a_1D + a_2\text{DIM1}\cdot D + \ldots + a_n\text{RCNTDEMS}\cdot D\,. \tag{2}$$

Because of the dummy variable in each of the a coefficient terms, the as are only computed for roll calls taking place after 1970, when $D = 1$. If an a coefficient fails to reach statistical significance, this means simply that there was no significant difference (between the 1964–70 and the 1971–73 periods) in the relationship between the independent variable and DOVEYEA; the b coefficient for that variable is the best estimate of the relationship for the entire war. In cases where an a coefficient is significant, the hypothesis of "no turning point in 1970" will be rejected. In such a situation, the a coefficient gauges the change in the relationship occurring between the earlier and later periods. The relationship between the independent variable and DOVEYEA is gauged by the b coefficient for the earlier period, and by the sum of the respective a and b coefficients for the later period.

FINDINGS

Zero-order correlations are presented in table 1, and we can immediately see some findings of interest, findings which also cause problems for multivariate analysis. MISTAKE, COSTS, DEATHS, and DEMONS are very highly intercorrelated;[17] each is a simple monotonic increasing function of time. It is perhaps not surprising that costs of the war and battle deaths were so highly correlated. Of more substantive interest is the fact that

[17] See Mueller (1973, chap. 3) and Hibbs (1974), for detailed discussions of the relationship between MISTAKE and DEATHS.

TABLE 1

ZERO-ORDER CORRELATIONS, MEANS, AND STANDARD DEVIATIONS

	1	2	3	4	5	6	7	8	9	10	11	M	SD
1. DOVEYEA ..												40.9	24.0
2. DIM1.	208											56.3	41.4
3. DIM2	−394	−007										7.2	35.5
4. DIM3.	−478	−331	101									5.1	34.5
5. COST..	587	440	−472	−214								87.0	31.1
6. DEATHS. . .	629	453	−454	−195	977							386.4	141.6
7. RCNTDETH...	−256	−186	331	322	−435	−357						14.2	17.3
8. MISTAKE.....	580	417	−454	−223	980	935	−480					59.4	12.8
9. APPROVE. ...	452	108	−373	−171	435	400	−260	464				53.2	9.2
10. DEMONS. . .	579	427	−483	−228	940	903	−611	954	513			139.1	56.8
11. RCNTDEMS .	243	133	−007	107	182	285	202	088	129	232		21.4	20.0
12. TIME* . ..	583	410	−501	−240	974	927	−611	990	504	971	080	78.6	25.7

NOTE.—N = 91; decimals implied; correlations greater than .17 are significant at .05, greater than .24 at .01, and greater than .31 at .001.

* Time measured in months since Gulf of Tonkin resolution.

public opinion and demonstrations are so highly correlated with war costs broadly defined. The most obvious interpretation is that the general public, as well as the antiwar demonstrators, were quite conscious of the costs of the war; as costs escalated without compensating benefit, people turned against the war.[18]

We had initially hoped to assess the relative impact of war costs and public opinion on Senate voting as well as to produce estimates of the effects of each of our independent variables. Unfortunately, the extremely high correlation between the measures of public opinion and war costs made satisfactory estimation impossible.

As a consequence of the pattern of correlations we discovered, we decided to leave COST out of the multivariate analyses. DEATHS, the other "war costs" variable, is more highly correlated with DOVEYEA and was a slightly better predictor in trial equations we ran in which we included first one and then the other. In addition, DEATHS is a more sensitive indicator, since we can gauge it accurately on a monthly basis, whereas COST is available only on a yearly basis and is less sensitive to specific events in Vietnam. But even after we omitted COST, the high degree of multicollinearity among MISTAKE, DEATHS, and DEMONS would produce unreliable parameter estimates were we to include them all simultaneously in our equations.[19]

We ultimately decided to estimate the parameters in the three equations presented in table 2: an equation including measures of content alone, an equation including content and war costs, and an equation including content, public opinion, and recent demonstrations.

The simplest potentially appropriate estimation procedure is ordinary least squares (OLS). When the models presented in table 2 were estimated using OLS, however, the Durbin-Watson d-statistic showed that serial correlation was a potential problem in every equation. In such a situation, OLS is likely to produce exaggerated estimates of t- and F-statistics and of R^2. To overcome this problem, we used an appropriate variant of generalized least squares (GLS), which is well described in the econometrics literature, to estimate the models.[20]

[18] Mueller hints at such an interpretation for his more limited data set but does not make it explicit (1973, chap. 3).

[19] As noted by the econometricians, little can be done statistically in such a situation, where we have no independent way to estimate parameters. We can make predictions using these data but are very limited in the causal interpretations we can legitimately make, especially given the fact that several of the independent variables increase monotonically with the passage of time.

[20] Problems concerning serial correlation and the proper use of ordinary least squares and generalized least squares are described in Wonnacott and Wonnacott (1970, chap. 6; the procedure employed here is described on pp. 331–32; see Theil [1971, chap. 6]). Hibbs (1974) provides a good explanation of GLS with sociological and political exam-

TABLE 2

MODELS AND DATA, SENATE DOVISH VOTES, 1964–73
(N of Roll Calls = 91)

UNSTANDARDIZED COEFFICIENT	EQUATION 1	EQUATION 2	EQUATION 3
CONSTANT:			
1964–70	35.65(4.99)**	...	−27.50(−2.01)*
Change, 1971–73	8.52(.99)	..	9.63(.18)
DIM1:			
1964–70		−.20(−2.88)**	−.21(−2.95)**
Change, 1971–73	.	.21(2.34)*	.23(2.54)**
DIM2:			
1964–70	−.03(−.55)	.05(.92)	.00(.02)
Change, 1971–73	−.35(−2.44)**	−.42(−3.03)**	−.35(−2.34)*
DIM3:			
1964–70	−.20(−2.95)**	−.27(−4.33)**	−.28(−4.44)**
Change, 1971–73	04(.32)	.03(.23)	.06(48)
DEATHS:			
1964–70	..	.15(10.87)**	..
Change, 1971–73	..	−.06(−3.23)**	.
MISTAKE:			
1964–70	...	...	1.23(3.48)**
Change, 1971–73	...	..	−.28(− 33)
RCNTDEMS:			
1964–70	..	...	.48(2.68)**
Change, 1971–73	.	.	−.70(−2.73)**
R^2	.30	.57	.61
F	7 1	16.1	11.2
Estimated serial correlation	.65	.29	.24

NOTE.—t-statistics in parentheses
* Significant at the .05 level, one-tailed test.
** Significant at the .01 level.

ples, including one rather similar to the analysis presented here. The program used is in Peck (1975). Another way to deal with serial correlation is to employ the method of first differences. This is most appropriate where theoretically required or where the serial correlation approaches one; neither condition applies here. In addition, there are conceptual (if not technical) problems in using first differences in a data set such as this, where the time intervals between data points vary so greatly and are often effectively zero with regard to possible change in many of the independent variables. Technically, since some of the independent variables are highly correlated with a variable measuring the passage of time, results from the use of first differences would be extremely similar to those presented. Table 2 presents GLS estimates. We also ran the same equations using OLS, and the differences in results were those expected theoretically: the relative magnitudes of coefficients were the same in both sets of results, but R^2 and F- and t-statistic estimates from OLS were higher than those from GLS; in a number of cases, we would have concluded from OLS that coefficients were significantly different from zero, whereas we could not so conclude from the GLS results. Thus our substantive interpretations do depend to some extent upon our choice of procedure. The GLS results are clearly more conservative. Finally, in order to avoid specification error (e.g., Duncan 1975, chap. 8), the equations were first estimated including all independent variables; then they were reestimated including only those variables that had significant coefficients in the first estimation, for either the 1964–70 period or the "change" variables. Variables with t-statistics of less than 2.0 in both cases were eliminated from the equations presented.

We see from equation (1) that content determines roll call outcomes to a significant extent. The *F*-statistic shows that the equation as a whole is significant at the .01 level.[21] In part, this is not a finding about voting but a test of the validity of the content measures. Senators voting on how dovish policy is to be must necessarily be affected by the dovishness of the motions being considered. If the correlation between content, as measured, and voting were zero, we would have reason to doubt the validity of the content measures (more on this point can be found in Burstein [1978]).

At the same time, however, the relationship between content and roll call outcomes still must be gauged; ours is the first work we know of which yields a quantitative estimate of the relationship. This is important for two reasons. First, as will be shown below, the relative importance of different content dimensions can vary over time; by examining the changes we can learn something about how the Senate operates. Second, given the results shown in equation (1) and the correlations of the content dimensions with other variables in table 1, it is clear that inclusion of the content measures in the analysis is essential; were they left out, the models would be seriously misspecified.

Equation (2) gauges the impact of war costs on DOVEYEA, while equation (3) focuses on the impact of public opinion. DEATHS had a significant impact on DOVEYEA, but RCNTDETH did not. The Senate seems to have responded to the cumulative cost of the war, but not to costs incurred during the period immediately before voting. MISTAKE and RCNTDEMS had a significant impact on DOVEYEA, but APPROVE did not. The fact that attitudes about presidential handling of the war are not related to Senate voting is perhaps not surprising, given the twists and turns of presidential policy and the fact that we are looking at Senate voting rather than presidential behavior. The "mistake" question, which refers to the war itself, is clearly the better predictor of Senate behavior.

As mentioned above, we cannot, unfortunately, satisfactorily separate the effect of MISTAKE from that of DEATHS because they were so highly intercorrelated. War costs seem to be the more basic variables, in that we would expect them to affect public opinion, rather than the reverse, in the short run. At the same time, the costs would have political meaning for the senators mostly insofar as they affected public opinion. We really cannot say that the Senate was responding to war costs rather than public opinion or the reverse.[22] We can say that the data are consistent with

[21] The use of significance tests where the relationship between the sample and the universe is unclear raises interesting questions; it is standard procedure among economists, who have developed an elaborate rationale for their procedures; see Johnston (1963, chap. 1).

[22] By using appropriate leads and lags, we should be able to establish causal ordering among the variables in a way not possible with the usual cross-sectional data; see Sims

the Senate's being responsive to public opinion and/or war costs. The fact that both costs and opinion were moving in the same direction might have reduced the degree of uncertainty felt by senators, and so have hastened their response, but at this point emphasis on one variable or another depends upon one's purpose and point of view.

Whereas table 2 presents changes between the two periods, table 3 offers the actual coefficients for dovish votes in each period. Focusing on equations (2) and (3) of table 2 and on the second and third sections of table 3, we can see that DIM3, the issue of presidential war powers, turns out to have had a consistent importance throughout the war, regardless of how the remainder of the equation is specified. In the light of that fact,

TABLE 3

COEFFICIENTS FOR SENATE DOVISH VOTES

	1964–70	1971–73
Content:		
DIM 1	...[a]	...[a]
DIM 2**	−.03	−.38
DIM 3	− 20	−.16
Content and war costs:		
DIM 1*	−.20	.01
DIM 2**	.05	−.37
DIM 3	−.27	−.24
DEATHS**	15	.09
Content and public opinion:		
DIM 1**	− 21	.02
DIM 2**	.00	−.35
DIM 3	−.28	−.22
MISTAKE	1.23	.95
RCNTDEMS**	.48	−.22

[a] Variable significant at the .05 level in neither period.
* Difference between time periods significant at the .05 level.
** Difference significant at the .01 level.

(1972). Because several of the variables are strongly correlated with time, however, leads and lags tell us little. Correlations among DEATHS, COST, MISTAKE, DEMONS, and DOVEYEA are highest in all cases where the variables are all measured for the same months, and the correlations decrease monotonically but very slowly as six-month, one-year, and two-year lags are introduced. We considered examining the impact of distributed lags, but there were major conceptual and practical problems in doing so; in ordinary time-series analyses, variables are measured at regular intervals —annually, quarterly, etc.—but the intervals between roll calls were extremely variable. On one hand, the nature of the relationships among the independent variables makes the Vietnam issue less than perfect for demonstrating the utility of a new approach to studying legislative change; we would prefer to be able to come to grander and more elegant conclusions. On the other hand, the issue was an extremely important one, well worth looking at for anyone concerned with democratic political change. It would be silly not to study an issue merely because theoretically relevant independent variables were highly intercorrelated. In addition, discovering the correlations teaches us something about the issue and would probably not have occurred if we had not developed the approach being demonstrated in the paper.

the varying impact of the other two dimensions is somewhat surprising; DIM2, the funding dimension, becomes important only after 1970, with DIM1, the main hawk-dove dimension, being important only up until 1970.

DEATHS had a significant impact on Senate voting, but one that varied over the course of the war. Translating the coefficients into concrete terms, we can say that every 700 battle deaths produced another dovish vote, on the average, during the earlier part of the war (as far as we can estimate given the set of variables included in these equations). From 1971 on, however, the impact of deaths declined, with about 1,100 deaths being required to produce another dovish vote.

The situation with regard to measures of public opinion was mixed. As mentioned, APPROVE had no significant impact on Senate voting. MISTAKE was related significantly and consistently to Senate voting; the greater the proportion of the public that thought our entry into the war had been a mistake, the greater the number of dovish votes in the Senate, for the entire war.

Finally, the results for RCNTDEMS are striking. Contrary to what many have speculated, and also somewhat paradoxically, as the demonstrations became more widespread and acceptable in later years, they became less productive as well. Recent demonstrations seem to have moved the Senate in a dovish direction up through the time of the Cambodian invasion; after that, their impact became negative. In other words, those who claimed that the Senate responded positively to antiwar demonstrations and those who claimed exactly the opposite were evidently both partly right. Early demonstrations may have indeed had the consciousness-raising impact that their supporters claimed, but later demonstrations may also have had the alienating effect that many of their opponents charged.

If 1970 was a turning point, what kind of turning point was it? Our findings on the content dimensions—that the hawk-dove dimension was salient only up through 1970, with the funding dimension becoming significant only thereafter—were initially surprising to both authors, yet they make considerable sense after a moment's thought. The power of the purse is in fact a time-honored source of congressional control over public policy and over the executive branch. The data may imply that up through 1970 the Senate was not inclined to treat American participation in the war as an ordinary issue to be dealt with by usual means. The 1970 turning point may have been the time when the war issue came to be treated like other issues, with debate and effective control over policy revolving around funding.

In the same context, our initially puzzling findings on the importance of antiwar demonstrations also begin to make sense. As the 88–2 margin on the Gulf of Tonkin resolution (along with the consistently lopsided ad-

ministration victories and the small proportion of Americans opposing the war during the first several years) indicates, opponents of the war had to do a great deal of consciousness raising before they could expect to be taken seriously and begin to debate the merits of their stance from a position of even moderate strength. It appears possible that demonstrations may be fruitful at an early stage—when the aim is to make an issue salient and the subject of serious debate—but ineffective later, when the Senate doves were more concerned with more subtle problems of drafting legislation that could gain a majority of votes. Demonstrations are more effective for attracting attention than for providing detailed guidance on coalition building.

CONCLUSIONS

In this paper we have shown, by means of a single significant and substantive example, that it is now possible to deal directly and quantitatively with the causes and correlates of major legislative changes over time. The method is nonarbitrary, relatively straightforward, and easily replicable. Probably more important, however, the findings are sensibly and substantively interpretable, and they have provided us with knowledge about an important topic that has simply not been available before. We have seen that content alone, not surprisingly, can explain a reasonable proportion of the variance in Senate voting on Vietnam but that the addition of a very small number of other variables boosts the proportion explained substantially. As far as we can tell from our data, our hypotheses about Senate voting have been supported in a general way: although public opinion and war costs (measured either in dollars or in human lives) were so tightly correlated that it simply was not reasonable to include both types of variables in our equations at the same time, either type individually had a strong and consistent predictive impact on Senate voting throughout the war. The Senate did respond to war costs and public opinion.

Our findings are all the more encouraging in view of the lack of precedents in this area and the fact that we have not yet included a wide range of other factors that might reasonably be expected to lead to a further improvement in R^2. On the other hand, at this point we have not been able to explain many of the results in other than an ad hoc way: our speculations about the impact of demonstrations may or may not be correct, for example, and we are also not sure why the impact of public opinion as measured by the "mistake" question remained constant, while battle deaths appear to have had a declining marginal impact—although we could clearly offer speculation on these points as well. The crucial point to be made here, however, is this: we do not have a satisfactory theoretical framework in

which to explain the results, in part because it has not previously been possible to arrive at such results. That is, the development of a new approach to the study of legislative change—one which enables us to make relatively precise estimates about the impact of various factors on legislatures—may be producing a new body of data which we do not yet have the theories to explain.

We have outlined a new approach to the study of legislation and tried to demonstrate some of its strengths and weaknesses by applying it to a particularly important issue in recent American history. The approach needs to be applied to many more issues before its full range of strengths and weaknesses can be known; other types of variables must be included in the models as well. Yet even in this obviously preliminary paper, we have been able to see things we had not been able to see before, and that, perhaps, is the clearest test of value for any new perspective.

APPENDIX

Data Sources and Definitions of Variables

Senate voting results.—*Congressional Quarterly Almanac* (1964–73). Each vote was treated as if it took place on the first day of the month in which it took place, except for *CQ* roll call 136, 1970, which was placed in June with other votes on Cambodia.

Incremental costs of the war.—For fiscal years 1965–74 (estimated), from Fried et al. (1974).

Battle deaths.—U.S. Senate Committee on Foreign Relations (1971, pp. 5–6); U.S. Department of Defense (1974, p. 61). Summed to the beginning of the roll call month where feasible or estimated where only quarterly or annual data were available; from 1971 on, only annual figures are available.

Public opinion.—Gallup results (and estimates—see below) for the month of the roll call were used.

A. Mistake to have entered the war: results from two questions were merged; cf. Mueller (1973, chaps. 1, 3); $N = 23$: "In view of the developments since we entered the fighting in Vietnam, do you think the U.S. made a mistake in sending troops to fight in Vietnam?" and "Some people think we should not have become involved with our military forces in Southeast Asia, while others think we should have. What is your opinion?"

Because the data showed a strong linear trend over time ($R^2 = .84$ for the "mistake" response), the percentages were regressed on time to provide estimates for months when the question had not been asked. For months for which observations were available, they were used; for other months, regression estimates (ordinary least squares) were used.

B. Approval of handling of war, $N = 47$: "Do you approve or disapprove of the way the [Johnson, Nixon] administration is dealing with the situation in Vietnam?"

Sometimes there are slight variations in wording. Because the percentages showed only a weak trend over time, simple linear interpolation was used to estimate values for months when the question was not asked. Results from the first and last asking of the question were used for earlier and later months, respectively.

Data are taken from Gallup International Inc. (1964–1973) and Gallup (1972, vol. 3).

Antiwar demonstrations.—New York Times Index, 1964–73. Domestic antiwar demonstrations mentioned each month in the *Index* were simply tabulated; when the *Index* referred to numbers in general terms, the *Times* itself was consulted for specifics.

Measuring legislation.—In order to analyze legislative outcomes, we must be able to conceptualize and measure them adequately. Several methods have been employed in the past to deal with legislation quantitatively, but unfortunately none was adequate for this paper without extensive modification: (*a*) dummy variables, often used to indicate whether particular provisions have been adopted, are too crude (e.g., Jackman 1975; Walker 1969); (*b*) sophisticated content analysis is best used where there is a sound theoretical basis for defining dimensions, which was lacking here; in addition, there are few models to draw on (e.g., Ogburn [1912] 1964; Pomper 1968: chaps. 7–8); (*c*) expenditure data do not adequately describe all legislative activity; (*d*) roll call analysis has not incorporated ways of dealing with a single, fairly specific issue over time, nor of dealing with personnel turnover; roll call analysts do not deal with situations in which a legislative body as a whole shifts its position on an issue (see MacRae 1970; Clausen and Cheney 1970, p. 149). To overcome these problems, roll call analysis techniques were modified as briefly described below; an extensive discussion is provided in Burstein (1978).

Essentially, the aim was to develop a technique that would make it possible to determine whether the motions voted on could be described in terms of a small number of dimensions and, if so, to place each motion on the dimensions, so that locations on the dimensions could be treated as variables used in statistical analysis.

We concluded that our intuitive way of thinking about dimensions, when we are concerned with change, is in terms of a Guttman-like scale of items. For example, we might think of motions as being on a hawk-dove dimension, with motions calling for withdrawal at one end, those maintaining the status quo in the middle, and those calling for great escalation at the other end. If we want to measure change in legislation over time,

we want to be able to form dimensions in terms of the relationships of the motions to each other. Each dimension should be a function of similarities and differences among the items—similarities in that the items all reflect part of a single underlying dimension and differences in that each item represents a different point on the dimension.

The conceptual framework for such an approach was outlined by Guttman (1954), who suggested defining dimensions in terms of rank orders among variables called "simplexes." A set of variables forms a simplex if the matrix of correlations among them can be arranged so that the largest correlations are all next to the main diagonal and decrease monotonically as one goes away from the main diagonal. The relations among the variables (motions) can be interpreted in a manner comparable to the interpretation of relations among items in a Guttman scale. Defining dimensions of legislation in terms of simplexes enables us to define dimensions in terms of the relationships among the items, while arraying the items along the dimensions.

The procedure was as follows: the seat, rather than the senator, was chosen as the unit of analysis. (When roll call analysts study voting over time, they usually consider only seats continuously occupied by the same person for the entire period. This is simply not an acceptable procedure when a long period is being considered and turnover is high [over 50% in the period studied].) A vote on the dovish side of a motion gave a seat a score of one and a vote on the hawkish side a score of zero; then the correlations among the 91 roll calls were calculated. Using seats as the unit of analysis, when newly elected legislators perceive the dimensions of legislation the same way as incumbents, the same dimensions should be detected as if no replacement had occurred. If newcomers see things differently, there will be no interpretable dimensional structure.

The correlation matrix was subjected to Smallest Space Analysis (SSA), the Guttman-Lingoes version of nonmetric multidimensional scaling, which can be used to discover simplexes in correlation matrices (unlike factor analysis; see MacRae 1970, p. 47; on SSA, see Guttman 1968; Lingoes 1973; Roskam and Lingoes 1970). A good solution (coefficient of alienation of 0.16) in three dimensions was found; the dimensions were interpretable as a hawk-dove dimension, a funding dimension, and a war-powers dimension. After rotating the axes, which are initially arbitrary, each motion was given a set of scores, each score being the position of the item on each dimension. The scores were then treated like other variables in the analysis (for an example of a similar procedure, see Laumann and Pappi 1973). The three variables thus became the measures of content. To control for changes in context, it is necessary to control for time in the statistical analysis (a fairly common procedure where trends are involved); the

equivalent control in the analysis here is the variable very highly correlated with time—DEATHS.

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Modernizing Institutions, Mobilization, and Third World Development: A Cross-national Study

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Following the implications of modernization theory, we test via panel regression analysis several hypotheses relating modernizing institutions to the economic development of poor countries. Controlling for the economic constraints imposed by initial poverty and world-system position, we find that the school furthers economic development while exposure to the cinema hinders it. Further analysis shows that these effects vary by political context: in countries with mobilizing regimes, the positive contributions of the school are strong, while in countries with nonmobilizing regimes the adverse effects of the cinema are strong. Following Portes, we argue that the cinema impedes economic growth by transmitting and promoting Western values incompatible with the social ethos that must accompany programs of national economic development. We discuss the relevance of our findings to the problem of social mobilization in poor countries.

I INTRODUCTION

The Problem

The body of works known collectively as "modernization theory," including its more or less structural and more or less psychosociological variants, has been under attack from without and within. Dependency-oriented scholars fault this school for promoting ideologically biased or otherwise false theories of economic development (Hechter 1975; Wallerstein 1975; Hill 1975). At the same time, Portes (1973*b*), a major contributor to modernization research (1973*a*), attacks other modernization theorists for promoting a counterproductive model of national economic development. In contrast to world-system-oriented scholars who dismiss the whole modernization approach as an ideological red herring (for example, Wallerstein 1975), Portes sustains the essential validity of the concept of modernity. His critique bears on the developmental consequences at the national level of the modernization of individuals.

In this paper we address the schism between Portes and other modernization theorists, particularly Inkeles and Smith. Specifically, we test Portes's contention that the modernization of individuals has a detrimental impact on the economic development of poor countries. The test we present constitutes more than a simple adjudication between Portes and other modern-

ization theorists; we also give consideration to an explicitly political factor —regime orientation toward social mobilization—and we control for transnational economic factors thought to circumvent or override modernization processes.

Individual Modernization and Economic Development

The primary interest of modernization-oriented scholars such as Inkeles and Smith is the social psychology of individual change rather than the sociology of economic development. Indeed most modernization scholars seem to harbor a diffuse, multidimensional concept of national development: "Many of the new [post-World War II] states were actually hollow shells lacking the institutional structures which make a nation a viable economic enterprise" (Inkeles and Smith 1974, p. 3; see also Inkeles 1971; Almond and Verba 1963; Lerner 1965; Eisenstadt 1966; Kahl 1968; Bernstein 1970). Neither Inkeles and Smith nor other scholars who share their perspective present findings that link the modernization of individuals directly to economic progress. This state of affairs leaves open a question of great importance to contemporary theory of economic development: Does modernization theory, in particular its social psychological variant, necessarily contain a model of economic development, as its critics assume?

Schnaiberg (1970) argues that a major obstacle to the evaluation of modernization research has been the failure to specify the focus of analytic concern. According to his argument, two crucial dimensions characterize the modernization literature: (1) "the unit of analysis," that is, whether the focus is on the aggregate or the individual and (2) "the aspect of development," that is, whether the concern is for the mechanisms of modernization or the end state. He uses these two dimensions to delineate four subareas within modernization research (see table 1).

In fact, however, modernization theory is not as fragmented as modernization research. Implicit in modernization theory is a specific causal chain connecting these four "subareas" (see fig. 1).

The most complete and probably the most explicit presentation of this chain is found in Inkeles (1971) and Inkeles and Smith (1974). A syn-

TABLE 1

AREAS OF STUDY WITHIN MODERNIZATION THEORY

	Aspect of Development	
Unit of Analysis	Mechanisms	End State
Individual...	1	2
Aggregate............	3	4

Source.—Schnaiberg 1970.
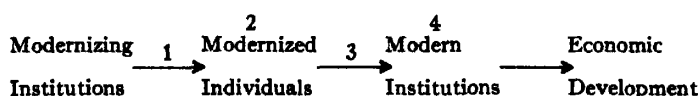
FIG. 1.—The process of modernization (based on Inkeles and Smith 1974)

thetic reading of their arguments confirms the idea that they are primarily interested in psychosocial change and its structural source. The structural sources of change they study are several modernizing institutions: the factory, the school, and the mass media. Inkeles and Smith originally thought of the factory as the prime individual modernizing institution (1974, p. 6). Given an interest in aggregate economic development, this view of the factory constitutes a near tautology. It only suggests that industrialization creates modern individuals capable of staffing industrial establishments in ever widening circles. Inkeles's main findings, however, are that the school, closely followed by the mass media, modernizes individuals more than does the factory (1971, p. 277).

How does a poor country benefit economically from a more modernized population? Inkeles and Smith argue that modernized individuals staff the modern institutions that are necessary for economic growth:

> . . . There is more to national development than a high GNP per capita. Some of the new nations have become aware of the critical importance of institution building as a concomitant of, indeed, as a prerequisite for sustained national development [itself previously defined to include economic development]. [1974, p. 4]
>
> . . . modern political and *economic* institutions make certain general demands on the people who work within them. They require [there follows a list of modern traits]. . . . [Ibid.; emphasis added]
>
> We are of the opinion that neither rapid economic growth nor effective government can develop or, if introduced, will be long sustained without the widespread diffusion in the rank and file of the population of those qualities we have identified as those of the modern man. [Ibid., p. 316; closing statement of the book]

Inkeles and Smith's argument, then, can be summarized as follows: Modernizing institutions such as the school and the mass media create modern individuals who staff the modern institutions that are necessary for economic growth. This argument—also implicit in Moore (1951, 1964), Hunt (1966), Hoselitz (1957, 1960), and Hagen (1962)—is the one we illustrate in figure 1.

Here Inkeles and Smith's theoretical arguments encompass Schnaiberg's fourfold classification of the modernization literature. Schnaiberg's four subareas address different phases of the sequence of modernization found in Inkeles and Smith's implicit paradigm. In figure 1 we show the corre-

spondence between Schnaiberg's division of the field and the causal order implicit in Inkeles and Smith.

In principle, the relationship between individual modernization and economic change can be studied empirically by selecting causal variables from within any of the three first steps in figure 1 (modernizing institutions, modernized individuals, and modern institutions). However, there are practical and theoretical advantages to beginning with a study of the relationship between the strength of modernizing institutions and economic development. (1) Modernizing institutions are causally prior to modernized individuals and modern institutions. Traditional individuals must be modernized before they can create and staff modern institutions. (2) By implication, the strength of modernizing institutions is the most policy-relevant variable in figure 1. If a modernizing elite attempts to foster economic development directly by creating modern institutions without first cultivating the necessary human capital, the modern institutions are likely to be stillborn. (3) Modernizing institutions such as the school and the mass media are typically state sponsored, controlled, or influenced. Political elites may seek to diffuse modern values and attitudes through these institutions in order to prepare the work force for the staffing of modern institutions in the production realm and elsewhere. They may also use these institutions more directly to promote the mobilization of the population in support of national programs of economic development.

The reach and influence of modernizing institutions is the starting point of two widely divergent views of the consequences of unbridled sociocultural modernization for the economic performance of poor countries. The mainstream prediction, represented by Inkeles and Smith, views such modernization as favorable to economic development. Portes's critique points to the dysfunctional economic effects of modernization.

Exogenous Modernizing Institutions: Portes's Critique of Mainstream Modernization Theory

Portes's attack on the model of development implicit in modernization theory is two-pronged (1973*b*). First he denounces the unspoken assumption that traditional institutions are necessarily antieconomic. His lively defense of traditional institutions is based in part on the notion that they are vulnerable to the threat of *non*traditional institutions that successfully encroach upon them. The perceived potency of this threat is illustrated by Portes's second attack (1973*b*, pp. 258–60 and 265–69), that the attitudinal modernization of the populations of poor countries may hinder their economic progress. Of interest to us is his speculation that modernization is in fact "Westernization" and that it retards economic progress

in three distinct ways. (1) Competent individuals with modern attitudes are more likely to emigrate than others, thereby depriving their countries of their skills. (2) Modernized individuals must be consumption rather than savings oriented. This orientation undermines capital accumulation. (3) The individual secularism inherent in a modern outlook diverts energies from the mobilizing ideologies used by successful elites to induce the population to greater effort. The gist of Portes's reasoning is that the attitudes prevalent in a developed country are inappropriate in a developing country.

If individuals' traditional ways of thinking and acting are being changed, there must be agents of such changes. The most likely candidates for this role are the school and the mass media, the very modernizing institutions identified by Inkeles and Smith. Portes's conceptualization of the link between modernization and economic change is substantively opposed but morphologically parallel to that of Inkeles and Smith (fig. 1). The key difference between the two perspectives is Portes's insistence that to the extent that modernizing forces are exogenetic they are largely malignant:

> . . . Modern traits do not arise naturally from internal processes of structural change, but artificially from the impact of Western cultural diffusion centered in urban areas. Factors which have been repeatedly found to promote modernity . . . may function less as carriers of self-evident psychological implications discovered anew by each developing population than as vehicles for closer and more extensive exposure to Western values and life styles. [Portes 1973*b*, p. 271]

Thus, according to Portes, a major task facing the elites of poor countries is to restrain and channel modernism, not to encourage its unbridled expression. The presumed adverse effects of Westernization may be mitigated, for example, by the actions of a regime oriented toward social mobilization. Such a regime might use modernizing institutions to generate support for national development programs. Such programs, which often take a communitarian, collectivist form, may well be utterly irrational from the individualistic, utilitarian viewpoint central to mainstream modernization theory (Portes 1973*b*, pp. 268–69).

Portes's view thus paves the way for a reformulation—within modernization theory—of the problem. To the question, Do modernizing institutions affect economic development? we must now also add the questions, (1) Do exogenous, particularly Western, modernizing institutions and domestically based modernizing institutions affect *differentially* the economic progress of poor countries? (2) Do the effects of modernizing institutions on economic development vary by political context? In Sections 4 and 5 of this paper we show that Western and domestically based modernizing institutions affect the economic performance of poor countries differentially. We show further that the political context within which such

institutions exert their influence profoundly affects the developmental outcomes. In order to carry out this double demonstration, we first establish that the cinema is an important modernizing institution in poor countries (Sec. 2) and that modernizing institutions are eminently available to indigenous elites seeking to subordinate them to political purposes (Sec. 3).

2. THE CINEMA AS AN EXOGENOUS MODERNIZING INSTITUTION

Inkeles and Smith emphasize the efficacy of two modernizing institutions: the school and the mass media (1974, p. 283). Our model therefore includes measures of school and mass media influences. (We do not include in the model a measure of the influence of the factory, the third modernizing institution in their study. It would be inappropriate to include such a measure in a study of economic development because of the near equivalence of industrialization and economic development. [See also Sec. 1.]) In terms of the reformulation presented above, the school is predominantly a domestically based institution, while the mass media may be domestic or foreign.[1]

Inkeles and Smith construct their measures of mass media influence with data on newspaper and radio exposure. We use, instead, information on the dissemination of modernizing influences through the cinema. This substitution has a number of methodological and theoretical advantages. Four distinct methodological advantages may be pointed out:

1. The distribution of movies, worldwide, is a strictly commercial operation(except for a very small number of advertising and propaganda films). Therefore the degree to which individuals are exposed to movies leaves a reliable trace in the form of commercial records. Such records are easily collected by the national agencies that report them to international organizations.

2. The determining methodological reason for using the cinema is that movie attendance is not limited by literacy (Rogers 1969, p. 155), often problematic in poor countries. Hence, by choosing the cinema as our second modernizing institution, we are giving propositions derived from Inkeles and Smith a greater chance for verification than if we followed these scholars directly by selecting other kinds of mass media.

3. It seems to us that the message content of films varies less from culture to culture than that of radio programs and newspapers, which are

[1] We recognize that some schools in Third World countries were established by Westerners and may transmit Western values today. However, compared with the cinema, the school is primarily domestic, and there is a strong tendency for all schools to be controlled by the polity as a part of an ongoing process of state formation (see Rubinson 1972). Our reformulation of modernization theory requires that we distinguish degrees of Western influence. In this light, schools are captive institutions and they are eminently domesticable; foreign films shown in the cinema are not.

often tailored to a local audience.[2] The message content of films shown in poor countries is especially uniform, since, as we will see, many of them come from a single source. If, as Inkeles (1969) argues, the influence of modernizing institutions leads to the same attitudinal changes in all cultures, then the originally uniform content of movies should have more uniform consequences than the less uniform content of newspapers and radio broadcasts.

4. Using the cinema as an instance of modernizing mass media allows us to cover many poor countries. The cinema, as we will show, is a cheap modernizing institution. Its deployment does not require the financial investment or the technological resources associated with other modernizing institutions such as the factory or even the school (see Sec. 5 below, esp. table 3).

It is easy to justify the substitution of the cinema for other mass media on theoretical grounds also. Whatever the other media are supposed to do to individuals, movies do as well or better.

First, movies are made by modern rather than traditional individuals. This is likely to be less true of radio broadcasts and newspapers because of the possibility of local (that is, "parochial") control. A fair share (mean = 61%, ca. 1953) of all movies shown in poor countries during the period we consider originated in the United States. It is not difficult to speculate that many of these were "western" (that is, cowboy) movies and surely conveyed a sense of "participation" and "individual affirmation and fulfillment," the "keywords" in modernization writings, according to Portes (1973*b*, p. 256).[3] In general, movie watching is likely to expose traditional individuals to massive, concentrated doses of the most unadulterated representations of modern values. (See Rogers 1969, pp. 155–56.)

The last observation introduces the primary theoretical justification for selecting the cinema as the relevant mass medium. Our reformulation requires that we distinguish between Western and domestic modernizing forces. The commercial structure of the world movie industry facilitates such distinction: while many countries possess a domestic movie industry, the vast majority of poor countries show films imported mostly from the

[2] In other words, the cinema as a modernizing institution is less likely to suffer from the disabilities discussed by Inkeles and Smith (1974, p. 144). Films are far less likely to have a "neutral" or "traditional" content than smaller media such as radio and newspapers. Inkeles and Smith, for example, refer to the use of radio by a "fundamentalist" church. On the other hand, it seems possible to argue that the smaller media with their susceptibility to local control may lead more promptly to political involvement.

[3] In general, we make the assumption that the more modern the country in which a film is made, the more modern is its content. While the relative rankings of developed countries on this dimension might be difficult to establish, there is little reason to doubt that American films carry a more modern message than films of other "Western" origins for the period considered.

West, particularly from the United States.[4] Thus the separation of films into domestic and foreign categories allows a direct test of the speculation that exogenous modernizing influences hinder the economic development of poor countries. In the variable we construct to capture the modernizing influence of Western films, we enter only data on films of American origin. This poses no problems of interpretation, since U.S. films constitute a large percentage of all Western films shown in poor countries, and, as we have mentioned (n. 3), they should be the most modern in content.

3. THE POLITICAL AVAILABILITY OF MODERNIZING INSTITUTIONS

The cinema clearly illustrates one of the outstanding features of modernizing institutions considered from a policy viewpoint: they are very "politically available." Their political availability is manifested in two distinct ways: (1) their expansion is only moderately limited by structural or economic factors, and (2) the content of the influences disseminated by them can be manipulated toward particular objectives.

Contrary to Inkeles and Smith (1974, p. 144), the deliberate expansion of modernizing institutions such as the school and the cinema is relatively unconstrained by a low level of industrialization or by initial poverty. As Bendix (1977, p. 419) argues, (the political elites of) poor countries must place a premium on education and communication systems since they are "more available than the capital required for modern technology." In addition, the development of these institutions is not circumscribed by the capricious distribution of natural riches over the earth's surface. This freedom from structurally limiting conditions should increase the attractiveness of these institutions to the elites of poor countries, insofar as they are perceived as instruments of economic development.

The inclination to utilize these institutions must be just as great as the inclination to develop them, because they may be viewed as empty containers to be filled according to the goals and means of the user. The corollary to Portes's speculation that modernizing institutions transmit (malignant) Western values is that they can be used to transmit appropriate (that is, health-inducing) values. Education can be a destroyer of tradition, or it can be used as a bulwark of tradition. Scrupulous officials and hard-working small peasants can be turned into movie heroes almost as easily as *pistoleros*. Furthermore, modernizing institutions may be used to reformulate modern values so that they are more compatible or consistent with indigenous cultures. This reformulation of modern values can be perceived as a remedy to what O'Dea (1971, p. 175) calls "the lack of social and political leverage of the new [Third World] elites" which is in

[4] Elliott and Golding (1974) have shown that the international distribution of news follows a similar pattern.

part a product of "the superficiality of the new modernization ideals, their lack of rootedness in the indigenous culture."

To argue that modernizing institutions are politically available is not to argue that they are always utilized by elites or that they are always utilized successfully. The political elites of poor countries differ widely in their propensity to intervene in the value orientations of their constituencies. Regimes with a high propensity to do so—"mobilizing regimes" (see Deutsch 1953)—are most likely to exploit the availability of modernizing institutions. Such institutions may be more or less effective, depending on the political context of regime orientation toward mobilization. In practice, this may mean that modernizing institutions will affect the course of economic development only within the context of regime orientation toward mobilization.

A regime lacking a mobilizing orientation, for example, may fail to utilize the positive potential of modernizing institutions. A nonmobilizing regime may fail as well to bar the entry of Western values (which are detrimental to development, according to Portes). Such a regime may also be less able than a mobilizing one to counter the influence of such values in the exposed population. This reformulation of the relationship between modernizing institutions and economic development is substantially in line with Portes's third objection to the intrusion of Western values (see Sec. 2 above). If the reformulation is correct, two consequences should follow. (1) The school must have a positive effect on development and exposure to the Western cinema a negative one. (2) The magnitude of both effects must vary with orientation toward mobilization; countries with mobilizing regimes should be characterized by stronger positive effects of the school and weaker negative effects of the cinema than countries with nonmobilizing regimes.

In the analysis below, we examine the direct and indirect consequences of political context, conceived as the orientation of regimes toward mobilization, on the relationship of modernizing institutions to economic development.

4. DESIGN AND MEASUREMENT

According to mainstream modernization theory, the psychosocial modernity of the populations of poor countries is an important factor in these countries' economic progress. It is argued that individuals can be modernized by being exposed to modernizing institutions. Inkeles and Smith have shown that the school is a major modernizing institution. We have argued that the cinema must also be an important modernizing institution. It follows from the statements above that the greater the exposure of the populations of poor countries to the school and the cinema, the greater should be these

countries' economic progress in a subsequent period. However, according to the reformulation based on Portes presented above, the greater the exposure to the Western cinema, the less the economic progress. We consider the school to be primarily a domestic modernizing institution and the Western cinema an exogenous one (see Sec. 2). We test the partially contradictory propositions above by regressing a measure of economic development on measures of school and cinema influences.

Measuring Economic Development

We examine these propositions in panel regression analyses. A panel design allows for the incorporation of variables measured at two points in time. This reduces the likelihood of false inference due to reciprocal causation. More than one method is possible. One can, for example, model change in countries' wealth by regressing wealth at $time_2$ on wealth at $time_1$ and on the other independent variables measured at $time_1$ (Heise 1970; Chase-Dunn 1975). With this method it is necessary to include the lagged dependent variable in the regression equation because of the stability of countries' wealth over the short time span covered by our data. We prefer, however, a different formulation. Statistically equivalent to the lagged dependent variable technique, it consists of regressing the arithmetic difference between wealth at $time_2$ and wealth at $time_1$ on the value of wealth at $time_1$ and on the $time_1$ values of the other independent variables. This technique permits the examination of the effect of the initial level of wealth on subsequent changes in wealth. The relationship between initial level and change in wealth is of intrinsic theoretical interest and thus demands explicit estimation.

The equation that describes our panel model is as follows:

$$(Y_{t2} - Y_{t1}) = A + BY_{t1} + C_iXi_{t1} + U_{t1}$$

where

$(Y_{t2} - Y_{t1})$ = change in wealth from $t1$ to $t2$;
Y_{t1} = initial level of wealth;
Xi_{t1} = independent variables at $t1$;
U_{t1} = error term;
A = constant;
B, C_i = regression coefficients.

In order to maximize cases we use a conventional measure of wealth, GNP/capita. This measure is appropriate because it has a direct economic interpretation (Emmanuel 1974, pp. 70–73) and because it is highly correlated with indicators of the more general concept of development (Fin-

sterbuch 1973). The dependent variable, change in wealth (GNP/capita in 1970 minus GNP/capita in 1950), incorporates 1950 and 1970 data from ICPR, the International Bank for Reconstruction and Development *World Tables*, and from the World Bank *World Atlas*.

Initial GNP/capita (1950) has a positive, monotonic relationship with 1950–70 change in GNP/capita, but the slope of the regression of the latter upon the former flattens in the upper ranges of GNP/capita (1950). To correct for this nonlinearity and to conform to the assumptions on which regression analysis is based, we use a logarithmic transformation (base 10) of the lagged dependent variable, GNP/capita (1950).

Measuring the Magnitude of Modernizing Influences

Following the implications in Inkeles and Smith (1974), we examine the effects of two modernizing institutions, the school and the cinema, on economic change. We infer the strength of school influences through a single indicator: secondary school enrollment as a proportion of secondary school age population circa 1953 and in 1965 (secondary-enrollment ratio). This indicator is an explicit measure of the degree to which the school as a modernizing institution reaches its appropriate target. We use secondary- rather than tertiary-enrollment ratio because the latter is more highly correlated with initial GNP/capita. Furthermore, the analysis of the effects of secondary-enrollment ratio is not affected by the ceiling effect that complicates models using primary school enrollment. The mean value of secondary-enrollment ratio is 0.10, and the standard deviation is 0.09 for the set of countries we use in this study. This measure is coded from the 1971 *UNESCO Statistical Yearbook*.

To measure the exposure of the general population to the cinema, we use movie attendance per capita circa 1953 and in 1965 collected from three sources: *International Motion Picture Almanac*, UNESCO's *World Communications* for the years 1951 and 1956, and ICPR (Russet et al. 1964).

The cinema carries Western as well as domestic modernizing influences (see Sec. 2). To measure the differential supply of Western films, we use the percentage of all films shown imported from the United States circa 1953 (% U.S. films). We use percentages rather than absolute numbers because (1) more countries report percentages than absolute numbers, and (2) absolute numbers of films of each origin do not indicate the relative supply of Western influences; frequently those countries that show a large number of domestic films also import large numbers of American films.[5]

[5] Perhaps contrary to intuition, in the 1950s the most diverse underdeveloped countries relied heavily on imports of American films: South Korea, 95%; Jamaica, 88%; Angola, 74%; Sierra Leone, 70% of all films shown.

This measure, % U.S. films, is coded from the same sources as movie attendance per capita.

We attempt to capture the presumed modernizing influence of the cinema through a measure of exposure, movie attendance per capita circa 1953, and the measure of the relative supply of Western modernizing influences, % U.S. films circa 1953.[6] By combining the measure of exposure with the measure of supply, we are able to estimate the magnitude of exogenetic, Western modernizing influences carried by the cinema. This is necessary because Portes argues that modernization in contemporary underdeveloped countries is often synonymous with Westernization, which is likely to have adverse consequences for economic growth. We have argued above (Sec. 2) that American-made films are a more likely instrument of attitudinal Westernization than most other films.

Operationally, we measure the presumed exposure to Western influences in the cinema by computing a multiplicative interaction term:

$$\text{Exposure to U.S. films} = (\text{attendance per capita}) \times (\%\ \text{U.S. films})$$

To avoid the problem of multicollinearity usually associated with multiplicative interaction terms, we residualize the interaction term on its component variables.[7] We call the residualized interaction term U. Interaction

[6] One might be tempted to treat the two composite measures (% U.S. films × movie attendance per capita and % non-U.S. films × attendance per capita) as *direct* indicators of attendance at the showings of U.S. films and non-U.S. films, respectively. We avoid such a direct interpretation of these figures because (1) the available evidence indicates that (with the exception of India, Egypt, and Hong Kong) attendance is higher at the showings of U.S. films; % U.S films × movie attendance per capita would thus underestimate direct exposure to U.S. films; (2) neither modernization perspective allows us to predict the effect of exposure to non-U.S. films on economic performance. Portes's perspective addresses the effect of exposure to explicitly Western values. Inkeles and Smith's perspective emphasizes exposure to the media in general.

[7] Frequently regression equations that include multiplicative interaction terms are distorted by the high correlations obtained between the interaction terms and their component variables (see, for example, Gordon 1968). In order to correct for this distortion, we regress the interaction term on its component variables and compute a residualized interaction term. Thus U. interaction′ is regressed on attendance per capita and percentage U.S. films as follows: U. interaction′ $= A + B_1$(attendance per capita) $+ B_2$(% U.S. films) $+ E$. Residuals are then computed: U. interaction = U. interaction′ − [$A + B_1$(attendance per capita) $+ B_2$(% U.S. films)]. This residualizing procedure removes the additive effects of the component variables from the interaction term and thus allows a clearer specification of our regression equation. Since the correlations between the residualized interaction term and its component variables are set to zero by this procedure, the estimation of the effects of the component variables is unaffected by the inclusion of the interaction terms. No information is lost, however, by residualizing. An unresidualized interaction term contains information that is redundant with and inseparable from that contained in the component variables; the residualizing procedure removes this redundancy. Operationally, the residualized interaction term indicates exposure to American films relative to the magnitude of all cinema influences. Thus for example, if the degree of exposure to U.S. films is dispro-

(exposure to U.S. films). For practical purposes the interpretation of the beta and *B* coefficients associated with the residualized interactive measure is no more problematic than the interpretation of usual regression coefficients.

Controls for World-System Position

Some world-system-dependency theorists would argue that cultural dependency—of the form embodied by reliance on imports of American movies—accompanies or follows other kinds of dependency, especially economic (see, for example, Galtung 1971; Elliot and Golding 1974). Thus any apparent effect of exposure to Western films may in fact be an attenuated manifestation of more fundamental (that is, economic) dependencies. The latter, in turn, are said to have a negative effect on economic growth (see Chase-Dunn's [1975] review of the relevant literature; Galtung 1971; Wallerstein 1974). If we did not include explicit controls for economic dependencies in our model, we might wrongly interpret any spurious negative relationship between Western film measures and economic growth as evidence against mainstream modernization theory.

Western films may simply accompany other processed, finished products, which are usually Western. Alternatively, dependent countries that participate in the world system as suppliers of raw materials may be dependent on Western cultural goods because of a structural inability to produce local equivalents. This structural inability would be a consequence of the low level of internal differentiation associated with raw material export specialization. This particular position in the international division of labor may lead to a dependency on Western cultural products that does not necessarily have any autonomous negative effect on economic development. Therefore we include in the model two control variables derived from Galtung (1971) that express two aspects of dependency or position in the world division of labor. Galtung uses a composite measure of dependence based on the relative value of raw and processed exports and imports. We disaggregate Galtung's Trade Composition Index because we wish to examine the relationship between the importation of films and that of other finished products as well as that between the importation of films and the exportation of raw materials. We are not testing Galtung's more general argument concerning the relationship between economic dependence and economic growth. The two measures of world system position—value of finished goods imported circa 1953 expressed as a proportion of the value of all imports (% finished imports) and value of raw materials exported circa 1953 expressed as proportion of the value of all exports (% raw

portionate relative to cinema attendance or to the availability of U.S. films, then a high score on U. interaction results.

exports)—are computed from the *UN Yearbook of International Trade Statistics*.[8]

Controls for Style of Regime

In Section 3 we argue that political factors may influence the relationship between modernizing institutions and economic development. Such influence occurs in at least two ways. (1) Regimes that are oriented toward social mobilization may develop or expand modernizing institutions more than nonmobilizing regimes in order to facilitate overall mobilization efforts. Since modernizing elites usually adopt multifaceted programs of development, the effects of expanded modernizing institutions may be confounded with the effects of other development programs carried on by an activist state. It is necessary to control for state orientation toward mobilization so that the specific effects of expanded modernizing institutions may be separated from the overall (presumably positive) effects of mobilizing regimes. (2) The efficacy of modernizing institutions may depend on the content of the information transmitted by them. Thus, as Portes argues, if these institutions transmit malignant Western values, they may hinder economic development. We hypothesize that mobilizing regimes have a propensity to utilize modernizing institutions to facilitate development (see Sec. 3).

To measure regime orientation toward social mobilization (the political context of regime style), we use a typology of political systems developed by Blondel (1972). Briefly, he specifies three dimensions that can be used to describe political systems: the degree of participation, the degree of authoritarianism, and the degree of egalitarian orientation. State-sponsored social mobilization is most likely to occur in systems that are oriented toward participation and authoritarianism. In Blondel's typology, these include "populist" regimes (for example, Algeria) and "radical authoritarian" regimes (for example, Cuba). We also include as mobilization oriented some "traditional-conservative regimes" (for example, Iran) known to be mobilization oriented. These are included because they are relatively authoritarian *and* because a store of traditional values persists that can be utilized by the state to support its legitimacy and to combat harmful Western values. The inclusion of certain traditional-conservative regimes is consistent with Portes's argument that traditional values are not necessarily antieconomic and that they can be used to mobilize a population in support of development programs (see also Chirot 1977, pp. 172–74). We classify as nonmobilizing regimes liberal-democratic regimes (for exam-

[8] We code exports in SITC categories 0–4 as "raw" and imports in SITC categories 6–8 as "finished." We reclassify according to the SITC scheme all data not given in SITC form before computing the two measures.

ple, India) because they are not authoritarian (see Moore 1967), most authoritarian-conservative regimes (for example, Haiti) because they are not mobilization oriented, and colonial units (for example, Hong Kong). In our analysis mobilizing regimes are coded one; nonmobilizing regimes are coded zero.

Cases

Our statistical analysis covers 49 poor countries. This set includes (1) most of the countries that were independent in 1950 and had a GNP/capita inferior to US$600; (2) colonial administrative entities, most of which were to become sovereign subsequently and which also had a GNP/capita inferior to US$600 in 1950. The countries, according to their mobilizing scores, are as follows:[9]

1. Countries with mobilizing regimes: Algeria, Burma, Cameroon, Cuba, Cyprus, Egypt, Ghana, Indonesia, Iran, Iraq, Jordan, Mexico, Panama, Peru, Syria, and Tunisia.
2. Countries with nonmobilizing regimes: Afghanistan, Angola, Argentina, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Guyana, Haiti, Honduras, Hong Kong, India, Jamaica, Khmer Republic, Morocco, Mozambique, Nicaragua, Nigeria, Pakistan, Paraguay, Philippines, South Africa, South Korea, Sierra Leone, Thailand, Trinidad and Tobago, Turkey, Uganda, and Venezuela.

5. ANALYSIS

The Availability of Modernizing Institutions

To argue that modernizing institutions are used deliberately to further economic development is absurd if, in fact, their expansion requires that poor countries first become rich. Our first task is to establish the availability of modernizing institutions by demonstrating their independence from level of economic development. We do this by regressing the time_2 values (1965) of movie attendance per capita and secondary-enrollment ratio on their t_1 values and on GNP/capita at t_1. If GNP/capita in 1950 has a positive, significant effect on the time_2 values of the dependent variables net of the autoregression effect, the argument that modernizing institutions are relatively free from the constraints of poverty is not supported.

We report the results of these two regression equations in table 2. In

[9] One could protest our assignment of certain countries to a given subsample. When in doubt, we tended to place marginal cases in the larger "nonmobilizing regimes" subsample. Transferring these marginal cases into the smaller mobilizing regimes category, however, does not alter appreciably the findings we present in tables 5 and 6.

TABLE 2

REGRESSION OF STRENGTH OF MODERNIZING INSTITUTIONS AT TIME$_2$ ON TIME VALUES AND GNP PER CAPITA: FULL SAMPLE ($N = 49$)

	INDEPENDENT VARIABLES		
		Strength of Modernizing Institutions at Time$_1$	
DEPENDENT VARIABLE	Initial Wealth (GNP per Capita in 1950)	Movie Attendance per Capita ca. 1953	Secondary-Enrollment Ratio ca. 1953
Movie attendance per capita, 1965 ($R^2 = .720$):			
Beta	.005	.845	...
B	1.834	1.031	..
P	N.S.	00	...
Secondary-enrollment ratio, 1965 ($R^2 = .712$):			
Beta	.086	...	808
B	98.120	...	1 211
P	N.S.	..	.00

both cases GNP/capita in 1950 has no significant effect on subsequent increases in the strength of modernizing institutions. This finding is probably not due to the stability of secondary-enrollment ratio and movie attendance per capita over time; in both cases the autoregression estimate is less than 0.85. This leaves 28% of the variance in the time$_2$ measures unexplained by the autoregression effect.

Modernizing Institutions and Economic Development

To test the argument of mainstream modernization theory that modernizing institutions have uniformly positive effects on economic growth and Portes's argument that exogenous modernizing institutions (carrying harmful Western values) have a detrimental effect on economic development, we regress change in GNP/capita from 1950 to 1970 on (1) log of initial GNP/capita in 1950; (2) percentage of finished imports and percentage of raw exports circa 1953; (3) regime style, 1950–70; (4) secondary-enrollment ratio and movie attendance per capita circa 1953; and (5) percentage of U.S. films and U. interaction circa 1953. The results of this analysis are presented in table 3. Also in table 3 we report the results of a simplified analysis with nonsignificant variables excluded.[10] The correlation matrix used to compute

[10] Following Duncan (1969), one could construct an alternative model that would allow time$_1$ *and* time$_2$ measures of the strength of modernizing institutions to affect economic development at time$_2$. This alternative formulation is unjustified, however, given the time frame of the modernization perspectives. In both perspectives the effects of modernizing institutions on economic development cannot be instantaneous; modernizing institutions affect economic development through their effects on individuals

these equations is reported in table 4. Returning to table 3, note that the strong effect of initial GNP/capita on GNP per capita change (beta = .59) may reduce the potential impact of other independent varables to the extent they are correlated with initial GNP/capita.

The two measures of world-system position were included in the equation to guard against possible spurious effects of the film variables. Their apparent lack of effect on GNP/capita change is of little substantive interest here, since the equation is not intended as a test of world-system theory.

Of our two measures of exposure to modernizing influences, movie attendance per capita and secondary-enrollment ratio, only the latter has an impact on change in GNP/capita (beta = .26, $P = .02$). The positive effect of secondary-enrollment ratio confirms the argument implicit in mainstream modernization theory (Sec. 1), according to which the modernization of individuals contributes to economic progress.[11] This finding has importance beyond this confirmation because, as we have shown, the school is a modernizing institution that is relatively free from the constraints of poverty and underdevelopment.

In spite of the moderately high zero-order correlation with change in GNP/capita ($r = .53$) shown in table 4, movie attendance per capita has no autonomous effect on this variable. This finding is apparently in contradiction with mainstream modernization theory since (1) as we demonstrate in Section 2, the cinema is a good instance of the presumably modernizing mass media; (2) according to Inkeles and Smith, the mass media constitute an important modernizing institution; (3) as we argue in Section 1, according to this perspective, modernizing institutions should contribute to economic progress via the modernization of individuals.

Percentage of U.S. films does not have any significant effect on change in GNP/capita. This is not surprising, since modernization theory emphasizes the role of the modernized individual. Mere measures of the supply of modernizing influences such as % U.S. films do not express the degree of exposure that makes individuals modern. The extent to which individuals have been exposed to the modernizing influence of Western films is captured instead by U. interaction.

(see fig. 1). It is unlikely, for example, that an attempt to improve the quality of the work force via the expansion of secondary education could instantaneously improve economic performance.

[11] A plausible alternative argument might be that secondary-enrollment ratio actually indicates the degree to which national income is concentrated in the hands of a small, mobilized elite. Such income concentration, in turn, may affect economic growth positively at an early stage of development. However, the correlation between secondary-enrollment ratio and income inequality (gini coefficient) is negative, —.36 for the 24 countries in our sample of 49 for which we have data on both measures. The correlation between secondary-enrollment ratio and percentage of national income accruing to the top 5% of the population is —.159 for these same 24 countries.

TABLE 3

REGRESSION OF CHANGE IN ECONOMIC DEVELOPMENT ON MODERNIZING INSTITUTIONS: FULL SAMPLE ($N=49$)

	INDEPENDENT VARIABLES							
		World-System Position			Strength of and Exposure to Modernizing Institutions			
DEPENDENT VARIABLE	Initial Wealth (Log GNP/ Capita in 1950)	% Finished Imports ca. 1953	% Raw Exports ca 1953	Regime Orientation Mobilization Dummy	Secondary-Enrollment Ratio, ca. 1953	Movie Attendance/ Capita, ca. 1953	% U.S. Films, ca 1953	U. Interaction
Change in GNP per capita, 1950–1970 (R^2= .738):								
Beta	586	.044	− 046	056	.255	.042	143	−.215
B	46 899	084	−.059	2 47	61.081	.187	.185	−.101
P	00	N.S.	N.S.	N.S.	.02	N.S.	N.S.	.04
Change in GNP per capita, 1950–1970 (R^2= .719):								
Beta	.652	.	.	. .	.280	. . .	. . .	−.212
B	52.173	.	. . .	. .	67.114	.	. . .	−.100
P	.00	. .	. . .	. .	.00	. .	. . .	.01

TABLE 4

CORRELATION MATRIX FOR EQUATION IN TABLE 3: FULL SAMPLE

	GNPCCHG	LGNPC50	RAW53	FIN53	SECRAT53	ATTCAP53	USAFLM53	UINTER
LGNPC50	.76125							
RAW53	−.36491	−.24033						
FIN53	−.25610	−.13248	.25613					
SECRAT53	.51674	.28689	−.33957	−.41252				
ATTCAP53	.52990	.56182	− 22664	−.16084	.48981			
USAFLM53	36075	.32687	06112	−.21929	.15371	.22502		
UINTER	− 36763	− 13792	.49464	32163	− 23400	.00002	00002	
MOBILIZ	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	

The main finding in table 3 is that U. interaction has a significant negative effect (in the complete analysis beta = —.22, $P = .04$). This finding constitutes strong evidence against the assumption of mainstream modernization theory that modernizing institutions have uniformly positive effects on economic progress. Considered jointly with the positive effect of secondary ratio on GNP/capita change, this finding (1) verifies the causal efficacy of modernizing institutions and (2) supports Portes's contention that this effect is negative when modernizing institutions carry Western values.

It should be noted that this equation includes a control for regime orientation toward mobilization (regime style). Mobilization has no direct effect on economic development. Note also that the correlations between the mobilization measure on the one hand and the measures denoting the reach of modernizing institutions on the other are near zero. Consequently, neither the positive effect of secondary ratio nor the negative effect of U. interaction are indirect manifestations of the influence of mobilization on GNP/capita change. This observation does not exclude the possibility that regime style (mobilization) affects the respective influences of both institutions on economic development by providing the context within which they operate and by determining the content of the messages they disseminate. We now turn to this problem.

Regime Style and Modernizing Institutions

We examine the effects of regime orientation on the relationship between the presumed influence of modernizing institutions and economic development by dividing our set of 49 countries into two groups according to their regime style (see list above). In the first subset are 16 countries with mobilizing regimes; in the second are 33 countries with nonmobilization-oriented regimes. By dividing the set of countries in two, we are able to examine the differential effects on the influences of modernizing institutions in the two sociopolitical contexts.

We do this by recomputing the equation presented in table 3 for each of the two subsets (excluding, of course, the regime-style variable which was used to delineate them). The results of these two regression analyses are presented in tables 5 and 6. We also show in these tables the simplified model we present for the full set in table 3.

The findings in table 5 for the subset of nonmobilizing regime countries are with one exception, similar to those for the full set presented in table 3. The exception concerns the estimate of the effect of secondary ratio, which is nonsignificant here. Only in the simplified equation (bottom of table 5) does this variable show a significant positive effect (beta = .22, $P = .01$). Note, however, that the unstandardized coefficient (B) attached to this

TABLE 5

REGRESSION OF CHANGE IN ECONOMIC DEVELOPMENT ON MODERNIZING INSTITUTIONS: SUBSAMPLE OF NONMOBILIZING REGIMES (N = 33)

DEPENDENT VARIABLE	INDEPENDENT VARIABLES						
	Initial Wealth (Log GNP/Capita in 1950)	World-System Position		Strength of and Exposure to Modernizing Institutions			
		% Finished Imports, ca. 1953	% Raw Exports, ca. 1953	Secondary-Enrollment Ratio, ca. 1953	Movie Attendance per Capita, ca. 1953	% U.S. Films, ca. 1953	U. Interaction
Change in GNP per capita 1950–70 (R^2= 744):							
Beta	.587	−.117	.037	.118	.159	.017	−.297
B	47.262	−.207	.042	27 902	.646	.022	−.138
P	.00	N.S.	N.S.	N.S.	N.S.	N.S.	.03
Change in GNP per capita 1950–70 (R^2= .720):							
Beta	.660	...	..	.220	...	...	−.276
B	53.068	.	..	52.335	..	..	−.128
P	.00	...	..	.01	...	...	.04

TABLE 6

REGRESSION OF CHANGE IN ECONOMIC DEVELOPMENT ON MODERNIZING INSTITUTIONS: SUBSAMPLE OF MOBILIZING REGIMES (N = 16)

DEPENDENT VARIABLE	INDEPENDENT VARIABLES						
	Initial Wealth (Log GNP/Capita in 1950)	World-System Position		Strength of and Exposure to Modernizing Institutions			
		% Finished Imports, ca. 1953	% Raw Exports, ca 1953	Secondary-Enrollment Ratio, ca. 1953	Movie Attendance per Capita, ca. 1953	% U.S. Films, ca. 1953	U. Interaction
Change in GNP per capita, 1950–70 (R^2 = .926):							
Beta	.583	.281	−.120	.543	−.057	.142	−.035
B	46.325	.711	−.248	133.845	−.344	.179	−.018
P	.01	N.S.	N.S.	.00	N.S.	N.S.	N.S.
Change in GNP per capita, 1950–70 (R^2 = .775):							
Beta	599	...	...	.449	...	...	−.028
B	47.624		...	110.663	...	...	−.014
P	.00	...	...	.01	...	...	N.S.

variable is not as large for this subset of nonmobilizing countries as it was for the full set (52 vs. 67). For both the full and simplified equation in table 5, the estimates of the effect of U. interaction are slightly stronger than ($B = -.14$ vs. $-.10$ and $-.13$ vs. $-.10$, respectively) and as significant as they were in table 3, covering the full set.

Table 6 shows the corresponding findings for the subset of countries with *mobilizing regimes. These findings differ markedly from the findings for* nonmobilizing countries, and they differ appreciably from those for the full set.[12] First, the estimates of the effects of secondary ratio (standardized as well as unstandardized in both the full and simplified equations) are double the full set estimates in table 3. Second, the regression estimates attached to U. interaction are reduced to approximately zero. Note that the nonsignificance of the estimates is probably not due to the small size of the set of mobilizing countries included in this equation. In its simplified form, with only three independent variables, the smaller estimates for U. interaction remain nonsignificant.

Comparison of tables 5 and 6 leads to a clarification of the three-cornered relationship between modernizing institutions, mobilization, and economic development: a mobilizing regime enhances the contribution of education to economic progress, while counteracting the detrimental influence of the Western cinema. These findings confirm the idea that modernizing institutions are politically available rather than mechanistically progressive.

Admittedly, our measure of regime style is crude, and the dichotomy we have employed (mobilizing versus nonmobilizing) may obscure other meaningful dichotomies. However, most alternative explanations would support a direct association between this dichotomous measure and economic development. One could argue, for example, that our measure of regime orientation conceals some aspect of world-system position. In this event, however, one would observe a direct relationship between this measure and economic development. But the estimate of the effect of the mobilization variable on change in GNP/capita in table 3 is small and nonsignificant. Furthermore, this variable has a low correlation with both measures of world-system position (see table 4). Therefore this alternative explanation is unsupported. Instead, the influence of mobilization is seen to operate in conjunction with modernizing institutions of a decidedly noneconomic nature.[13]

[12] The estimate of the effect of percentage of finished imports on change in GNP per capita in this equation seems large and positive, though not significant at the .05 level. Its lack of significance in this equation is not due to the small number of countries included. When percentage of finished imports is included as a variable in the simplified equation, its effect remains nonsignificant.

[13] A cultural relativist's alternative interpretation might be that societies enjoy different levels of inherent cultural "immunity" to the noxious consequences of exposure to

6. DISCUSSION

This study confirms and qualifies some of the predictions of mainstream modernization theory while contradicting others.

Education furthers the economic progress of poor countries. However, we cannot say whether the positive effect of the school operates through the individual-level mechanisms whose existence has been documented by Inkeles and Smith on one hand and Portes on the other. What we do know is that education is an institution relatively free from the usual constraints of underdevelopment and therefore should be eminently responsive to voluntaristic intervention. In most underdeveloped countries, the state appears as the most likely agent of such intervention. Thus, whatever attitudes and skills the school may instill, it is a secular institution that may be used to mobilize individuals for political and economic action. The magnified influence of education on economic development under mobilizing regimes (but not under others) suggests that this may be precisely how the school is used.

Note that this perspective neither confirms nor denies the individualist, social-psychological hypotheses of mainstream modernization theory. Education may further economic progress by instilling modern attitudes in individuals and by serving as an instrument of political manipulation.

Some of our findings are clearly contrary to the most straightforward implications of mainstream modernization theory; they support instead Portes's argument that to the extent modernization is Westernization, and only Westernization, it retards economic progress.

We argue that the cinema, according to the logic of modernization theory, is a modernizing institution of prime importance as far as underdeveloped countries are concerned. Yet simple exposure to this institution (attendance per capita) seems to have no effect on economic growth. The time span involved (15 years) is surely not responsible for this poor showing, since other factors have a considerable effect on economic growth over the same period. Exposure to specifically Western films appears to impede economic growth to a considerable extent. This is contrary to the expectations of mainstream modernization theory since (1) Western films should have the greatest modernizing impact because of their "modern" content and (2) the modernization of individuals is expected to contribute to economic progress. This finding, however, is fully consistent with Portes's speculation that some of what passes for modernization (actually Westernization) is what underdeveloped countries need least. He argues

Western values. A glance at the list of countries with mobilizing and nonmobilizing regimes, however, does not suggest that the dichotomous regime-style variable we use in this study conceals a hypothetical distribution of such immunity. It would be difficult to specify what presumptive basis for immunity is shared by Ghana, Iraq, and Panama, for example.

in particular that the modernization of individuals contributes to the emigration of skilled persons and stimulates prematurely high levels of personal consumption.

These arguments become especially pertinent when the probable location of movie theaters is taken into account. In most poor countries the urban segment of the population is disproportionately reached by the cinema and its normal Western fare. But in such countries the urban population is most likely to be integrated into monetary circuits. The cinema-modernized, urban population is therefore the most likely to be tempted by emigration and the most capable of engaging in excessive consumption. Therefore, if Portes's description of the economic consequences of individual modernization is correct, these consequences should be most apparent where the cinema is a modernizing agent.

Our findings illustrate Portes's third and most important objection to the intrusion of Western attitudes, that they interfere with mass mobilization. This argument has the merit of explaining at once the positive effect of the school and the negative effect of the cinema. Following Deutsch (1953), Portes argues that a Western mental orientation diverts energies from mobilizing ideologies. He notes that there have been few escapes from underdevelopment since the end of the 19th century that have been unaided by the nonmaterial incentives offered by mobilizing ideologies. These ideologies have ranged from socialism to the cult of the emperor in Meiji Japan.

Portes's concern for concerted mass mobilization is paralleled by Myrdal's arguments concerning "hard" and "soft" states. Myrdal (1970, pp. 208–9) suggests that the key difference between poor countries is their softness and hardness rather than their placement on a right-left continuum. Chirot (1977, pp. 207–8) amplifies this view by suggesting that soft societies are characterized by their openness to the outside world, while hard societies are relatively closed. Hardness and closedness are, in this perspective, two facets of a single response to the social indiscipline found in all strata of such societies. (Note that such social indiscipline may be either primeval or an indirect product of imperialism.)

Our study does not show explicitly the effect of hardness and closedness. However, to the extent that mobilizing regimes are hard regimes, our findings suggest in what manner closedness may operate. Mobilizing (hard) regimes are able to mitigate the deleterious consequences of the exposure of their constituencies to Western values. They achieve this by successfully manipulating their home-grown institutions (for example, the school). Our findings do not illustrate directly how these manipulations proceed. However, many of the countries that we classify as having mobilizing regimes are known for their "puritanical, nationalistic" (Chirot 1977, p. 208) ideologies (for example, Algeria, Cuba, Egypt, Iraq, Peru, Syria). The

function of these ideologies can be explicated via an examination of O'Dea's and Myrdal's arguments concerning the disjunction between modernizing means and ends in the underdeveloped world. O'Dea (1971, p. 177), citing Myrdal, introduces an important distinction between the relationship of means to ends in Asia and the same relationship in the region of classical development. Practically all regimes share a proclaimed faith in modernization as a goal. However, in Asia the "spiritual basis for modernization" is "derived and borrowed" (ibid.). This explains, according to O'Dea, the hesitation of elites to implement modernization objectives and the reluctance of the masses to embrace modernization programs.

The idea of a disjunction between modernizing means and ends in Asia probably can be generalized to the Third World as a whole. If O'Dea's view is correct, the theoretical problem posed by the relationship between the nature of modernizing institutions and economic development must be radically reformulated: the principal obstacle to self-generated development is not the traditionalist orientation of either masses or elites, as mainstream modernization theory seems to imply. Instead, the main obstacle is the inadequacy of modern attitudes as means to modernist ends, among which economic development looms large. Accordingly, the advancement of the culturally diverse countries of the Third World does not require the uniform establishment of modernizing institutions aimed at the eradication of traditional mentalities. A central part of the modernization process entails, instead, the selection of the proper weapons from an arsenal that includes "traditional" ideologies (for example, the cult of the emperor), "modern" ones (for example, Marxist-Leninism), as well as many syncretic ones (for example, Arab socialism).[14]

[14] Note that we do not address the question of possible compatibilities between traditional national cultures and the requirements of a mobilizing ethos (which might explain why a particular ideology is selected over others), nor do we speculate on the differential capacities of national cultures to contribute to syncretic mobilizing ideologies. Such theorizing lies outside the purview of this study, and our findings do not suggest that it would be fruitful. We believe, with Portes, that mainstream modernization theory has systematically underestimated the variety of elements within traditional cultures. Yet our findings point to a confirmation of Inkeles's (1969, p. 225) contention that the efficacy of modernizing institutions is not sensitive to cultural context (though of course we do find that their effects are not uniformly positive). The influences of modernizing institutions are modified instead by regime style. One could argue that regime style in turn may be affected by the content of specific national cultures. However, the list in Sec. 4 and data in tables 5 and 6 do not support this supposition. Such culturally diverse traditions as those of Algeria, Cameroon, Burma, Iran, Cuba, and Peru allow for the emergence of mobilizing regimes that presumably share an ability to constrain the actualization of Western values. We are inclined to believe that all traditional cultures offer elements that can be utilized to construct or to prompt the adoption of a mobilizing ideology, though we are not able to sustain this countersupposition empirically. Possibly, national cultural traditions are reflected in the outer forms of an ideology while its central message is constant across mobilized societies. According to this perspective, one may agree with Chirot (1977, p. 208) that "communism" best exemplifies mobilizing ideologies and speculate that its explicit

The common denominator of these varied and often contradictory ideologies is their stress on individual frugality and collective effort. The findings reported in this study sustain the instrumental importance of this normative orientation. The parading of Western images in the cinema which celebrate the rugged pursuit of individual self-fulfillment may well deter the state-sponsored actualization of such values, as the school may further it. This study suggests that "hard" (that is, mobilizing) states enjoy a superior capacity to carry out the mission of fostering a normative orientation that facilitates economic development.

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Urban-to-Rural Diffusion of Opinions in Contemporary America[1]

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Recent studies indicate continuing cultural differences between residents of larger and smaller communities. This paper argues that these differences persist because they are constantly generated anew. Innovations emerge in metropolitan centers and diffuse from them to smaller places, so that there is always a gap between the two. Predictions from this model are evaluated with poll data, covering 20 years, on changing social attitudes. The results are consistent with the predictions. Patterns in the rates of opinion change indicate a hierarchical diffusion model in which early adoption and/or rate of diffusion are positively associated with community size.

A debate continues in the sociology of community on whether cultural differences among settlements of varying sizes are vanishing in modern societies. One position is that differences in beliefs and behavior between residents of large and small communities are nil or soon to be nil (e.g., Sjoberg 1964). The other view is that cultural differences persist because they are continually generated anew by some intrinsic feature of urbanism. A specific thesis of this kind is that cities concentrate and intensify "deviant" subcultures. These subcultures produce and then disseminate innovative cultural forms within the cities and from the cities to less urban places. Yet, even as the new forms are being accepted in the hinterland, the cities are breeding still further innovations (Fischer 1975; Glenn and Alston 1967, p. 399). The conclusion is that "cultural differences between urban and rural persons are persistent" (Fischer 1975, p. 1336).

Previous research has provided some evidence for the latter position. There are notable size-of-community differences in beliefs and opinions even in contemporary America (urbanites are less conservative than rural

[1] This research note has been substantially abbreviated from a longer report (Fischer 1977), available at cost from the Institute of Urban and Regional Development, University of California, Berkeley, California 94720. The work was supported in part by a small grant from the University of California, Berkeley, Committee on Research, and by the Institute of Urban and Regional Development I want to thank Professors Sidney Verba and Ann Swidler of Harvard University for providing access to the data and the Roper Center for its assistance. Helpful comments were provided by Paul Burstein, Ron Burt, Elihu Gerson, and Alan Pred, but they are not responsible for remaining errors.

persons) that cannot be completely explained by individual characteristics such as age or education.[2] And a few studies suggest that size-of-community effects have not been declining here (e.g., Willitis, Bealer, and Crider 1973 and Williams, Nunn, and St. Peter 1976). This paper builds on the earlier studies in two ways: first, it outlines a model explaining why and how urban-rural cultural differences are continually generated; second, it evaluates that model—in a preliminary way—by predicting and testing patterns of changes in attitudes within populations of varying urbanism. Early in the history of an opinion's development, acceptance increases more rapidly in large than in small communities; late in its history, acceptance increases more rapidly in smaller places. The model is elaborated from a "subcultural" theory of urbanism (Fischer 1975, 1976). The evidence consists of aggregate results from Gallup Polls (AIPO 1954–1976) for over 20 years: the distribution, by size of community, of individuals' responses to various questions on moral and social issues.[3]

A MODEL OF PERSISTENT URBAN-RURAL CULTURAL DIFFERENCES

Elsewhere, I have presented a theory of why urbanism tends to generate innovation and unconventionality. The variety of population types in cities, their concentration at "critical masses," and their intermingling foster invention and the propagation of invention (Fischer 1975, 1976). Here I will briefly expand the analysis to the diffusion of cultural inventions. The form of diffusion to be considered is hierarchical, the "flow" of innovation from one community to another in proportion to the communities' functional importance (strongly related to size).[4]

Just as the probability of innovation is associated with urbanism, so is the probability of its adoption and, therefore, the course of its diffusion. This happens for two reasons. (1) Personal contact and network density both within and between communities increase with size.[5] (2) The probability that a subculture receptive to and nurturing of the innovation exists is also associated with community size. Thus, diffusion from the community's innovative subculture to "conventional" groups will commence sooner in the larger cities. The process of diffusion does not mean that in the end the urban-rural differences disappear. Diffusion takes time. During

[2] Two recent studies are Glenn and Hill (1977) and Roof (1976; see review in Fischer 1976, pp. 192–97).

[3] For this paper, I have not sought to partition effects and trends any further than by time and community size or to allocate effects to any other factors.

[4] The other general form of diffusion is the neighborhood effect, in which diffusion spreads across space from its origin.

[5] For example, a resident of Los Angeles has a greater chance of contact with someone from Chicago than with someone from Springfield (holding distance constant).

that time, yet newer forms, some perhaps in direct contradiction to the prior ones, are emerging in and diffusing from the urban centers.

Some of these ideas about urban-to-rural diffusion are commonplace—with regard to material items. It is generally accepted that innovations tend disproportionately to diffuse from urban centers to their hinterlands. In this paper I apply these ideas to the diffusion of cultural items. It is also generally accepted that adoptions of an innovation follow an elongated S-shaped growth curve. If we combine this information with the theoretical argument that diffusion is associated with urbanism, we can derive a model such as that illustrated in figure 1.

The horizontal axis represents time; the vertical axis marks the percentage of a given population that has accepted an innovation, which in this paper means endorsing a particular opinion. The solid curves represent different populations distinguished on theoretical grounds: people living in large urban places, those in small urban places, and those in nonurban places. By assuming that the three populations are of equal size—which is roughly true in the data described below—we can simply average the values for the three groups to obtain the national percentage, the unweighted mean. That is represented by the broken line. Finally, three "periods" have been arbitrarily designated in terms of the national mean.
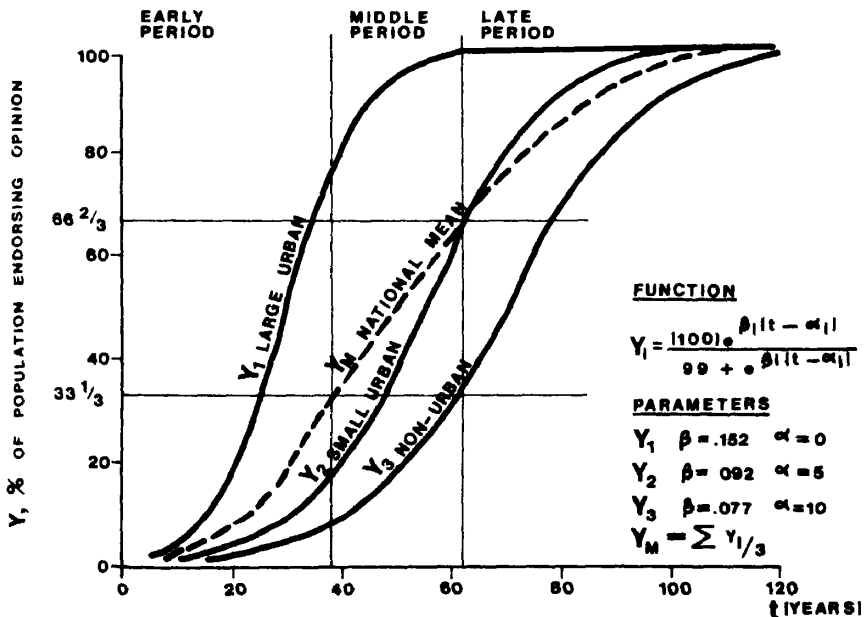
FIG. 1.—Hypothetical curves showing diffusion of opinion within communities of different sizes.

The period when less than one-third of the nation endorses the opinion is called "early"; the period between one-third and two-thirds acceptance is "middle"; and the last period is "late." (Does this process lead irreversibly from 0% to 100% acceptance? No; see discussion later.) Below, specific issues are placed within each period.

The particular values and shapes of the curves are arbitrary, but their general features are both fundamental to diffusion processes and validated with material diffusion.[6] Figure 1 suggests that new opinions emerge first and fastest in the more urban places, and urban-rural differences widen, but that, as acceptance becomes virtually complete in urban areas, it accelerates in less urban places, and differences narrow.

With a sufficient number of data points, one could simply fit the facts to the formulas and assess the adequacy of the model. However, data of that kind are not available. In order to do some curve fitting, I abstracted the logistic curves of figure 1 into line segments in figure 2. I broke up the curves according to the periods identified before and drew straight lines approximating those sections of the curves. This procedure simplifies the analysis. It does so without losing information since, as it turns out, most of the opinion time series examined below fall clearly into one of the three periods. And this procedure permits some relatively specific predictions, to wit: (*a*) early in the spread of an opinion, the rate of adoption is positively associated with urbanism; (*b*) in the middle period, the rate is curvilinearly related to urbanism, with the most rapid acceptance occurring in the moderately urban categories; and (*c*) late in the spread of an opinion, the rate of diffusion is inversely associated with urbanism.

DATA

The data are simple cross-tabulations of answers to Gallup Poll questions by size of respondent's community. I examined the Gallup Poll from 1965 through 1976 for questions that tapped the domain of social beliefs and behaviors, indicating people's adherence to, or deviation from, traditional views and mores.[7] The chosen items were traced as far back as 1953. The

[6] The function of fig. 1 is drawn from Coleman's (1964, p. 43) logistic eq. 4.4. The populations are differentiated on both parameters—rate of diffusion and starting point—thereby assuming that the less urban a population, the later the process starts and the slower it proceeds. I believe that either assumption would suffice for the purposes of this theory and this paper. (Note that the specific parameters have been picked only for illustration.)

[7] The questions must also have been repeated in virtually the same form at least three times. I then examined the Roper Public Opinion Center files for instances of these questions before 1965. For those items in series that appeared promising, special cross-tabulations were ordered from the center. (I could not use items administered in polls before 1953 because the community-size categories were changed then.) The reader will note that the items listed in table 1 are by no means a random or necessarily
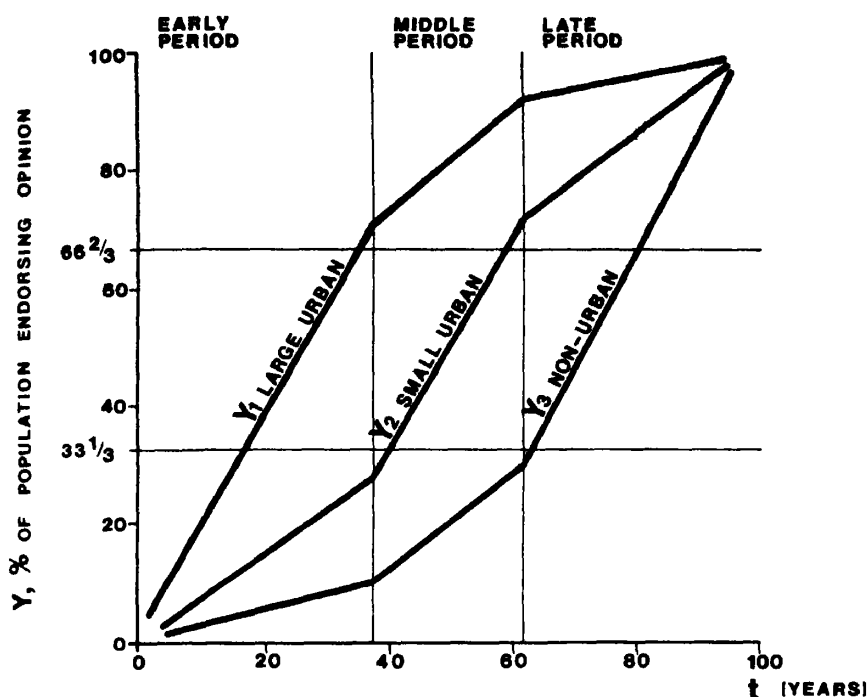
FIG. 2.—Line segment abstraction of fig. 1

final set used is indicated in table 1. For all but the last two, the national trends have been "liberal," and each item falls into one of the three periods. Three categories of community size were defined: large urban—cities over 500,000 and their suburbs; small urban—cities 2,500 to 499,999 and their suburbs; and nonurban.

ANALYSIS

For each item, I calculated the best-fitting regression equation within each community-size category. The dependent variable is the percentage of agreement with the liberal, advancing attitude, and the independent variable is year (minus 1900). The regression coefficient (b), the slope, represents the rate of change for each community type; it is the relative sizes of those slopes, in different periods, that the model predicts.[8] (In the longer

representative sample of the specified domain. They form a judgmental sample. However, they were chosen before examining the fit of the model.

[8] Two statistical issues should be noted. (1) The regression equations are intended only as a means of easily drawing the best-fitting line through a set of points. There is no attempt here to explain variance, only to compare trends. (2) The question of sig-

TABLE 1

COMPARISON OF OBSERVED SLOPES TO PREDICTIONS (% AGREEMENT REGRESSED ON YEAR)

Issue	Years	N Observations	Average Annual Change LU	SU	NU	Ntl	Prediction Confirmed?	Mid-Year Estimate (%)* LU	SU	NU	Ntl
Early period. Prediction $b_L > b_S > b_N$:											
Agree marijuana should be legal	1969–74	3	3 1	2.9	2.6	2.8	Yes	24	16	11	17
Do not believe in life after death	1957–68	3	.8	6	.2	.5	Yes	24	14	9	16
Middle period. Prediction $b_L < b_S > b_N$:											
Admit to drinking alcohol	1954–74	6	.3	.6	.2	.3	Yes	79	63	49	64
Would vote for Negro for president	1958–71	7	1.9	2.9	1.8	2.2	Yes	65	51	44	54
Approve abortion for deformed child	1962–69	3	1 7	.8	.6	1.1	Mixed	62	59	53	59
Late period. Prediction $b_L < b_S < b_N$:											
Would vote for divorcee for president	1961–67	4	1.8	2.0	2.8	2.2	Yes	82	82	75	80
Approve dissemination of birth control information	1959–64	3	1.4	1.3	1.7	1.5	Mixed	79	76	71	75
Reversals and static opinions (no predictions):											
Oppose the death penalty	1965–76	6	−1.2	−1.3	−2.1	−1.5	..	36	39	38	38
Support requiring gun permits	1965–72	5	.3	−.5	−.1	−.1	.	82	72	61	71

NOTES.—b = slopes. Predictions derived from fig. 2; see text. b_L = regression coefficient for large-urban populations; b_S for small urban; and b_N for nonurban. LU = large urban places; SU = small urban places; NU = nonurban places; and Ntl = unweighted mean of three size-of-place categories, approximately equal to true national average.

* Percentage of agreement, for middle of the series of years, as estimated from the best-fitting regression line.

paper, I derive, test, and support predictions for equation intercepts as well.) Table 1 summarizes all the results. The opinions are organized by period, with the appropriate predictions noted.

The results for the three periods largely support the model. The patterns of differences among slopes fit the predictions fully in five cases and partly in two. Rates of opinion change differ by degree of urbanism, and those differences, in turn, vary by the historical period of the opinion. This is a complex pattern, but one predicted by the model.[9] In addition, the figures on the right-hand side of table 1 show the familiar, cross-sectional differences by community size: the larger the community, the more likely that a "progressive" opinion is supported.

Two items in table 1 do not fit the periodization used here. The first seven represent social issues for which there has been a consistent, liberal trend, and urbanites lead the way from traditional consensuses to new consensuses. But, on many issues, there is not a simple movement from one pole of opinion to another. Instead, social changes alter the process, reversing the trend of opinion or blocking it. It is difficult to make straightforward predictions about intercommunity differences in such cases. Depending exactly on relative rates of diffusion of the nontraditional "wave" versus those of the revivalist "counterwave," one derives different specific curves and predictions (see Fischer 1977). All we can observe, for now, is that patterns of change for such opinions are unlike those of the periods discussed earlier but not inconsistent with the present model.

The death penalty issue is a case of reversal. Opposition to the death penalty almost doubled from 25% in 1953 to 47% in 1966, but dropped back afterward to 28% in 1976, no doubt under the impact of rising fear of crime. We pick up the trend from 1965 through 1976 in table 1. The results there prove little, except in a negative way: the pattern is indeed different from the prior ones. The element most inconsistent with the present model is the rapidity with which opposition to the death penalty dropped in the most rural places. The gun permit issue is an instance of stasis (and also related to fear of crime). From 1965 through 1972 there was virtually no secular change in national opinion on whether people should be required to have permits (—0.1% change per year). We do

nificance tests is a difficult one. On the one hand, many of the equations described below have fewer than five units of analysis—no comparisons would be significant. On the other hand, each observation is based on at least 250 cases. In the end, I decided to present the data without such tests and allow the replication of patterns of results to attest to substantive significance.

[9] In 1977, Gallup asked both the marijuana question and the alcohol question again. Entering the data for the former accentuated the predicted pattern. The slopes are then 2.7, 2.4, 1.6 by size of place. The data about alcohol are more complex, but also consistent (see Fischer 1977).

observe substantial size-of-community differences, but no noticeable patterns of change.

These two items indicate that the original model is indeed a simplified one and that when public opinion ebbs and flows the patterns of community differences become much more complex. However, where we see a clear national trend, the community components of the change tend to fit the model.

CONCLUSION

The results basically support the model of urban-to-rural diffusion presented earlier, according to which opinions diffuse down the urban hierarchy at a decelerating rate. There are, to be sure, limitations to this analysis.[10] Also, alternative explanations can be offered.[11] However, when this study is added to earlier ones, the findings point persuasively to the underlying theory: cultural innovations are born in subcultures of large urban centers, from which they spread to other social worlds in those centers and to communities beyond. The pattern of intercommunity diffusion is also a function of urban size, so that new cultural forms flow "down" the urban-rural continuum, just as do material innovations. This is a continuing process; there are always new opinions, beliefs, and behaviors spreading from the urban centers in the wake of those that went before. What is perhaps most striking about this study is that cultural differentiation of communities by size continues in modern America—telecommunications, highways, national corporations, and "mass society" notwithstanding.

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[10] (1) The sample of issues was judgmental. That this study is confirmed in a few respects by others is, however, encouraging. (2) Replications across time are imperfect, but this should have introduced random error. (3) I could not decompose the trends by race, region, age, and other such variables (see Fischer 1977).

[11] In the longer version, I discuss two general alternatives: one, that the structural causes of opinions—rather than opinions as autonomous cultural forms—diffuse from urban to rural; and two, that urban and rural persons vary in receptivity to innovation.

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Token Women: An Empirical Test of Kanter's Hypothesis[1]

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In this article we test empirically Rosabeth Kanter's hypothesis that minority achievements are diminished by the underrepresentation of minority persons in majority-dominated work groups. Using data on male and female achievements at two law schools with significantly different sex ratios, we find evidence that performance pressure, social isolation, and role entrapment all operate to diminish the achievements of women law students where they are only a small minority of the student body.

In a recent theoretical article, Rosabeth Kanter (1977) states that one important determinant of achievement by minority-group members is contextual—the significant factor being the ratio of minority to majority persons in the group. The more isolated the minority group members are among majority persons, the less likely they are to be high achievers by group standards. Conversely, the higher the proportion of minority members in a group, the more likely they are to be successful. Kanter focuses particularly on the effects of group proportions upon female achievement in male-dominated work groups. Our purpose is to test empirically, with data drawn from a study of law students, Kanter's hypothesis about the effect of sex ratios on female achievement. A comparison of female students in a male-dominated law school with female students in a law school where the percentage of women is higher may reveal whether sex ratios significantly affect female achievement.

Kanter (1977) begins with a continuum for describing group compo-

[1] This paper is one of a series from the Law Student Activity Patterns Project of the American Bar Foundation. We would like to thank the following people for support and comments: Myra Ferree, Mary L. Ford, Spencer Kimball, Felice Levine, Michael Lewis, Charles Page, Peter Rose, and the members of the American Bar Association's Special Committee for the Study of Legal Education.

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sition. At one extreme are uniform groups which consist of "only one kind of person, one significant social type" (p. 966). At the other extreme are balanced groups, composed of roughly equal numbers of all the "social types" in the group. Between uniform and balanced groups are skewed and tilted groups. For skewed groups, those whose minority/majority ratio is approximately 15:85, Kanter hypothesizes, minority-group members are perceived less as individuals than as tokens. Further, token status is associated with three conditions which, in turn, generate responses detrimental to the minority member's performance.

First, token persons are highly visible. They lack privacy; they are overobserved; they have fewer "backstage" areas than dominants. Visibility generates disproportionate performance pressure. A token person's performance is likely to affect not only his or her own personal advancement but also the future acceptability of other members of the minority group. This creates performance demands which may be met by overachieving. On the other hand, the token person may be encouraged to allay resentment and fears about competition by performing at relatively low, nonthreatening levels. Whichever option is chosen, people in token status do not perform under the same conditions as dominants.

Second, the presence of tokens leads to polarization within the larger group. Those characteristics which distinguish the token person from the members of the dominant group come to be perceived as highly salient, even if they are irrelevant to task performance. Polarization thus leads to the isolation of token members of the group, especially at informal moments.

Third, the individual characteristics of token persons are distorted or misperceived to bring them into line with stereotypes about the token's group. The imposition of stereotypes on individuals leads to role entrapment. The only behavior which is accepted from tokens is that which conforms to the stereotype. Tokens can break out of this role entrapment only if they are willing to challenge their peers' expectations at almost every meeting. Often it is easier to accept limited and traditional opportunities.

Minority members have greater representation in tilted groups, where the minority/majority ratio is typically 35:65. Kanter hypothesizes that tilted groups exhibit many of the same dynamics as skewed groups, but to a lesser extent. Minority performance in tilted groups will be more like that of majority members, more sensitive to individual differences in ability and motivation.

Kanter proposes three specific hypotheses which can be related to an academic setting and tested by comparing the performance of women students at a law school with few women (skewed group) with that of women at a school where they constitute a larger proportion of the student popula-

tion (tilted group). The hypotheses are that women in the setting with fewer female colleagues (1) will do less well academically (performance pressure), (2) will be less integrated into the law school (social isolation), and (3) will be more likely to demonstrate traditional female preferences in study strategies and career choices (role entrapment).

METHOD AND DATA

The present analysis is part of the Law Student Activity Patterns project, one of the studies in legal education of the American Bar Foundation. Questionnaires were group administered to randomly selected samples of law students at seven law schools during 1975–76. The total sample size of 1,370 represents a response rate of 70%. (For a more detailed discussion of the sample and study design, see Pipkin [1976].) In this paper we will use data from two of the seven schools, labeled school A and school B.

School A is a public law school serving primarily residents of the state in which it is located; women constitute 33% of the student body. In Kanter's terminology, this school has a tilted sex ratio. School B is a private school, nationally prominent, and generally regarded as among the most prestigious in the country; women constitute 20% of its student body. School B has a skewed sex ratio.

Control for School-related Differences

Schools A and B differ in many respects in addition to sex ratios. Respectively, one is public, the other is private; one is a commuter school, the other a residential school; one is nonelite, the other is elite. Consequently, differences between female achievements at these two schools may stem from sources other than differing sex ratios. School differences which are not gender related, however, should affect both sexes equally. For example, women at school B join law-school groups more often than women at school A. However, this datum tells us little about relative female isolation because students generally join groups and clubs more often at a residential school such as school B than at a commuter school such as school A. By subtracting male scores from female scores on salient variables within each school, we can create difference scores that reflect only sex-related differences and control all other school-related sources of variation. In the example, if the male rate for group joining is subtracted from the female rate at each school, the resulting difference score reflects only the impact of the sex ratio on group joining. School differences will be evident in raw percentages presented in the tables. The difference score, however, is free from school effects.

Finally, it should be noted that difference scores can be seen as measures of polarization: if the difference score on a variable is larger (irre-

spective of sign) at the school with the skewed sex ratio than at the one with the tilted sex ratio, Kanter's hypothesis that tokenism gives rise to polarization is supported.

Variables

Performance pressure.—In this study, performance pressure is assessed through (*a*) grades earned, (*b*) volunteering to speak in class, and (*c*) contemplation of withdrawal from school. We hypothesize that each of these performance measures would be adversely affected by performance pressure. Grade-point average is computed by converting the grading systems at each school into a four-point scale (excellent, good, average, below average). Volunteering to speak in class is measured by the response (always, frequently, sometimes, never) to the question, "How frequently do you volunteer to speak in class?" Contemplation of withdrawal is simply scored yes or no from responses to the question: "Have you ever given any real consideration to withdrawing from law school?"

Social isolation.—We use two indicators of social isolation. The amount of leisure time spent with other law students (low: 0%; medium: 1%–50%; high: 51%–100%) reflects integration into or isolation from the informal sociability of the law school. Membership in extracurricular groups (from none to four groups) is a measure of formal integration into law-student life.

Role entrapment.—Role entrapment is measured in two ways: (*a*) by the extent to which female students employ study strategies that minimize their contact with the almost entirely male faculties and (*b*) by the extent to which women select traditionally "feminine" practice areas. For the first, we use the question: "When you have trouble understanding an issue raised in class how frequently do you (1) ask the professor to discuss the issue further in class; (2) ask the professor about it directly after class; (3) ask the professor about it later outside of class?" Responses were coded as sometimes or never. For the second, respondents' three choices of practice interests were grouped into four categories: corporate law; general practice; public law; and "feminine" specialties. (The specialties included in each category are listed in table 3.) Practice interests are not mutually exclusive; every respondent has a score for interested or not interested in each work category.

FINDINGS

Performance Pressure

Table 1 supports the hypothesis that in a law school with a skewed sex ratio women's performance differs from men's to a greater extent than in a

TABLE 1

Performance Pressure on Women in Law School

	Tilted Sex Ratio Law School A	Skewed Sex Ratio Law School B
	Grades, % Distribution	
Excellent:		
Women	8.9	9.3
Men	5.9	16.7
Difference score (W−M)	3.0	−7.4*
Above average:		
Women	39.3	32.6
Men	42.6	40.0
Difference score (W−M)	−3.3	−7.4*
Average:		
Women	42.9	44.2
Men	38.6	37.5
Difference score (W−M)	4.3	6.7*
Below average:		
Women	8.9	14.0
Men	12.9	5.8
Difference score (W−M)	−4.0	8.2*
	Volunteer to Speak in Class, % Distribution	
Always:		
Women	1.4	1.7
Men	2.4	0
Difference score (W−M)	−1.0	1.7*
Frequently:		
Women	24.6	6.8
Men	24.2	10.4
Difference score (W−M)	4	−3.6*
Sometimes:		
Women	50.7	42.4
Men	52.4	55.2
Difference score (W−M)	−1.7	−12.8*
Never:		
Women	23.2	49.2
Men	21.0	34.4
Difference score (W−M)	2.2	14.8*
	Thought Seriously about Withdrawing from Law School, % Responding Yes	
Women	30.4	2.2
Men	30.5	18.3
Difference score (W−M)	− 1	10.9*
N(W/M)	193 (69/124)	213 (59/154)

Note.—The percentage distribution for grades is based on N excluding first semester students. N(W/M) for School A = 157 (56/101); for School B = 163 (43/120).

* Probability ≤ .05.

school where women are a larger proportion of the student body. The effect of the sex ratio on women is revealed by the larger difference scores for every measure of performance pressure at school B. In general, performance pressure seems to hinder women's achievement when they are tokens. There is no evidence in table 1 of compensatory overachievement.

At school A, with a tilted sex ratio, a larger percentage of women than men earn outstanding grades, and fewer women than men do unsatisfactory work (table 1, top). At school B, with a skewed sex ratio, the opposite is true: men outperform women. In this setting, the achievements of token women (who were previously high achievers as evidenced by their admission to an elite law school) seem to be inhibited by performance pressure. The relationship between sex and grades at school B remains statistically significant even when controls for ability—Law School Admissions Test scores—are introduced (data not shown).

Performance pressure also appears to inhibit women students with regard to the anxiety-producing activity of volunteering to speak in class (table 1, middle). Here again, the difference between men and women is greater at the school with the skewed sex ratio and apparently works to the detriment of female students. Women at school B are considerably less likely than men to speak in class. Fully half of them never volunteer to speak.

The meaning of performance pressure for token women is also reflected in the bottom part of table 1, which displays data on possible withdrawal from law school. At law school A virtually the same percentage of women and men have thought seriously of withdrawing. At law school B, more women than men give serious consideration to quitting school.

Social Isolation

Table 2 displays data on the polarization process that is hypothesized to isolate women in male peer groups. The difference scores in both parts of table 2 show that in each instance the differences between men and women are greater where sex ratios are skewed—where women are tokens—than where they are tilted.

The data on informal peer integration reveal the complexity of token women's responses to their positions. At school B, with a skewed sex ratio, a greater percentage of women than men is at the extremes of the scale: more than half of the women show either high or low peer integration, while more than half of the men show medium peer integration. Although these differences do not attain statistical significance, they are in the direction hypothesized by the theory and substantively consistent with all our other findings. At school A there are no substantial gender differences in peer integration.

TABLE 2

SOCIAL ISOLATION OF WOMEN IN LAW SCHOOL

	Tilted Sex Ratio Law School A	Skewed Sex Ratio Law School B
	Informal Peer Integration, % Distribution	
High:		
Women	13.0	37.3
Men	16.1	31.8
Difference score (W−M)	−3.1	5.5
Medium:		
Women	59.4	45.8
Men	63.7	60.4
Difference score (W−M)	−4.3	−14.6
Low:		
Women	27 5	16.9
Men	20.2	7.8
Difference score (W−M)	7.3	9.1
	Formal Peer Integration, % Distribution, *N* Law School Groups Joined	
None:		
Women	71.0	55.9
Men	86.3	78.6
Difference score (W−M)	−15.3*	−22.7**
One:		
Women	21.7	22.0
Men	12 1	13.0
Difference score (W−M)	9.6*	9.0**
Two or more:		
Women	7 2	22.1
Men	1 6	8.4
Difference score (W−M)	5.6*	13.7**
N(W/M)	193 (69/124)	213 (59/154)

* Probability ≤.01.
** Probability ≤.001.

These data bear on the point of compensatory "overachievement." It appears that there are, in fact, two groups of women at law school B—those who choose not to associate informally with their classmates at all and those who spend most of their leisure time with fellow law students. It is likely that the women who spend none of their leisure time with their peers are avoiding potentially friction-laden situations. Conversely, women who spend almost all of their leisure hours with law students seem to be overcompensating for their relatively isolated position in the law school. This pattern does not change when information on marital status is introduced (data not shown).

The information on formal peer integration (table 2, bottom) parallels the data on informal socializing. The differences between men and women are greater at the skewed sex ratio school. Further, some women at school B

seem to ward off potential social isolation by joining groups in substantial numbers, while many others shun school voluntary associations entirely.

Together, the data from tables 1 and 2 suggest that some token women make their way in an unreceptive environment by emphasizing sociability while others deemphasize the competitive aspects of their group membership.

Role Entrapment

Table 3 displays the most relevant data on role entrapment. Women in the skewed sex ratio law school have trouble interacting with their teachers. At school B, women are much less likely than men to ask professors for clarification of troublesome issues. Their reticence, in turn, lends credence to and helps perpetuate the stereotype that depicts female law students as less professionally committed than male ones (Fasteau 1971; Schwartz 1976; Wallach 1975). At school A, in contrast, faculty are approached by students of both sexes more often, and male and female students show no significantly different patterns in this behavior.

An aspect of role entrapment of perhaps greater consequence is reflected in the data on interest in areas of professional specialization. In general these data (table 3, bottom) support the hypothesis that token status may lead to a long-term role entrapment, although they also reveal the impact of school prestige on law careers. At school A (a nonelite law school in which women enjoy substantial representation) women are as interested as men in general-practice careers, show a somewhat greater willingness than men to accept public sector law employment, and differ significantly from men only in their acceptance of the low-status, low-paying "feminine" practice specialties (Carlin 1966; Laumann and Heinz 1977). At school B (an elite law school with few women), women are significantly less likely than men to choose the lucrative, high-prestige corporate law specialty and significantly more likely than men to choose public law. Women at school B also opt for the lowest-status practices embodied in "feminine" specialties significantly more often than men at school B, but much less often than women at School A.

These data reveal the contradictory pressures of tokenism and school prestige. Women at both law schools are likely to have less prestigious and lucrative careers than men from the same school (Epstein 1970; White 1967). The sex differences in achievement will probably be greater among graduates of the elite law school. Nevertheless, elite credentials do confer career advantages upon women. They steer women away from the lowest-status rungs of their profession. The absence of elite credentials leaves women from "local" law schools relatively open to recruitment into just these low-status specialties.

TABLE 3

ROLE ENTRAPMENT OF WOMEN IN LAW SCHOOL

	Tilted Sex Ratio Law School A	Skewed Sex Ratio Law School B
	% Responding Never to "When you have trouble understanding an issue raised in class, do you . . ."	
Ask the professor to discuss the issue further in class?		
Women	21.7	55.9
Men	21.0	40.8
Difference score (W−M)	.7	15.1**
Ask the professor about it directly after class?		
Women	21.7	44.8
Men	21.0	28.8
Difference score (W−M)	.7	16.0**
Ask the professor about it later outside of class?		
Women	43.5	74 6
Men	39.5	61.4
Difference score (W−M)	4.0	13.2**
	Area of Specialization, Indicating an Interest as a Career Choice	
Corporate law:		
Women	40.6	47.5
Men	47.6	59.1
Difference score (W−M)	−7.0	−11.6**
General practice:		
Women	63.8	40 7
Men	66.1	37.0
Difference score (W−M)	−2.3	3.7
Public law:		
Women	55 1	62 7
Men	42.7	44 2
Difference score (W−M)	12.4	18 5*
"Feminine" specialties:		
Women	55.1	28 8
Men	25 0	16 2
Difference score (W−M)	30.1**	12 6**
N(W/M)	193 (69/124)	213 (59/154)

NOTE.—Professional specialization areas are constituted as follows: corporate law general practice for corporate or wealthy individuals, house counsel for corporation, tax law, international law; general practice general practice for small businesses and middle income individuals, criminal law (defense), insurance law, real estate and property, land use and planning, public practice criminal law (prosecution), administrative law, public interest law, judiciary, government, municipal law, civil rights; "feminine" specialties probate and estate, family law, juvenile law, poverty law, educational law, mental health law.

* Probability $\leq$.05.

** Probability $\leq$.01.

CONCLUSION

We began this paper by proposing an empirical test of a theoretical insight recently offered by Rosabeth Kanter: the performance of individual minority-group members in majority groups is affected by the proportion of minority members in the group. We compared the performance of women

law students at two law schools which have different sex ratios—in Kanter's terms, one law school is a tilted sex ratio group, the other a skewed sex ratio group. Kanter's specific hypotheses are supported by our data: women and men in the skewed sex ratio group differ in performance to a greater extent than women and men in the tilted group. Our test suggests strongly that the dynamics of tokenism are operating in law schools and that, as Kanter argues, they operate to the detriment of women where they have low proportional representation in the student population.

Our data bear directly upon another aspect of tokenism discussed by Kanter, who hypothesizes that women use two contrasting strategies—underachievement and overachievement—in responding to the dilemmas of tokenism. Our study suggests that in the law school setting some token women underachieve in competitive work-related tasks, some overachieve in social tasks. The choice of strategy is probably not random. Further study of the determinants of over- and underachieving among tokens is clearly needed. To this end, the impact of several other factors—the substance of ascriptive minority stereotypes, the nature of the task itself, and the organizational context in which work is performed—should be systematically explored.

We would have liked to conclude with the optimistic observation that rapidly increasing female enrollment in law school will make the problem of tokenism among women law students one of merely historical interest. And indeed the data do warrant optimism. At a school where women are a sizable percentage of the student body, female students' performances are generally indistinguishable from males'. However, our optimism must be tempered by the fact that at the more prestigious law school—one whose imprimatur gives access to the higher ranks of this highly stratified profession—women are tokens, with all the attendant disabilities that Rosabeth Kanter has called to our attention.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

COMMENT ON JONES'S "ON UNDERSTANDING A SOCIOLOGICAL CLASSIC"

Jones's essay (*AJS* 83 [September 1977]: 279–319) states his conception of the desiderata for sound intellectual history. The historian should consider the "social" setting of the works he or she analyzes and not only the linguistic meaning of texts but also the author's (say, Durkheim's) *intended* contributions or innovations, which the historian may identify by studying the somewhat fluid polemical field of opinions of which Durkheim is known to have been aware. "Influences" are narrowly defined—for example, to have been influenced by preceding writers, Durkheim must have been clearly aware of their ideas. Further, ". . . statements about Durkheim must fall within and make use of the range of descriptions which he could have applied to what he was doing . . ." (p. 288). Although it may be important that Durkheim suggested something interesting to later writers of whom he never heard, nevertheless, according to Jones, it is dubious intellectual history to speak of Durkheim's contributions to or anticipations of symbolic interactionism, for instance, or to the developing Parsonian general theory of action. Such claims, it is suggested, are "mythology" designed perhaps to "legitimize" the later theories. Equally

dubious, allegedly, are discussions of "convergence" between Durkheim and, say, Weber. Such claims, it is said, rest on highly selective reading, give a false impression of integrated thought, and, furthermore, are unlikely to gain general agreement. Above all, they violate "rigorous" principles of the writing of history. A justification given for sticking to rigorous principles is that histories produced in accordance with them give us knowledge of the creative process itself.

This conception of history writing may for convenience be called the subjective approach, with subjective referring to the point of view of the subject, say, Durkheim. An apparently rigorous procedure, however, may be too narrow. "Understanding" texts in Jones's sense may be useful, but it is definitely not enough for the history of sociology or any other science. In the history of science, the influences a writer exerts are just as important as the influences upon him. A text's linguistic meanings and its author's intentions do not exhaust its objective significance. By objective, here, I mean "from the point of view of an observer (say, the historian), as opposed to that of the subject (say, Durkheim), an observer with a wider knowledge of the history and development of thought than his or her subject and/or reader had or has." Sociologists will probably continue to examine Durkheim's texts (for example) with new questions in mind, suggested by new frames of reference, and will continue to say important and true things (as well as false things) about these texts, even though Durkheim himself might not have been able to make exactly the same comments. A good example is Parsons's "double take" on the "sociology of knowledge" aspect of *Elementary Forms*. Durkheim (like Jones) does not make a careful distinction between "social" and "cultural," but objectively Durkheim does deal with religion in the context of cultural, social, and psychological systems in the analytical senses of those terms. Having noticed that *Elementary Forms* is not only a "sociological" classic, Parsons can now observe:

> What I take to be his [Durkheim's] basic theorem is that human society and the cultural framework of the human condition, including knowledge, have evolved concomitantly from a common basis and, in relatively advanced stages of sociocultural development, have come to be differentiated from each other. This conception of common origin is very different indeed from a one-way conception of determinism, namely, that of society as an independently existing entity, determining the nature of the organization of knowledge. This, of course, has been the common sense of what might be called the vulgar sociology of knowledge, of which Durkheim most definitely was not a proponent. [1973, p. 158]

Concerning a "great" text in social science, we might well wish to know its objective contributions to theory of the time and to later theory or theories, why these contributions should be considered important, the text's

inconsistencies and gaps, and its unacceptable and acceptable implicatioı (some of which the author him- or herself may well have been partiall or totally unaware of)—all this in addition to the state of science at tl time it was written and its author's intended contributions.

As I mentioned above, Jones's conception of influence is too narrow (s< pp. 293–94). There are many ways of influencing other writers, amor them at least the following: (1) presenting a concept, a theory, or method that can be seen (by the writer being influenced) to have furth< implications or uses; (2) presenting ideas that can be seen to fit in a context of significance of which the author may never have dreamec (3) presenting data that are not completely accounted for (thus posir a challenge); and (4) posing a problem and then proposing somethir correct, incorrect, or incomplete by way of solving it. Further, if A directl influences B in any of these ways and B influences C, then it is not i correct to say that A has indirectly influenced C (even though C ma never have read A and may not mention him).

Because in writing history at any given moment we cannot foresee in portant objective influences of these types, history must be rewritten agai and again, to keep it not only up to date but also "vital" (interestir to living scientists). Further, there may well be many objective chains (influence and many convergences.

Space does not permit my giving illustrations for all these types of ii fluences and contributions. Durkheim's *Division of Labor,* being concerne with "mechanical solidarity," "organic solidarity," and the famous noı contractual aspect of contract, definitely made a contribution to the the retical "problem of order," whether or not Durkheim himself ever use that expression. He made a further significant contribution to the proble of order in his *Elementary Forms,* with his fundamental insight that a herents to a unified system of beliefs and practices with regard to "sacrec things compose a "moral community." From this idea it is a perfect legitimate step to Bellah's concept of civil religion, dealing as it does wi an important logical implication of Durkheim's idea, namely, that if "society" in Parsons's technical sense composes to a significant extent moral community embracing adherents of different traditional religion then there should be other "sacred things" in which they "believe" ar with regard to which they engage in rituals. These sacred things might we be called, in full keeping with the plain meaning of Durkheim's text, "coı stitutive symbols" (Parsons's term), that is, a *cultural* basis for the veı identity of the society. Thus, Durkheim's treatment of religious symbo becomes an objective contribution to a logically and empirically base classification of socially and culturally significant symbols, partly coı tained in but also going beyond ordinary speech (see Parsons and Pla

1973, p. 17). Further, these legitimate extensions of Durkheim are an important part of the objective significance of *Elementary Forms*, whether or not Durkheim ever used the term "civil religion" or ever had a clear or unclear concept of "society" corresponding to that of Parsons.

It is also an objective fact that there is convergence in important respects between Durkheim and Mead. The objective fact of this convergence does not depend at all on Durkheim's (or Mead's) awareness of it. In order to establish the convergence that also objectively exists among Durkheim, Weber, Pareto, and Marshall, one must, of course, have competent interpretations of what "convergence" Parsons had in mind, as well as competent interpretations of Durkheim, Weber, Pareto, and Marshall. It is highly irresponsible "instant sociology or psychology of knowledge" (as I call it) to refer to such important connections in the history of science as mythology or as mere attempts to legitimize later theory.

Of course Durkheim never heard of Parsons or Bellah, and *of course* Parsons and Bellah have gone beyond Durkheim in certain respects. As Merton points out, we stand on the shoulders of giants, and they were giants partly because *they* stood on the shoulders of giants (1965). Discovering what I am calling "objective" influences, contributions, and convergences does, of course, require judgment on the part of the historian and the scientific community in which he or she participates. That such judgments are sometimes difficult to make and that agreement may not at any given moment be complete may well be true, but these controversies and judgments cannot and should not be avoided. Rigorous principles that lead to neglecting them or requiring that one brush them off are simply too narrow and unrewarding. We should combine the subjective and the objective points of view. Otherwise, we risk burying ourselves in pedantic, myopic antiquarianism. Understanding classics, by itself, does not even tell us a great deal about the process of creation, although it does provide important data that, together with other data and appropriate analysis, creative theorizing, and testing, might eventually yield knowledge about the creative process. In any case, we should not confine attention narrowly to explicit "meaning" and conscious intentions but should note, examine, and evaluate connections not necessarily foreseen or intended by the contributors we study—cumulative chains, networks, and gradually shifting complexes of expanding and more discriminating objective significance.

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SUBJECTIVITY, OBJECTIVITY, AND HISTORICITY: A RESPONSE TO JOHNSON

The rather polemical tone of "On Understanding a Sociological Classic" is ample testimony to its intended purpose, which was less to convert historians of sociological thought to an intentionalist perspective than to arouse them from their dogmatic slumbers. Bearing a healthy respect for the functions of intellectual conflict, I thus anticipated substantial opposition, particularly from Marxists, psychohistorians, and structuralists, whose interests would seem to be very much at stake. Unfortunately, Professor Johnson's comment serves less to elevate the level of scholarly debate in the history of sociological thought than to document the intransigence of its current orthodoxy.[1]

The primary difficulty is simply that Johnson has not taken his critical responsibilities very seriously. He has availed himself of none of the philosophical, methodological, or substantive sources upon which my paper is based and has studiously ignored our more prominent critics as well.[2] Nor has he followed my own argument with any degree of intellectual discipline. The result is less a penetrating criticism than a ludicrous parody. In response, therefore, it is necessary to restate, at a still more primitive level, arguments which are quite abstract and esoteric at their source.

An initial difficulty with Johnson's comment is that it manifests the common tendency among sociologists to place more weight upon their own meanings and intentions than upon those of their subjects (see below). Since there are those who ignore the importance of subjective intentions altogether, I should thus begin by observing that they constitute an in-

[1] This response does offer me the opportunity to correct the one statement in "On Understanding a Sociological Classic" which I regret. On page 311, I indicated that "the difficulty" with leaving the history of sociological thought to professional historians rather than sociologists is simply that "it is so blatantly counterfactual." This is not at all the difficulty, of course, as my stated admiration for the work of Coser and Lukes, for example, attests. I am grateful to Werner Cahnman for pointing out this ill-considered remark, as I am for his helpful comments throughout this project.

[2] See, for example, Tarlton 1973, Leslie 1970, Mew 1971, Parekh and Berki 1973, and Schochet 1974.

eluctable subject matter for sociology *and* history, particularly when our concern is with an activity as self-conscious as the writing of sociological theory (see Skinner 1969, p. 44; Jones 1977, p. 295, n. 19). Weber thus included in his notion of action "all human behavior when and in so far as the acting individual attaches a subjective meaning to it" (1947, p. 88); the phenomenologists stress that the aim of the social sciences must be to gain "insight into the meaning which social acts have for those who act" (Schutz 1960, p. 203); and the followers of Wittgenstein emphasize that the forms of activity studied by social science will be those "of which we can sensibly say that they have a meaning" (Winch 1958, p. 45; see also Skinner 1972*b*, p. 136). If only to indicate how traditional many of our intentionalist arguments are, however, it is sufficient to cite only the most common undergraduate text in historical method, which states unequivocally that "the historian's task is to understand not only what a document's words may formally mean but also what his witness *really intended to say*" (Gottschalk 1950, p. 134). To resist such injunctions is to run the same risk as the behaviorist whose purpose it is to show that human action can be explained without reference to human purpose.

Johnson, of course, is not so resistant; rather, he argues that the appeal to intentions is "too narrow" and, in the absence of his "objective" corrective, may bury us in "pedantic, myopic antiquarianism." Johnson's mistake here is to assume that I have offered an intentionalist perspective as the sole and sufficient approach to the study of classical social theory. I have specifically denied such a view (1977, p. 291), as has Skinner: "I see no impropriety in speaking of a work having a meaning for me which the writer could not have intended. Nor does my thesis conflict with this possibility. I have been concerned only with the converse point that whatever a writer is *doing in* writing what he writes must be relevant to interpretation, and thus with the claim that *amongst* the interpreter's tasks must be the recovery of the writer's intentions *in* writing what he writes" (1972*a*, p. 405). Such a commitment in no way denies the value of other approaches to the study of classical social theory or the writing of its history, insofar as these are accurately characterized and successfully rendered; but it does represent a new determination to write the history of sociological thought as the history of a real, social activity. My essay was thus about *understanding* a sociological classic; and its interdictions exclude only those practices which hinder, vulgarize, or subvert this pursuit.

One of the key elements in this enterprise is Skinner's notion of "privileged access" (1969, pp. 28–30; Jones 1977, p. 288), which Johnson mistakenly assumes would exclude the possibility that an "objective" observer could give a fuller or more convincing account of the actions of the historical agent. This is not the case; indeed, as Skinner has stated (1969,

p. 28) and I have noted (1977, p. 288, n. 12), psychoanalysis is literally based upon this possibility. But another interesting fact about psychoanalysis is that "in the course of psychotherapy, Freudians try to get the patient himself to recognize the validity of the proffered explanation, that this indeed is almost a condition of its being accepted as the 'right' explanation" (Winch 1958, p. 48). Unlike the psychoanalyst, of course, the historian is denied personal interaction with his or her "patient"; but it remains at least a logical impossibility for an agent to be said to have meant or done something which he or she could not, at least in principle, have accepted as an accurate account of what he or she was saying or doing (Skinner 1969, p. 28; 1972*a*, p. 405; see also Hampshire 1959). This does not mean, Johnson's vulgarization notwithstanding, that we are limited to the words Durkheim specifically used. But it does mean that "any agent is obviously in a privileged position when making statements about the correct characterization of his own intentions and actions." And it follows that "it must always be dangerous, and ought probably to be very unusual, for a critic to override a writer's own explicit statements about what he was doing in a given work" (Skinner 1972*a*, p. 405).[3] Finally, it means that when the "objective" historian says that Durkheim "meant" and "was trying to convey" a message whose terms were available only to the former, as Parsons in fact does (1975, p. 106; 1973, p. 160–61), we should suspect the presence of an anachronism.

Perhaps the most serious disability of Johnson's comment is his distinction between the "objective" and "subjective" approaches to the study of the history of sociological thought. My own dictionary defines "objectivity" as follows: "In scientific research the quality that is expressed in the effort to eliminate distortions in perception or explanation due to the relatively temporary social or psychological biases of a group or individual, and to achieve the most unbiased and universal generalizations possible under the conditions of knowledge at the time. In an objective analysis, one utilizes the best available theoretical framework and methods of observation" (Theodorson and Theodorson 1969, p. 279). The reader of "On Understanding a Sociological Classic" will recognize that this was precisely my goal in that essay.

"For convenience," however, Johnson uses the term "objective" to refer to history written from the standpoint of the historian and "subjective" to refer to that written from the standpoint of the historical agent; he then calls for a combination of these two allegedly discrete perspectives. It is a distinction, of course, which no historian would accept; indeed, it is reminiscent

[3] For this reason, my substantive discussion of the *Elementary Forms* placed considerable emphasis on "Le Problème religieux et al dualité de la nature humaine," Durkheim's defense of that work before the Société française de philosophie just months after its appearance. This *séance* deserves a translation and considerably more attention.

of Durkheim's vacuous observation that the distinctive characteristic of the sacred and profane is their absolute heterogeneity (see Stanner 1967, p. 232; Lukes 1972, p. 24–28). For it is commonplace that historians, like all scientists, always write from the standpoint of the present, must base their judgments entirely upon presently existing documents, and necessarily impose their own, present criteria of what is rational, significant, and interesting. I therefore emphasized (1977, p. 282–83) that "presentism" is not the problem, for we (not Durkheim) write the history, and also that "paradigmatic preconceptions" are not the problem, for they are unavoidable.

But these commonplace observations in no way relieve the historian of the responsibility to understand past phenomena, insofar as it is possible, in their own terms—that is, in terms of the system of thought and action of which they were a part (Murphey 1973, p. 120). Admittedly this is difficult, but that does not make it less necessary. The particular difficulties I have emphasized, of which Johnson's argument itself provides a striking instance, are the historian's familiarity with and commitment to present preconceptions (1977, p. 284); and the particular controls I have introduced are specifically intended "to eliminate distortions in perception or explanation due to the relatively temporary social or psychological biases of a group or individual" (see above). Despite Johnson's invocation of the old bromide that history must be rewritten each generation, therefore, it is these difficulties which require rather than dismiss the methodological argument I have made. Notwithstanding the fashionable attitude that history consists of "stories,"[4] an additional feature of historical stories is that they are supposed to be true (Skinner 1969, p. 29).

Venturing blithely into the parlous realm of historical explanation, Johnson criticizes my essay on the ground that the term "influence" has been "narrowly defined—for example, Durkheim must have been clearly aware of the influences preceding writers had on him." To paraphrase Russell's charming remark on Descartes's "cogito ergo sum," rarely has a sociologist made more mistakes in fewer words. I proffered no "definition" of the term "influence" at all; on the contrary, I simply tried to determine, from the understandably vague and insipid usages of the term by others, its empirical content. I concluded from these usages that the term was intended to convey some sense of causality. This is certainly true, for example, of each of the four usages which Johnson commends to us, his obviously unexceptional case of "indirect" influence, and Newton's remark about the "shoulders of giants" as well.[5] Otherwise, I am at a loss to see why the term is introduced at all. But if some degree of causality

[4] For interesting discussions of the implications of history as narrative, see Mandelbaum (1967) and Murphey (1973, pp. 122–24).

[5] The remark, incidentally, was made at least as much to pacify Robert Hooke as to provoke sweeping images of "cumulative chains."

is thus implied, and explanatory power thus claimed, such discussions must adhere to at least a few of the minimal criteria of historical explanation. I have described what these are; and despite Johnson's misrepresentation it is awareness "of the antecedent party or idea" (Jones 1975; 1977, p. 293; Skinner 1966, pp. 199–215; 1969, pp. 25–26; Jones 1943), *not* of the alleged causal connection, which is among the standards in question. Finally, Johnson's appeal to the "judgment" of the historian here is of no assistance whatever; for judgment without some standard is no judgment at all, there being no standard by which to judge the judgments.

Johnson "illustrates" his more objective historiography with the prosaic observation that Durkheim "definitely made a contribution to the theoretical 'problem of order.' " Since the only textual warrant here is Johnson's emphatic insistence that it is so, it is worth noting that Anthony Giddens (1972) has come to precisely the opposite conclusion. At the risk of suggesting that the emperor has no clothes, however, my own request is for a clear and precise statement of just what the "problem of order" *is:* a statement general enough to stand as something to which Hobbes, Durkheim, and Parsons may be said to have "contributed," yet specific enough to count as a description which Hobbes, Durkheim, and Parsons would each accept as a reasonable account of what they were doing. And were such a statement fothcoming, we might still ask whether Hobbes's, Durkheim's, and even Parsons's solutions to this "problem" still carry force in the present, a question which seriously challenges the traditional justification for doing the history of ideas altogether (see Jones 1977, p. 295; Skinner 1969, pp. 52–53). Finally, it should not be assumed that *any* of these questions can be settled empirically; for behind them lurks the more formidable philosophical question of what we mean by the "same" idea (Winch 1958, pp. 26–31).[6]

The most curious aspect of Johnson's comment, however, is its very source. As one of the few scholars to truly master the "most secret workings"[7] of the Parsonian system, Johnson, in his defense of Parsons (like Parsons in defending himself), has typically appealed to what the latter meant, intended, and was doing: The appeal is a clear assertion of an author's privileged access to his own meanings and intentions. The question of what is "pedantic," "myopic," and "antiquarian" thus seems to be one of whose intentions are being gored.

But there is a difference, of course, in that Durkheim is no longer in

[6] It is observations of this kind which have led Foucault to his stunning dismissal of the focus on the historical continuity of ideas altogether (see *The Archaeology of Knowledge* [New York: Pantheon, 1972]).

[7] The phrase, of course, comes from Durkheim's famous advice to a student, which describes his own study of Renouvier. Johnson's similarly extensive knowledge of Parsons is evident in his admirable *Sociology: A Systematic Introduction* (1960).

a position to utter such assertions. It is at least a part of the historian's task, therefore, to see that such access is "logically retained." What initially appears as a methodological injunction is thus, in a quite different sense, a moral responsibility as well. Otherwise, the history of sociological thought will indeed remain a bag of tricks we play upon the dead.

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Review Essay: Creating Culture

Culture and Its Creators: Essays in Honor of Edward Shils. Edited b
Joseph Ben-David and Terry Nichols Clark. Chicago: University of Chi
cago Press, 1977. Pp. x+325. $15.00.

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Over the years, Edward Shils and I have, as it were, listened to very dif ferent drummers; it therefore gives me all the more pleasure to review thi distinguished festschrift published in his honor. Although, as the editor say, "every part of the book deals with problems related to [Shils's] worl and uses concepts derived from it," no other common thread runs througl these pages. I shall therefore consider several individual papers in som detail while dealing only briefly with others that seem to me of periphera interest.

Among the outstanding contributions is a paper by the Cambridge an thropologist Jack Goody, entitled "Literacy, Criticism, and the Growtl of Knowledge." Laying to rest the cultural relativism still prevalent amon many social scientists with its sentimental contention that all cultures ar of equal stature, Goody argues that systems of communication and intel lectual processes in nonliterate cultures differ qualitatively from those o literate ones. Writing, so he argues, makes possible a kind of scrutin of discourse which, in turn, increases the scope of critical activity, ra tionality, skepticism, and logic. Literacy, as the French sociologist Pierr Bourdieu has argued recently in a similar vein, "detaches cultural resource from persons" and, hence, frees them from the narrow limits of individua memory. Arguing along lines that seem partly related to Basil Bernstein' work, Goody contends that nonliterate cultures tend to be closed system since purely verbal modes of communication are tied to an "occasion," whereas in written forms of communication thought can become abstract depersonalized, formalized, transcending "occasions," and hence subjecte to a type of impersonal critique that is unavailable in nonliterate societies Philosophical and logical discourse becomes possible only when writin allows the formalization of propositions that are abstracted from the con crete flow of speech. Goody states that he does not intend to introduc another single-factor theory and that he is aware that "the social structur behind the communicative acts is often of prime importance" (p. 242) But he does contend, and has persuaded this reviewer, that difference between open and closed forms of communicative systems, or betwee literate and nonliterate cultures, point to significant differences in people' capacity to store, evaluate, and augment knowledge, as well as difference in the quality of culture and interpersonal relations.

The essay entitled "Patrons, Publishers, and Prizes: The Writer's Estate in France," by Priscilla P. Clark and Terry N. Clark, while not breaking novel ground in the manner of Goody, is still a valuable contribution to the sociology of culture. Contrasting the traditionally high standing of French writers to the lesser estate of writers in America, the authors suggest that it is not exceptional prestige accorded by the general public, or extraordinarily high sales of books as such, that accounts for the preeminence of men and women of letters in France, but a combination of four major factors: institutionalized and traditional patronage (in the Académie française and elsewhere), a well-developed and highly publicized system of literary prizes, established professional associations of writers, and an extensive network of publishing houses, some of which make efforts to disdain the forces of the market. The essay provides a wealth of detailed documentation and is indispensable for anyone interested in the sociology of intellectual life.

Joseph Ben-David's paper, "Organization, Social Control, and Cognitive Change in Science," counters the recently voiced criticism of the established sociological view which considers science as an institution governed by a set of norms. The critics, Ben-David argues, have failed to distinguish between what may often be the relatively normless behavior of individual research scientists, heads of schools, or disciplinary innovators and the institutionalized control mechanisms of science. Individual investigators or teams of researchers may well lack such qualities as "disinterestedness" or "skepticism"; they may be highly partial to their brain children and lack "objectivity," but this has little to do with the norms of scientific evaluation. In fact, it is precisely the effectiveness of the social control of science which makes it possible for researchers to roam freely among their data and use their imagination and intuition. They know that they will eventually be judged by strict and impersonal norms.

The scientific community that emerged in the 17th century institutionalized social control mechanisms within science that made subsequent research endeavors feasible. Science as a moral community, so Ben-David argues, has a logic and structure of its own that interacts with, but is not determined by, the cognitive contents of science and the organization of research. The ways in which scientific research is organized and supported, to be sure, have a selective influence on the cognitive contents of science through the emergence of distinct schools or paradigms with vested interests and built-in resistances to new ideas, or through straight monopolistic practices. But all this, the author contends, differs fundamentally from the influence exerted on the investigator by the moral community of science of which he or she is a part. The cognitive content of science has undergone tremendous changes since the 17th century, but the institutional aim of the scientific revolution of the 17th century, the establishment of science as a distinct intellectual activity controlled by its own norms, has stood the test of time. Revolutionary discoveries have dotted the history of science since that time, but there has been no further institutional revolution when it comes to science as a moral community. This

paper will, no doubt, become a main point of repair in the continuing controversies surrounding the sociology of science.

In "Province and Metropolis: The Case of Eighteenth Century Germany," Reinhard Bendix provides a welcome illustration of Shils's well-known distinction between the center and the periphery of societies and their cultural modes. German backwardness and political fragmentation made for the fact that, in contrast to England and France, a German middle class of merchants and manufacturers was slow to develop, so that the most active segment of educated commoners consisted disproportionately of civil servants, teachers, and clergymen. These strata were excluded from courtly society with its Frenchified culture and attempted consequently to develop their own cultural standards and life-styles. They supported a variety of "moral weeklies" which preached the virtue of "bourgeois" thrift, honesty, and modesty while criticizing the frivolities of courtly society. They upheld the status honor of the educated commoner and his sense of calling but never questioned the social and political hierarchy. By the end of the century, educated commoners had developed a distinctive life-style and cultural orientation. Their intellectual spokesmen, among whom one finds most of the best-known representatives of the great age of German letters and philosophy, had by then accepted an emancipatory program in the world of spirit, yet they continued to cling to the rejection of the political values that had spread from the other side of the Rhine. Wishing to realize the cultural goals of the Enlightenment without paying the cost of revolutionary political upheaval, they accepted the existing balance of power, while transcending it "spiritually." Middle-class identity had emerged in the periphery and in opposition to the courtly center, but that periphery continued to accept the political guidance of a center which it culturally rejected.

Saul Bellow's delightful and irreverent essay on "Writers and Literature in American Society" casts a cold eye on contemporary literary culture and its public. He argues that the accounts of human existence given in most contemporary literary works are shallow indeed. Writers and poets have become "truffle-hounds"; they analyze and dissect instead of giving free reins to the resources of their imagination. Bellow argues that if modern culture is not to die "through full immersion in the great noise" (p. 194) and clutter of the cultural apparatus, writers should not consent "to limit imagination by committing [themselves] to the formulas of modern intelligence" (p. 196).

I shall have to deal with the remaining papers in a more cursory fashion Raymond Aron and Seymour M. Lipset, Shils's old companions from the end-of-ideology wars, return to this theme in their contributions. Aron is largely self-critical in his stance. He now believes that liberals are given to ideological thought just as much as true believers in secularized religions "Modern societies evidence an extreme and irresistible penchant for ideological controversy" (p. 11). Lipset, however, eschews self-criticism in favor of self-justification. He shows at tedious length that the end-of-ideology theme can be found among radical and leftist writers of the fifties

just as much as among his own ideological brethren. Perhaps, but he ne glects to point out that his cothinkers advanced their theses with gloatin satisfaction about the beneficial effects of the decline of ideology, wherea most of the leftists and radical thinkers he quotes reacted with a sense o despair to what they perceived as the conformity and lack of vision o that period.

S. M. Eisenstadt contributes a wide-ranging essay on the sociologica tradition and its patterns of innovation as well as crisis in recent years It defies a short summary. In any case, the book by Eisenstadt and Cure laru, from which this was largely excerpted, has already been publishe roughly a year ago. The late Harry G. Johnson contributes a rather snig gering essay in which he reports with glee that the assault of radical econ omists on the received verities of the profession of economics has bee repulsed successfully so that economics is again back on the old stand Morris Janowitz analyzes the recently emerged split between the old bree of "objective" journalists and the Young Turks of "advocacy journalism" in terms of their divergent views of the professional role of modern journal ists. Clifford Geertz's essay on charisma and the symbolics of power, usin materials from Elizabethan England as well as from Java and Morocc as illustrations, argues that the charisma of dominant figures in a societ and the inherent sacredness of central authority are among the perennia master fictions by which any social order, no matter what its ideologica aims, must live. Talcott Parsons contends in the concluding essay tha apart from obvious economic interests, the growth of higher education an research in America is largely due to the twin influences of the Protestan ethic and of the high prestige of European academic-elite culture.

Uneven as its contributions inevitably are, this book is an intellectua treat and a befitting tribute to the eminence of the man it is designed t honor.

Review Essay: The Theoretical Polemics of Anti-Ideology

Center and Periphery: Essays in Macrosociology. By Edward Shils. Chicago: University of Chicago Press, 1975. Pp. xliii+516. $19.50.

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Readers of this *Journal*, of all people, will be familiar with Professor Shils's characteristic virtues. He is a Grand Theorist, firmly and self-consciously within the great sociological tradition (allying himself with Thomas, Park, Tönnies, Simmel, Weber, and Durkheim), but he is distinctive in that his theorizing has always been overtly, and often aggressively, politically committed. He is both a central figure within Standard American Sociology—Parsons's collaborator and a structural functionalist to the bone—and a high-level ideologist (though not according to his definition of "ideology") of "pluralism" and "civility," and the rest of the liberal, Cold War package of the fifties and early sixties. He is both an academic high priest—at the very least a cardinal—of the late sociological orthodoxy and a campaigning propagandist, *engagé* if not *enragé*—unless it be against those intellectuals who are ideological (according to his definition), that is, adherents of "intensely espoused value orientations which are extremely *seinstranszendent*, which transcend current reality by a wide margin, which are explicit, articulated and hostile to the existing order" (p. 5). (Needless to say, his definition of ideology is itself self-evidently ideological.) He is, in short, both an abstract and generalizing theorist and a concrete and particularizing polemicist. Readers of his subtle and perceptive *The Torment of Secrecy* (1956) and of this volume of largely theoretical essays might well suspect that his forte lies in the latter, not the former, direction—though this very suspicion doubtless springs from a view of what makes for good theorizing that he would reject.

What cannot be doubted is that he is an intellectual (according to his definition)—a member of the class of "persons with an unusual sensitivity to the sacred, an uncommon reflectiveness about the nature of their universe and the rules which govern their society" (Edward Shils, *The Intellectuals and the Powers* [Chicago: University of Chicago Press, 1972], p. 3). He is a paradigm of what Gramsci called the "traditional intellectual" and his motivating animus appears to derive largely from his own version of *la trahison des clercs* and, in particular, from his hostility to intellectuals of other stripes"—to "Marxists, refined and vulgar, liberals of faint heart, and other haters, high- and low-brow, of modern society" (p. xx). His aca-

demic project, of which this volume represents the principal theoretical statement to date, is thus politically engaged. It is to establish general theoretical arguments that will compel the assent and, indeed, the allegiance of intellectuals, in the liberal-democratic and the new states, to his own vision of modern society.

That vision is most clearly articulated here in his essay on "The Theory of Mass Society." It is of a society which is marked (albeit imperfectly) by "consensuality . . . , the closer approximation of center and periphery, the greater moral equality of the various strata and sectors, the growth of sensibility and individuality" (p. 105), a capitalist economy ("an economic life which is directed toward private gain" [p. 409]), and a consensually sanctified structure of authority and system of stratification and deference. In short, it amounts to a decidedly sanguine and idealized picture of contemporary liberal capitalist societies.

The essays in this volume furnish the general arguments for this picture. They deal with the influence of society's "outermost structure on the lives of the groups, strata and individuals who live within it and of the limit of its influence over those component parts" (p. vii); its author's theoretical interest is in "delineating the fluctuating shape of consensus and in tracing the conditions of its variation," while understanding the "limits of the capacity of the center to spread a cover of consensus around itself and over the entire society" (p. xxxviii). Such an inquiry is intended to establish, negatively, the "factual impossibility" of what the author takes to be implied in "the anarchist and communist idea" (and in ideological views generally) (p. xxxviii) and, positively, the distinctive traits of an emerging modernity.

Unfortunately, Shils's general theoretical arguments fail to compel, for a number of reasons. First, and most simply, they are couched in sentences whose meaning and truth conditions are so indeterminate and contestable and whose tone is generally so inflated that there is little chance of a precise understanding of specific social phenomena and processes. Terms such as "society," "order," "authority," "consensus," "central value system," "center," and "periphery," not to mention "charisma," "sacredness," "ritual," and "deference," are in play throughout. The point here is not just that they are used loosely and vaguely. It is that the author fails to see that they are *theoretical* terms whose links with observable phenomena are problematic and contestable. He uses them, on the contrary, as though they simply denoted observable phenomena, forces at work in social life, with their own principles of operation, interrelations, and consequences. Hence, there is a systematic reification of abstract concepts whose connection with the observable processes they purport to explain is inherently problematic and unexplored. For this reason, one rarely knows what their observational criteria are and thus what would count as evidence either for or against his claims.

"Authority," we are told, "is the agent of *order*, an order which may be largely embodied in authority or which might transcend authority and

regulate it, or at least provide a standard by which existing authority is judged and even claims to judge itself. The order, which is implicit in the central value system, and in the light of which the central value system legitimates itself, is endowed with dynamic potentialities" (pp. 5–6). Charisma "lives at the center of society" (p. 16): it is "a real and effective force. Quite apart from its manifestations in the routines of life which are loosely governed by religious attachments, it enters into obedience to law and respect for corporate authority. Furthermore, it provides the chief criterion for granting deference in the system of stratification and pervades the main themes of the cultural inheritance and practice of every society. Thus, normal charisma is an active and effective phenomenon, essential to the maintenance of the routine order of society" (p. 134). Again, the "center" of society "commands attention as well as demanding obedience. It has an attractive power which enters into imaginations and often preoccupies them. It both seeks to do so—although in varying degrees in different kinds of regimes—and it does so simply by its existence" (p. 39). It also plays a part in allocating resources, rights, and privileges; it is an integrating power in society; "it generates stratification and it entails authority" (pp. 69, 87).

Shils's world is, in other words, peopled by agencies (both this- and other-worldly) whose reality is assumed to be obvious. Yet whether these particular concepts (interpreted, moreover, in this particular way) make the best sense of all the evidence is disputable and, indeed, is disputed by contending theorists and by social actors. Not only are the very meanings of "authority," "central value system," "order," "charisma," and so on, in dispute among competing theorists (so that, for example, Shils's notion of dispersed charisma serving to maintain social order runs directly against Weber, coming close to Durkheim's notion of the sacred), but also, for any given meaning, what counts as an instance will be a matter of dispute. (What *are* the components of the "central value system" and, indeed, is there one? Was Nixon in authority during his last six months? Did Hitler have charisma?)

Furthermore, one may well doubt whether this family of concepts captures the factors and relations that explain the dynamics of contemporary or any other societies. Their reifying, hypostatizing character leaves totally opaque the relation between human agency (whether individual or collective) and the structural parameters within which it operates and which it in turn transforms (see the present author's essay, "Power and Structure," *Essays in Social Theory,* by Steven Lukes [New York: Columbia University Press, 1977]). Shils is, as he himself says, interested rather in the effects of society's outermost structure on its component parts—"the grip of society on its members and the attachment of individuals to society" (p. xv). Hence, there is no theory of change other than the assertion of "internal tensions," the "un-integratedness of the cultural value system," and the postulation (reminiscent of Molière's *virtus dormitiva*) of some men's "need for novelty" (p. 25). There is no

account (nor in this scheme of concepts could there be) of the dynamics which operate via and upon human agents within national boundaries and across them. It would appear, for example, to be far more explanatory to look at uneven development in a unified world economy, the external relations and internal constitution of states, and the interests and strategies of classes and parties if we wish to explain the complex range of phenomena invoked by the terms "social integration" and "social disintegration" (for a splendid recent example, see Tom Nairn, *The Break-up of Britain: Crisis and Neo-Nationalism* [London: New Left Books, 1977]).

In his introduction, the author even admits that the market and "the very important phenomenon of coercion" are also means of integration, remarking that "these essays deal mainly with the variations and mechanisms of consensus" (p. x). This will not do: first, because this exiguous trio of integrative mechanisms is obviously neither mutually exclusive nor jointly exhaustive; second, because their interrelations are of the utmost importance; and third, because his own explanations of integration in terms of consensus are ostensibly in competition with alternatives (in terms of class hegemony, say, or pragmatic role acceptance), yet he at no point weighs his own consensual explanation against any other, to see which the evidence best supports.

In the classical manner of Parsonian functionalism, Shils's key strategy is to explain in terms of "cultural" factors and, in particular, values and beliefs. This is a perfectly possible theoretical strategy, though one that is often contested and therefore in need of some defense. The key objection has been expressed brilliantly by Barrington Moore, Jr., as follows: "To maintain and transmit a value system, human beings are punched, bullied, sent to jail, thrown into concentration camps, cajoled, bribed, made into heroes, encouraged to read newspapers, stood up against a wall and shot, and sometimes even taught sociology. To speak of cultural inertia is to overlook the concrete interests and privileges that are served by indoctrination, education, and the entire complicated process of transmitting culture from one generation to the next" (*Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* [London: Penguin, 1967], p. 486). Shils clearly makes a different judgment about where explanations should properly stop. The proof that he is right, however, can hardly be said to lie in his particular pudding.

There is, moreover, a further and distinctive problem with many of his cultural explanations, namely, that he seeks to explain, not merely in terms of values and beliefs, but in terms of their religious or transcendental *content*. Hence, his frequent references to the " 'ultimate' structure of reality" (p. 7), to "an order of being where 'creative power' has its seat" (pp. 7–8), to "the vital zone which surrounds the center of society," as "the vehicle of 'the center of the universe' " (p. 13), to the charismatic quality as " 'connected' or . . . 'infused' with a 'metaphysical essence' " (p. 42), to the crowds at the coronation of Queen Elizabeth as entering "into contact with the mighty powers who were symbolically and, to some

extent, really responsible for the care and protection of their basic values and who on this day had been confirmed in these responsibilities" (p. 147). The use of quotation marks here does not obscure the fact that we are being offered explanations that are religious or transcendental rather than sociological. It is arguable that while the sociologist can leave open the question of whether the theological hypothesis is true he has, like Laplace, no need of it and should not, *qua* sociologist, employ it in explanations. The religious or transcendental content of beliefs and values is, in other words, properly to be found, if anywhere, in the sociologist's *explanandum;* while it remains in his *explanans,* he has more work to do.

I observed above that my criticism of Shils as an explanatory theorist might well arise from a difference of view about what constitutes good theorizing. It may, therefore, be appropriate to conclude with a reference to his own (highly subjectivistic) observations on this subject. Theories, he writes, "must be taken as general guides," they "must be brought into operation only on the basis of a feeling of personal intimacy," and they "must be used only after an osmotic assimilation which involves discriminating acceptance and rejection, which rests on a sense of fitness and appropriateness rather than on any formal test" (p. 125). Theory, he writes, "will bear fruit in sociology only when it has been assimilated into the perception of concrete and particular events" (p. 126). The irony is that Shils is as shrewd and subtle an observer of such events as he is pungent and hard hitting as a liberal ideologist. Unfortunately, his grand theorizing aids him in neither role and promises to bear little fruit.

Book Reviews

All Silver and No Brass: An Irish Christmas Mumming. By Henry Glassie. Bloomington: Indiana University Press, 1976. Pp. xx+192. $12.50.

Howard S. Becker

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For many years now, social scientists have taken some pains to ensure that what they did was science and not humanistic meandering, unchecked by the rigorous procedures which protect the world from the merely plausible that might be palmed off in place of the logically compelling. That needed to be done. For just as many years, social philosophers and commentators had said (as they still do, with many social scientists among them) almost any damn thing that came into their heads, relying on their intuition and what they read in the newspaper for evidence, never letting reality, systematically assessed, get in the way of their fantasies. That let the worst kind of prejudice dominate the field, leading to a lot of ancestors most of us would just as soon forget about.

But social science paid a big price for excising the humanistic. You can't excise it. You can make it look like you have, but it sneaks back in, in the form of rhetoric (see Joseph Gusfield's 1976 article, "The Literary Rhetoric of Science: Comedy and Pathos in Drinking Driver Research," *American Sociological Review* 41:16–32). Artistry and intuition are part of scientific work. The earlier folly was to think they were the whole thing. To imagine that they had no part in it was just as foolish. Pretending that they had no part meant (and this is the price we paid) that we lost control over those aspects of our work that involved the less systematic and rigorous (you cannot control what you pretend is not there) and that we did not get the advantage their controlled use might have afforded.

A new generation of social scientists seems intent on rectifying the mistake. Henry Glassie, a folklorist, exploits the marginality of a field that is simultaneously humanistic and social scientific in his study of an old and now disused Irish custom. Mummers go around at Christmas performing a simple play in rhyme, in which a motley cast of characters recite some seemingly meaningless doggerel and go through some equally puzzling mime, after which they take up a collection, perhaps stay for some tea, and then go on to the next house.

Aggressively humanistic, Glassie begins with his data, not very good by social science standards but, you eventually see, quite good enough. Since Ulstermen do not mum now (partly as a result of Ulster's chronic ethnic warfare), he could not see the custom in action and instead relied on four elderly informants and a variety of less intensive interviews with others in the small community he worked in. He summarizes, with many

verbatim quotes, the stories these people told him about what they used to do and what it was like. So the first section of the book is a personal account of his relations with them and what they told him. Informal, even casual, it is a good deal more informative than the "methods" section of most scientific works. You know how you can trust his data and why.

The second section, more analytic, relies heavily on social science theory of all kinds: theories about historical survivals and geographic dispersion of customs, interactionist theories of art as performance, structuralist theories of meaning, theories of the consequences of social activities (a nonobjectionable kind of functionalism). It is less concerned with proving hypotheses than with using ideas to make sense of an event closely observed, and this is another aspect of its humanistic bent. Simultaneously, our intensive knowledge of the event gives concrete meaning to the ideas and provides a touchstone for evaluating them, so we can also make better sense of the abstractions and evaluate them more rigorously.

Glassie has violated some social science conventions at a more superficial level as well. The book is illustrated with his own photographs and drawings, and the style is personal and even a little "literary." I'd quarrel with the latter, but only on literary grounds; the portentous, poetic manner got wearing for me, though not often. These minor items reminded me of how conventionalized our writing and reporting has become, how much of it is devoted, as Gusfield suggested, to giving the appearance of objectivity, though the relation of these scientizing devices to producing reliable knowledge is not established.

In any event, *All Silver and No Brass* is a good book and one that I think is part of a wave that is just beginning to sweep through sociology. This new wave might be a false start, but I prefer to think that it is the beginning of a trend in which we will reclaim the possibilities of humanistic methods and styles from the present limbo.

In Stalin's Time: Middleclass Values in Soviet Fiction. By Vera Dunham, with an introduction by Jerry F. Hough. New York and London: Cambridge University Press, 1976. Pp. xiv+283. $16.95.

Allen H. Kassof

International Research and Exchanges Board

This stunning book, by a literary scholar, is surely one of the finest studies of Soviet Russian society ever to appear. Vera Dunham's subject is the official shift following World War II from the sacrificial and "heroic" values of the postrevolutionary years to a system of material rewards (and the values associated with it) by which Stalin hoped to motivate the technical and managerial classes and which is now very much in place.

While the social and demographic outlines of this transition are well known to students of Soviet history, the subtle process of presenting it to the public, and then justifying and fine-tuning the new outlook, is illumi-

nated for the first time in Dunham's sophisticated excursion through the middlebrow literature of the period. Stalin's "Big Deal," as Dunham calls it, involved the cultivation, or the re-creation, of *meshchanstvo,* a petit bourgeois culture summoning up images of self-satisfaction and self-importance, tinged with shallowness and vulgarity, but totally devoted to the system that provides its life comforts. Since the original revolutionary asceticism had nothing but raging contempt precisely for the material and moral posture represented by the *meshchanin* and his family, the new legitimation required careful preparation. The campaign proceeded on several fronts, but nowhere did the new role models appear so vividly as in fictional literature—a medium, like others, echoing the party line through a system of precensorship and avidly consumed by a new urban class searching for a style. It its pages, the exemplars of the new values could display the rightness of their behavior, engage in arguments against outdated modes, and be pulled back into place if they went too far.

That, in brief, is what Dunham analyzes here, and her work is first-class literary sociology. What is simply breathtaking about the book, though, is Dunham's picture of Soviet culture-in-becoming during the late Stalin years, which formed the basis for many present-day values and practices. Those who know the Soviet Union today will instantly recognize in Dunham's brilliant characterizations and descriptions the sounds, sights, and language of Russian society. Thus:

> The regime's shift in the public realm from the revolution to Stalinism determines the curious relationship of two additional words, eminently translatable, *kultura* and *kulturnost.* Akin in etymology, substantively they stand at odds with each other. *Kultura* is the achievement of the intelligentsia in the sense of higher culture. . . . *Kulturnost* . . . is a derivative. . . . Having nothing to do with a spiritual legacy, it is instead a mere program for proper conduct in public. . . . *Kulturnost* represents, both at home and abroad, a victorious, conservative force in Soviet postwar life, embodying a slick decorum and a new kind of self-righteousness—stable, prudent, heavy. Its special function is to encode the proper relationship between people through their possessions and labels; between mores and artifacts, to put it more fancifully. [P. 22]

The Big Deal, of course, is not to be confused with a political embourgeoisement, for it concerns manners and mores rather than class solidarity; those who enjoy the perquisites of the new arrangement do so at the pleasure of the party, and not on the basis of their own independent wealth or standing. Nevertheless, the acceptance of the Big Deal by both sides has real consequences for Soviet society and politics, insofar as it binds the regime to take into account, however imperfectly, the expectations of people who behave increasingly like consumers. We can only wonder what Stalin himself would have thought of Italian-licensed Fiats being turned out in Soviet factories or American grain imports (needed, incidentally, not so much for bread as to assure the kind of animal protein diet now considered to be essential to the good life) or, yes, Pepsi-Cola.

All of this suggests an intriguing comparative question which Dunham does not raise (she has the good sense to leave superb enough alone), but which nonetheless invites consideration. Is the drive for material possessions (and the accompanying set of values) in the Soviet Union so very different from its more urbane versions elsewhere in the industrialized (or, for that matter, unindustrialized) world? What is the difference between the Soviet executive's lusting after a Volga automobile for family excursions and the Frenchman's regard for his shiny new Peugeot? Differences, of course, there are: since there is still much less to go around in the Soviet Union, and the quality is usually tacky, the quest for possessions often has an embarrassingly pathetic aspect to it; moreover, a much larger percentage of the population in Western countries enjoys access to mass-produced consumer and leisure goods, so that (some) ordinary workers have possessions that are still beyond the reach even of most of the best paid of Soviet professional employees. Nevertheless, this variation may be a matter of degree only: the social compact that has engaged the loyalties of the executive and managerial classes in postwar America, Europe, and Japan may not be very different in principle from the Big Deal, even though it offers, to say the least, better prizes.

However that may be, the Soviet version claims to be different, purer, and certainly faithful to revolutionary principles. But it will be more difficult than ever to take such claims seriously after reading Dunham's book. Acquisitiveness may indeed be an absolutely essential component of productivity in all modern societies, but it does not necessarily look any more attractive under socialism.

The Russian Revolution: A Study in Mass Mobilization. By John L. H Keep. New York: W. W. Norton & Co., 1977. Pp. xvii+614. $19.50.

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"It is generally characteristic of our scholarship on Soviet history," writes Stephen Cohen (in *Stalinism,* ed. Robert Tucker [New York: Norton, 1977], p. 7), "to explain social and political development after 1917 almost exclusively by the nature of the party regime and its aggression upon a passive, victimized society. Authentic interaction between party-state and society is ignored. Not surprisingly, the literature of academic Soviet studies contains little social history or real social studies; it is mostly regime studies." If, as Cohen's insightful comment suggests, the challenge currently facing Soviet experts is to begin producing social rather than simply regime studies, then John L. H. Keep's *The Russian Revolution: A Study in Mass Mobilization* certainly appears to take up the challenge. Keep focuses on the popular participants in the early revolutionary upheavals and discusses in detail the emergence and development of the many kinds of grass-roots organizations that sprang up in 1917 to repre-

sent the aspirations of workers and peasants. Nevertheless, Keep's book fails to break with old-fashioned modes of explanation long dominant in Soviet studies. Ironically for a work that focuses chiefly on the situations, aspirations, and organizations of ordinary people, *The Russian Revolution* still ultimately explains the course of events in terms of "the nature of the party regime and its aggression upon a passive, victimized society" (ibid.).

The most refreshing and valuable thing about *The Russian Revolution* is unquestionably the scope of its empirical coverage. Not that Keep's work is based upon large amounts of new primary data; rather, it assembles evidence from published collections of primary documents and from the articles and monographs of other historians, both Western and Soviet. But Keep cunningly paints large portions of the mural that others have left blank or only roughed in. He discusses the revolutionary events of 1917–18 in the various provincial centers of European Russia, valuably supplementing the usual emphases on the capital cities and minority regions. And he provides the very first general survey in English of the course of the peasant revolution in the core agricultural regions of the empire. The book as a whole is divided into five main parts. Part 1 surveys the socioeconomic circumstances in which Russian industrial workers and peasants found themselves in the years just before and during World War I. Parts 2 and 3 survey, respectively, the urban and rural popular revolts of March–September 1917. And parts 4 and 5 discuss the fate of popular organizations and interests after the Bolshevik assumption of state power, carrying the story forward until March 1918 for the urban areas and until the end of 1918 or so for the countryside. In the parts about the urban revolution, Keep provides separate discussions of factory committees, workers' militia units, trade unions, and the soviets. An unfortunate effect is to render the narrative and analysis quite disjointed; but, on the other hand, valuable information is pulled together on each of the organizational forms. The one important thing avowedly missing is any systematic coverage of soldiers' revolts in 1917; Keep acknowledges the gap and says that he is leaving this task to Allan Wildman (who is at work on a definitive study of the Russian military before and during the revolution). But probably no account of the urban popular revolts can be at all complete without including the garrison soldiers.

Where Keep really shines is in his presentation of the peasant revolts of 1917, which have "some claim to be considered the main dynamic force behind the Russian revolution . . ." (p. xiv). Two aspects of the peasant revolts are discussed in detail. First, there was the "passive resistance" by which peasants kept their produce off the market when they were not offered in return adequate repayment in noninflated currency useful for purchasing readily available manufactured goods. Keep shows that this resistance, which was to plague the Bolshevik regime in the 1920s, began during World War I and helped to undermine the original Provisional Government. Second, there was the "wave of confiscation and redistribution of landed property which occurred in 1917–18" (p. xiv). Borrowing his analysis from Socialist Revolutionary–oriented intellectuals such as

A. V. Chayanov and Theodor Shanin, Keep stresses the communal basis of the peasant land seizures in the village assembly (*mir*) and the repartitional land commune (*obshchina*). Keep points out that it was precisely in those "black earth" provinces, where traditionally strong peasant communes coexisted with private landed property, that the agrarian revolts of 1917 were most violent, most clearly directed against the properties of noble landlords and of *khutors* (peasant farmers who had broken off from the communes), and most likely to culminate in complete (or "black") repartitions, equalizing as nearly as possible the properties held by individual peasant families within each local commune. Keep argues that the Bolsheviks' analysis of the Russian peasantry, which stressed class divisions between richer and poorer peasants, was basically inaccurate because it failed to give the communal institutions adequate weight, misunderstood the (essentially factional) bases of intravillage politics, and mistook socioeconomic differentiation due to stages of the life cycle among peasant families for permanent class differences within the villages. I believe that Keep's discussion and analysis of these points (specifically in part 3 and in chaps. 28–30 of part 5) are essentially correct on the basics. It does, however, strike me as an unfortunate sign of ideological dogmatism (which still pervades Soviet studies) when Keep declares (p. xv) that "classical Marxist sociology" has nothing to do with the picture of the peasantry that he presents. If Marxist class analysis has been inappropriately applied to relations among the peasants themselves, this does not mean that it is inapplicable to the landlord-peasant relation. What else, after all, was going on in the Russian countryside in 1917 but a massive class struggle of the peasantry, a producing class against the noble landlords, quintessentially a property-owning, surplus-appropriating class?

If Keep's analysis of the peasantry in 1917 is essentially correct—or, in any event, certain to be a fruitful contribution to future debates—the same cannot be said for his explanatory analysis in the book as a whole. Keep claims an enormous import for that analysis: "This book seeks to show," he asserts at the start, "why the Russian revolution of 1917, an elemental popular movement inspired by the most egalitarian and libertarian ideals, gave birth to the twentieth century's most durable dictatorship" (p. vii). This is, indeed, the key question about the Russian Revolution, but how could a book that only surveys the events of 1917–18 make good the claim to answer it? Only if we can accept Keep's notion that the period he covers was the decisive turning point of the revolution, when the elements that characterized and determined its outcome first fully emerged. This was, in Keep's view, the crucial period of the "bolshevization of the masses," since "it was in this period that the Bolshevik party first acquired a mass following. This meant that their intransigent, authoritarian style of thought and action communicated itself to large numbers of ordinary people, instead of being confined as heretofore to a small intellectual elite" (p. 470). This remarkable conclusion rests on several analytic assumptions that pervade *The Russian Revolution*: first,

the assumption that ordinary people rebel in disorderly, apolitical ways only for elemental socioeconomic goals; second, the assumption that the various "mass organizations" that sprang up in 1917 were inherently vehicles through which political leaders gained heteronomous control over the masses; and, finally, the assumption that the Bolsheviks were at all times monomaniacally disciplined and oriented to controlling and manipulating the masses from above. Given these assumptions, it is possible to interpret the events of 1917–18 as primarily a process by which the Bolsheviks "took over" mass organizations and duplicitously used their energies to defeat the Provisional Government, the other socialist parties, and the Whites. In this view, the mass organizations, properly analyzed, can be seen as containing the seeds of the Bolshevik dictatorship.

One trouble with all of this is that it is nothing new—it is the same old "regime analysis" dressed in new empirical clothing. More telling, the analysis is simply not correct. Keep's own factual descriptions offer a much more nuanced picture, showing for example that both workers and peasants were perfectly capable of creating and acting through their own "self-constituted" bodies, such as factory committees, militias, and the commune meetings that undertook land seizures. And Keep reveals that the Russian peasantry had a very clear political program—that is, local self-rule! Moreover, during the period before October 1917 it was not the Bolsheviks but the Provisional Government and the other socialist parties that were most oriented to controlling popular organizations from above. During this period (as is vividly shown by Alexander Rabinowitch in *The Bolsheviks Come to Power* [New York: Norton, 1976]) the Bolsheviks were striving to keep in touch with quickly evolving popular sentiments, and they managed to do so only by tolerating considerable sacrifices of party unity and hierarchical discipline, since thousands of new recruits were flooding into the party and grass-roots organizers often pushed beyond the decisions of top party leaders.

After October 1917, of course, the Bolsheviks changed. But it seems very likely that the crucial period for the crystallization of a dictatorial party-state was not 1917 to early 1918, but the Civil War period of late 1918 to 1921, when the Bolsheviks suddenly reconstituted and began to rule through a centralized, professional army and state bureaucracy. To believe Keep one would suppose that the essence of the Bolshevik regime was party rule through mass organizations, especially the soviets. (Keep even tries to portray the Bolsheviks as consolidating control of the countryside during 1918 in this way, thus flying in the face of the virtual consensus among Soviet specialists that Bolshevik rural control remained very weak until after the forced collectivization of 1928–33.) "By the spring of 1918," Keep declares, "the *Gleichschaltung* of the mass organizations was virtually complete. . . . The Communists were masters of a ramified network of quasi-popular bodies. . . . [and they] could make use of this machinery for administrative, police, or military tasks. . . . [T]he mass organizations became sounding-boards for decisions by the ruling oligarchy, an integral part of the new command structure" (p. 471). Actually, this

would be a better description of the *Chinese* Communist system than of the Bolshevik regime. For in Russia, it was not that a communist party created popular organizations, then came to power and ruled through them; rather, it first allied with spontaneously created or popularly supported bodies in order to ride to urban-based state power and then increasingly by-passed or destroyed all such popular organizations as it consolidated power in and through traditionally organized state bureaucracies.

Keep's book contains much material of value, especially about the peasantry, but, despite its ambitions, it does not surpass William Henry Chamberlin's classic *The Russian Revolution* (New York: Grosset & Dunlap, 1965; originally published 1935) as a general synthesis. Chamberlin's book featured the complex interplay of social and political forces from 1917 to 1921, and it will not be surpassed until contemporary scholars accept that the Russian Revolution cannot be interpreted as the sheer realization of "a dictatorial element . . . embedded in Leninism from its first appearance in 1902" (p. 469). Only when we can see the dialectic of spontaneity and control played out, not merely among and against "the masses," but also in the very aims and organization of the Bolshevik party as it struggled both to respond to popular and socialist aspirations and to consolidate state power, only then will we truly understand the reality—and the full tragedy—of the Russian Revolution.

The Modern Corporate State: Private Governments and the American Constitution. By Arthur Selwyn Miller. Westport, Conn.: Greenwood Press, 1976. Pp. xvi+269. $15.00.

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Once again the large corporation is before us as a major problem of the political order. It has been with us for a century, of course, but Americans have been willing to look at it only fitfully and then only on occasions when particularly flagrant corporate behavior has been forced upon our attention. The railroads were the first to create a widespread sense of outrage, and they were followed by the rise of the "trusts" and a similar public outburst. Another spasm occurred during the New Deal era, along with occasional lesser moments of concern. And most recently there has been a minor explosion over revelations of nearly systematic bribery of foreign officials by American multinationals. At each point, however, the problem has been made to appear specific and narrow, as though there were nothing more fundamental at issue than the excesses of a few individuals in positions of corporate leadership.

This impression is of course false. The modern large corporation is a brute fact of the most fundamental social and political importance. The radical character of its development has been stated by a handful of ob-

servers, most notably by Adolph Berle and Gardiner Means in 1933 and by Edward S. Mason and his collaborators in 1960, but without perceptible effect on the mainstream of political thought in America. Now, Arthur Selwyn Miller, professor of law at George Washington University, has spoken out to say that the corporate emperor has no intellectual clothes. Miller's new book, *The Modern Corporate State: Private Governments and the American Constitution,* is a vigorous indictment of our failure to grapple in any serious way with the most important structural change in the political system of the century. He states quite flatly that a silent constitutional revolution has occurred and that the document of 1787 is now no more than a facade (pp. 112, 142).

As Professor Miller sees it, this constitutional revolution took place over time, beginning with the critical step of formal acceptance by the Supreme Court of the fiction that a collective body, the corporation, is a "person" for legal purposes. Indeed, this step accompanied and abetted a deeper trend in America, the substitution of the group for the human individual as the fundamental unit of American society. Miller suggests that the development, particularly as it affected the corporation, was quite consciously intended and was no accident. The consequence is the rise of an array of powerful organized interest groups that are in effect "private governments." These include farm groups, and labor and other unions, as well as corporations. These do not, as J. K. Galbraith once argued, exercise countervailing power (Miller gives short shrift to Galbraith). Instead, they tend to collaborate more frequently than not, and, more importantly, to mesh symbiotically with the public organs of government. The distinction between what is public and what is private has accordingly been blurred, and a fusion of public and private power has occurred.

According to Miller, this revolution has been marked by a change in governmental power, the rise of "positive government" during the New Deal that culminated in the Employment Act of 1946. This, together with the increased economic power of the large corporations to which it is joined, has reduced the individual to impotence and insignificance. What most exasperates Miller is not the degree to which this revolution threatens the individual but the utter failure of both constitutional and political theory to take the change into account. There is, as he repeatedly points out, no concept by which the corporation can reasonably be regarded as legitimate, just as there is no vision in which the organized group can be made to accord with any theory of democracy in America. This is the real scandal.

Miller does not himself undertake to repair this failure, apparently because he has grave doubts about the wisdom of attempting to endow the large corporation with the now-absent legitimacy. Despite this, he is willing to concede that in its materialism the American public is not dissatisfied with the anomaly of the situation, given the outpouring of goods that has come with it. The nearest he comes to suggesting a solution is an imposition of constitutional restraints on the large corporation by

the courts. In light of his distress with the courts' shortcomings, it is understandable that his enthusiasm for this approach is restrained.

This is a forceful book and one that needs to be heeded. Although individuals may differ with Miller on particular parts of his analysis, his basic point is correct, and we cannot continue indefinitely to ignore the problem.

From Mandeville to Marx: The Genesis and Triumph of Economic Ideology. By Louis Dumont. Chicago: University of Chicago Press, 1977. Pp. ix+236. $16.50.

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From Mandeville to Marx contains some striking insights into the emergence of the economic category, a novel approach to Smith's labor theory of value, and a remarkable misinterpretation of Marxian thought. Anthropologist Louis Dumont traces the political triumphs and moral retreats of the modern individualist viewpoint in economic theory. He has produced a highly provocative work, one that is sure to enlighten and annoy a great many readers.

One task of the social anthropologist is to study the ideologies that animate different social formations. In his previous book, *Homo Hierarchicus,* Dumont explored the cluster of values and beliefs which defined traditional caste societies. His attention has now turned to *homo aequalis*: the present work traces modern "individualist" ideology through its origins and development in economic theory. Dumont will be accused, with some justification, of ignoring the economic and political preconditions of ideological change. But despite its Durkheimian overtones, Dumont's notion of ideology bears a strong (if unacknowledged) resemblance to its Marxian counterpart, as developed in the writings of Lukács and Goldmann.

At the core of Dumont's study is his concept of "individualism." In traditional "holistic" societies, relations between persons have paramount importance and are more highly valued than relations between persons and things. In modern individualist societies this primacy is reversed, and "relations between men are subordinated to relations between men and things" (p. 5). The ascendency of property relations over personal ones is marked by the emergence of economic activity as an autonomous social realm: in the exchange and production of material goods, free from the social restrictions of politics and morality, the ethic of individualism finds its fulfillment. Following Tawney, one could view this isolation of the economic realm as the partial product of changing religious beliefs. Our author takes a different tack, looking instead to the internal logic by which the early economic theorists disentangled man's relations with material objects from his political and moral concerns. As sociology this is spotty, but for philosophical illumination it is right on the mark.

The key figures here are Locke and Mandeville. While Quesnay was the first to conceive of the economy as an interdependent whole, economic activity remained, in his vision, embedded in a network of political purpose and natural law. Locke's contribution was to reverse the rule of politics over economics, so that political institutions were seen to exist for the purpose of protecting property relations. Mandeville could then dismiss moral judgments from economic calculation by showing how the selfish pursuit of one's own economic interest unwittingly promoted the general good. Adam Smith would also argue that economic action escapes morality without being contrary to morals in a wider sense. In his account of these and other matters, Dumont commands assent up to the very last paragraphs of section 1, where he ventures some remarks on the relevance of this analysis to our current ideological predicament. The values of individualism are so intertwined with the autonomy of economic life that any threat to the latter is inherently a threat to the former. Attempts to subject economic activity to political control must invariably lead, in Dumont's view, to the "involuntary or imposed subordination and the disregard of the Individual as a value" (p. 107). But hasn't the individual, in our age of monopoly capitalism, already been subordinated to the control of outside agents? Here Dumont's single-minded focus on values fails him. He insists that whatever changes have occurred in reality, the individual remains at the center of our economic ideology. This ideological focus must be preserved, since in shaping political practice it is ideology, not reality, that counts. But the contradiction between theory and reality cannot be dismissed so lightly: if our ideology no longer fits the world around us, then ideology should change. *Belief* in the autonomy of the individual will not make it so.

In the book's second half, Dumont attempts to situate Marx's work in light of what has come before. Since Marx spent much of his life attacking the political and philosophical foundations of classical economics, one would assume that he rejected its individualist ideology as well. In Dumont's view, nothing could be further from the truth. In his account, Marx's work enshrines the "economic" and signals the triumph of the individualist viewpoint. The author traces this individualist commitment through both the earlier and later writings, and I shall deal with each in turn.

Marx's early philosophical writings, particularly his *Critique of Hegel's Philosophy of Right*, attacked the "transcendent" ideals of religion and and state and affirmed in their stead more humanistic values. Thus, Marx saw religion as both a product and promoter of human alienation, a hindrance to the full development of the human capacity. Dumont is right in equating this "destruction of transcendence" with the "emancipation of man," but he is wrong in perceiving here a priority of individual goals over social ones. In calling Marx an "individualist," he presumes a distinction which Marx (and Hegel, for that matter) wished to deny: Marx did

not seek to liberate individual activity from the constraints of the soci whole. He sought instead to construct a community in which society needs no longer conflicted with the projects of individual people.

The individualism of Marx's later work is most evident, Dumont feel in his emphasis on production. In conceiving of production as the found tion of all other activities, Marx accorded (in Dumont's view) an ind vidualist primacy to relations between people and things. But Marx focus on production carries no such meaning: in his criticism of the cla sical theorists, Marx constantly insisted that the categories of productic (capital, value, surplus value, etc.) be viewed as social categories. Produ tive activity for Marx was just as "political" as any other, and tl classical view of economic activity as a stark encounter between peop and things was but another expression of "commodity fetishism." Rath than endorse this view, Marx devoted much of his energy to attacking i

Two final notes. The author's remarks on the scientific status of tl labor theory of value suffer, I believe, from an unfamiliarity with recei economic theory, specifically the work of Pierro Sraffa. He also makes number of misstatements about Ricardo; for example, that Ricardo di not grasp the implications for the labor theory of value of differing capit investments in different industries (p. 100; see also p. 23).

The Noble Savages: The Primitive Origins of Charisma and Its Conten porary Survival. By Bryan Wilson. Berkeley: University of Californi Press, 1975. Pp. xii+131. $7.50.

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The constraints imposed by the brevity of this volume set serious limits o the author. A brief treatment of charisma necessarily invites generalizatio yet Wilson manages to develop the main characteristics of charisma, arguin that it is a phenomenon of followers and social circumstances rather tha of leaders themselves.

The author views charisma as a quintessentially premodern phenomeno of traditional or "less developed" societies. The analyses of specific cas studies of American Indians (Pontiac, Tecumseh), African societies (Wil liam Wade Harris, Simon Kimbangu), and the cargo cults of Melanesi support Wilson's emphasis on the premodern derivations of charisma. Th case studies are interesting and will be useful to those wishing a quic introduction to charismatics and millenarian movements, but they merel summarize well-trodden ground for those familiar with the literature.

Most students of charisma will take issue with three issues raised b the author: his insistence on charisma as mainly premodern, his emphasi

on faith as an element of charisma, and his view of charisma as being limited to individuals (rather than collectivities or movements).

"The likelihood of a socially significant manifestation of charisma undoubtedly diminishes with technical advance" (p. 104). While Wilson is correct in arguing that the rational character of modern society tends to resolve many issues that can give rise to charistmatic demands of a people, his view of charisma emphasizes its prerational character. Yet charismatics continue to be discovered, although their frequency would appear to diminish as the scale of society grows. Perhaps that is the key factor—the scale of society—rather than the issues of modernity, rationality, and "progress." Modern large-scale societies are not composed of homogeneous social categories—hence the character of social crisis cannot be uniformly meaningful for all social strata. This means that charisma is harder to find in modern societies but, once found, may be far more intense (in its social totality) than in premodern societies. It is always well to remember the devastating influences of Hitlerian charisma before attributing charismatic appeal simply to premoderns.

Wilson's treatment of charisma as encompassing faith (pp. 93–99) partially repudiates the earlier—and correct, in my view—formulation of charisma as being a product of social conditions and as a social result of a following (not the charismatic leader). While charisma inevitably involves faith it also is a function of the failures of existing social arrangements—traditional or rational-bureaucratic—to resolve some crisis in a given society. When such a failure occurs, the search for solutions is not simply an act of faith but a product of the rational understanding of the failures of existing arrangements. Wilson's formulation is therefore only partially correct.

Finally, Wilson's view attributes charisma to the individual and thus misses perhaps the key differences between manifestation of charisma in premodern and in modern societies. In modern times, the belief in the "gift of grace" is less the product of a belief in something supernatural, limited to individuals, than the ostensible product of superior "rationality." It is not only that charisma is attributed beyond Hitler (although it heavily focused on his person) to a Nazi party, but that some leaders can be almost totally devoid of charismatic qualities while still being enmeshed in a movement with charismatic attributes. One need only mention Stalin, who never had any charismatic appeal in the individual sense but who could utilize the charisma of a movement to entrench himself in power. And one must also remember the charismatic appeal of bolshevism for large segments of the working class movement—an appeal that is still undissolved in countries such as France and Italy, where the charisma of the world's first working-class revolution remains powerful even while Russia itself has become suspect. My point is that modern charisma is quintessentially organizational rather than personal: the followers still attribute a special "gift" to a movement rather than to a person.

Trial of Faith: Religion and Politics in Tocqueville's Thought. By Doris S. Goldstein. New York: Elsevier Scientific Publishing Co., 1975. Pp. xi+ 144. $10.00.

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The scope of this study is broader than the title suggests: it explores the place and significance of religion not only in Tocqueville's theoretical writings but in his practical political activity as well. "Because he saw himself as an *homme politique* rather than as a closet philosopher," Goldstein properly notes, "the political and social circumstances of his own country were not only the context, but the catalyst, of both thought and action" (p. x). Her strategy is accordingly to understand Tocqueville's religious position through an analysis of both his personal beliefs and his approach to 19th-century French society and politics.

Previous estimates of Tocqueville's religious concerns have spanned a broad range, identifying him as a Christian moralist, as a 19th-century liberal essentially indifferent toward religion, and as a pragmatist concerned solely with the social utility of religion. For Goldstein none of these interpretations does justice to the complexity of Tocqueville's attitudes. A religious crisis in his youth shattered the certitudes of his early Catholic training but could not extinguish his aspiration to firm belief and his continued adherence to such fundamental Christian teachings as the existence of God, the immortality of the soul, and much of the moral message of the Gospels. Despite the tension between his desire to accept Catholic doctrine fully and his inability to do so, Tocqueville regarded religious belief as an essential condition for a liberal democratic society He admired sincere religious conviction, he deemed it a necessity for a free society, and he strove to find personal religious certainty; thus the categories of faith, truth, and utility emerged in his mind (p. 124).

His experience in America confirmed and strengthened his belief that religious faith could not only survive in democratic society but also contribute positively to the development of free democratic institutions. Not until the 1850s did Tocqueville distinguish between the social utility of sincere religious affirmation and the hypocritical employment of religion to pacify potentially restive populations. His tardiness in recognizing the strength of authoritarian opinions within French Catholicism reflects his commitment to reconciling liberal and Catholic ideas and underscores his weakness as a practical politician. Goldstein shows how on numerous occasions Tocqueville's optimism about the compatibility of liberalism and Catholicism led him into ill-reasoned or indecisive positions. "The defection of the majority of Catholicism and clerical leaders to the Second Empire forced him to recognize how deeply rooted were the inadequacies of the Church in the public sphere; he searched back into the French past to discover the reasons for these shortcomings" (p. 96). This inquiry into the *ancien régime* produced his famous analysis of France's difficulties in recon-

ciling religion and liberty. Here in the theoretical sphere, diagnosing the imperfections of French public morality, Tocqueville was in his proper element.

Tocqueville's treatment of religion, like the rest of his political sociology, is couched in comparative analysis. Goldstein reviews Tocqueville's efforts to assess the compatibility of religion with a free and démocratic society in France, England, Ireland, and America. She then demonstrates that his interests extended to Islam and Hinduism as well. Tocqueville took a dim view of these Eastern religions, regarding them as conducive to despotism and stagnation. Because of their damaging impact, the prospects for liberal society in the East were slim.

Goldstein's essay rests primarily on a careful and thorough reading of the Tocqueville corpus. It is unlikely to generate substantial reinterpretations of Tocqueville's social theory, but her careful criticism of Tocqueville's assumptions as a working politician and as a theorist, particularly of his long-standing conviction that a union of liberalism and French Catholicism was a feasible and even natural outcome, is illuminating and makes a genuine contribution to one's understanding of Tocqueville and the genesis of his thought.

The American Catholic: A Social Portrait. By Andrew M. Greeley. New York: Basic Books, 1977. Pp. 286. $15.00.

Hart M. Nelsen

Catholic University of America

This well-integrated book contains Greeley's research findings over 15 years on the achievement of non-Spanish-speaking white Catholics, the graduate school experiences of the 1961 NORC college graduate sample, the political attitudes of American ethnics, the import of Catholic schools and changes in the Catholic church, ethnicity and family structure and values, and other related topics.

As Greeley notes, there are three themes here—(1) that there is cultural division of labor in American society which is legitimated by stereotypes of Catholics and which discriminates against them at the top end of the occupational ladder; (2) that "American ethnic groups are less a residue of Old World culture and more a dynamic innovation of the American environment" (p. 271), with Catholics having distinctive familial structures and values in comparison with Protestants, and maintaining their religious loyalty; and (3) that there is a "development of a communal style of affiliation with Catholicism," while at the same time there is a rejection of aspects of the institutional church, especially its sexual ethic (p. 272). The "communal Catholic" concept is not well developed. In the long run, the rejection of the church might well mean rejection of religious identity and ethnicity. Greeley writes that "most people tend, more or less, to identify with the religious or the religio-ethnic collectivity. There is no evidence that such

a tendency is diminishing appreciably" (p. 28). But neither is evidence presented to show the retention of such identity. Similarly, he does not present data to support his contention that the unique characteristics of American ethnic group cultures are more a product of their new experience than a carry-over from the old country. Greeley is not one given to grandiose statements, and therefore I look forward to seeing these data in future publications. In the present volume his best data concern the first theme, but there they do not really focus on discrimination at the upper level.

A few years ago, Greeley reacted to the work of Lenski and others by calling for a moratorium on Protestant Ethic studies making broad Protestant/Catholic comparisons. A good deal of Greeley's work relates to this topic, but his substantive interest has been ethnicity rather than religion or class. He argues that "there is cultural division of labor in American society by which Catholics are systematically underrepresented in certain high-prestige positions, most notably the foundations, the great private universities, . . . the boards of the most powerful business corporations. This . . . has been legitimated by a mythology which views Catholics as narrow, rigid, antiintellectual, morally authoritarian, politically corrupt, economically unsuccessful, politically conservative, racially prejudiced, and tied down by the obstacles of family and neighborhood loyalty" (p. 270). His important research attacks such a bias.

His recent NORC data clearly show that the Catholic averages for years of education and occupational prestige are right at the national mean, while Catholics earn more money than any other Christian denomination—$1,421 above the national average" (pp. 53–57). Taking into account parents' educational levels, Catholics are behind Jews in educational mobility but ahead of all other denominational groups. On occupational mobility (based on their own and their parents' education), they are about at the average, with the Episcopalians, Presbyterians, and Jews comprising the elite. He asks why Catholics are "able to make effective use of their parents' education to achieve disproportionately in the educational world, and then are unable to do appreciably better than average occupationally with the education they have received" (p. 55) and wonders if "there may be some discrimination which prohibits Catholic admission to the higher levels of professional and occupational success" (p. 56). He also examines mobility rates by ethnicity: discrimination "seems to be subtle and perhaps not all that harmful among the Irish, but rather blatant against southern and eastern European Catholics" (p. 63).

Parental occupational level does not constitute Greeley's baseline of occupational mobility. If the parental educational levels were especially low, the data could be interpreted simply as indicating that the offspring have secured the requisite education and are now at the occupational mean. As already noted, Catholics excel in income; but family rather than individual income is the measure, and this figure could be inflated by multiple incomes. Also, a disproportionate number might earn union-based wages and be employed in well-paying skilled labor or in the building industry. Given his measures, the data do not necessarily point to discrimination.

The findings on the graduate school experiences of the NORC 1961 college graduates also relate to the first theme. These and other data indicate that "a younger generation of Catholics have found no real obstacle between their religion and successful pursuit of academic careers" (p. 85). How does this mesh with his overall argument on discrimination? Anyway, these NORC data are very contrary to the 1950–61 (and earlier) data presented by Kenneth R. Hardy ("Social Origins of American Scientists and Scholars," *Science* 185 [August 9, 1974]: 497–506). Hardy argues that "there is a set of cultural values that promote scientific and scholarly activity . . . and the antithesis of these values is found most clearly among those groups [including Catholics] who are least productive of scientific and scholarly workers" (p. 503). Hardy does not refer to any of the Greeley findings presented earlier in the *American Journal of Sociology* and elsewhere, and he does not consider the possible impact of the variable generation-American on such old data. Hardy's bias is good evidence in support of Greeley's contention that prejudice and stereotypes of American Catholics still exist. Greeley's book deserves a wide reading.

The Prophetic Clergy: Social Activism among Protestant Ministers. By Harold E. Quinley. New York: John Wiley & Sons, 1974. Pp. ix+369. $14.75.

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Max Weber's distinction between priest and prophet is well known to social scientists. The priest preserves the doctrines and the practices and keeps the religious organization going. The prophet denounces immorality, personal and social, and inspires people to change and renewal. The prophetic clergy of this study are the social activists of the 1960s, the new breed (called "new bread" in the index) among Protestant parish ministers of nine mainline churches in California.

The prologue to 11 chapters of careful research and analysis is a quick updating to the 1970s of the contemporary "mood" of the church, its ministers and lay members. The generalizations of the prologue comprise the current conventional commentaries about churchly conservatism and retrenchment, and they suggest that Jeffrey Hadden's *Gathering Storm in the Churches* has now abated. Even though the liberal activists have retreated and the orthodox conservatives have advanced, the author opines that "the political future is likely to be influenced more by liberal clergymen than by conservatives" (p. 20).

The social activists had also to carry on their priestly tasks of maintaining the parish, performing the conventional functions of the ministry, satisfying the religious need of their people. Herein lay the dilemma of the clergy, as well as of their churches. If they failed to speak out against social evils they had betrayed their vocation of moral truth, spiritual ideals,

and human values. If they did denounce the evils of the day they were accused of political meddling. This is both a personal and an institutional dilemma. It is the problem of comforting versus challenging that Charles Glock investigated among the Episcopalians in the 1950s.

How can you tell a prophet from a priest? Before making this distinction, the author studies the theological beliefs of the ministers and places them in five categories on a scale graduated from modernist to traditionalist. Among the nine denominations represented by these 1,580 ministers, the Methodists and the United Church of Christ have the largest proportion of modernists, while the Southern Baptists and the Missouri Synod Lutherans contain the most traditionalists. Chapter 2 analyzes their religious beliefs, and chapter 3 relates these beliefs to social attitudes.

The main criteria for the measurement of social activism are spelled out in the fourth chapter. In California during the 1960s there were three areas of social conflict on which clergy, as well as laity, took sides. One was the Vietnam War, the second was the problem of migrant farm workers, and the third was the referendum on fair housing legislation. The prophetic clergy were those who took the liberal stance on these controversial issues. They were doves on Vietnam, supporters of the grape strike, and opponents of Proposition 14.

Factor analysis was employed to devise scales (pp. 328–29) of clergy attitudes and behavior, and the respondents were distributed in six categories, from the most active to the inactive. In chapters 5 and 6 a large number of other pertinent variables are related to these categories. In the next three chapters (7, 8, 9) the beliefs of the laity and their reactions to clergy activism are discussed in some detail. "As expected, we find Protestant laymen to hold generally conservative views on ethical and political issues and to act as a brake on the development of a more action-oriented church" (p. 260).

The final chapters deal with denominational leadership in relation to clergy activism and with the general Protestant response to the social problems of the 1960s. Quinley's careful handling of the empirical data is accompanied by penetrating theoretical analysis of the problems emerging from the data. He demonstrates a level of theological and historical knowledge that is relatively unusual among sociologists who do empirical research in American religion.

The deeply religious clergyman who is also a social activist has to be puzzled by studies of this kind. The findings of this and similar surveys indicate that "a commitment to a critical, ethically involved church is more likely to be forthcoming from the modernist clergy" (p. 40). It is as though these prophetic clergy jettison the basic beliefs of Christianity and find their motivation for reform outside of religion. This seems to be saying that the "weaker" a man's theology, the stronger his determination to attack social problems. If this is the case, one wonders about the many social reformers who found their inspiration and motivation in the sacred Scriptures and in solid theological orthodoxy.

The Political Pulpit. By Roderick P. Hart. West Lafayette, Ind.: Purdue University Press, 1977. Pp. 141. $3.50 (paper).

Calvin Redekop

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In recent times the scientific study of religion has shown us that various paradigms can be entertained, each having a certain validity and utility. In some circles there is still considerable hope that the strictly scientific paradigm will go far in defining religion (for example, Ralph Wendell Burhoe, "The Phenomenon of Religion Seen Scientifically" in *Changing Perspectives in the Study of Religion,* ed. Allen W. Eister [New York: Wiley, 1974]). Other scholars are not so sure; some are suggesting that other paradigms may be just as fruitful.

It is in the area of the relationship between religion and the nation-state that some of the greatest confusion, interest, and emotion is expressed by scholars. The bicentennial celebration with its fallout has itself provided enough grist for the intellectual mill for the next half century. Most volumes that have appeared with the national celebration as backdrop have been produced from a normative position—either denouncing the "prostitution" of religion as the unfortunate result of its being tied in with the government and state or demanding that the nation become more thoroughly permeated with the particular brand of religion the author is peddling.

The book under review comes in the aftermath of this era and attempts to provide another perspective on the vexing problem—namely, what is the nature of "civil religion in America." Hart proposes that a rhetorical paradigm is probably more useful and productive in understanding the connection and interplay between religion and (national) society than a more "substantive" one. He begins by criticizing the celebrated Bellah "civil religion" article (1967), which reified a concept by making civil religion a thing when it was not. By all accepted sociological canons, what Bellah defines as a new civil religion is not possible, since religion by definition includes institutional aspects such as the cultic, leadership structure, set of norms, correct beliefs, etc. (p. 36). Hart therefore concludes that Bellah has reified a concept and "created" a civil religion when he would have been much better served if he (Bellah) had used the term "civic piety," which is attributed to John Wilson (p. 35).

The author then provides an alternative paradigm—that of an American civic piety, which is an expression of an understanding between the United States government and organized religion. The essence of this paradigm is that both government and religion recognize the need for the other, are dependent on each other, and need to come to some polite accommodation for coexistence. This position allows for the integrity and existence of each, but also indicates what the relationship of each shall be to the other. The actual terms of the "contract" state that both parties to the contract are entitled to exert influence on the members.

But when the actual status of the parties to the agreement is analyzed, the "essential equality" turns out in fact to be an inequality, where "the church's realm is clearly the rhetorical—it can make words about social conditions and governmental priorities, but it cannot enforce its will directly and immediately as can government" (p. 57). It is this attempt to achieve equality that produces the mirage of the "rhetorical balance." It is in the church's manufacture of pious platitudes that the "equality" is maintained. Thus politicians must pay "proper homage to America's God, but [avoid] being gushy about it" (p. 62). The author suggests that whatever real influence state and religion have on each other, the operative "rhetorical balance" lies in the "collective fiction" (p. 64) that each is reverencing the role and legitimacy of the other.

In the fourth and concluding chapter, Hart probes deeper into the nature of American civic piety by analyzing its rhetoric. He describes it as being expedient, complex, nonexistential, ritualistic, and homocentric. He concludes by suggesting that the real battles between the claims of the state and of religion in the hearts of men and women have been consciously raised to a rhetorical level, because the "casualties of rhetorical war always live to speak another day." People have realized "that public rhetoric is an incredibly potent vehicle through which the social symbolism of religion and government can be effected, maintained, and made palatable" (p. 108).

This short volume must be considered a significant and provocative contribution, both for the new insights it provides and for the new questions it raises, many of which are probably more weighty than the ones answered. Only a few contributions and problems can be raised here. Hart rightly cleans up the "civil religion" terminology, by stating that in America we in no way have a "civil religion." President Eisenhower's "faith in faith" is no more a description of a religion than a presidential prayer breakfast.

The proposition that civil government and religion need to come to some type of understanding or "contract" which allows for mutual existence is not new, but Hart's proposition that the real battle is "sublimated" into a rhetorical level so that neither side will be decimated in what appears to be an irresolvable conflict is very helpful. The fact that each society has achieved a "modus vivendi" between state and religion tells us that some type of "understanding" has been achieved, often in the form of an outright "contract" (even in Russia today there is an understanding of what rights religion and the state have vis-à-vis the citizens). And in societies where no "formal" contract exists, the rhetorical premise makes sense.

But there are problems with the book, too. Hart moves from civil religion to civic piety but does not deal with the incipient institutionalism of "American religion," which could be raised to defend "civil religion" at least in crescent form. What about all the presidential prayer breakfasts, God and Country celebrations, and such orders as the Ku Klux Klan, the States Rights Parties, the Freedoms Foundations, etc.? Civic piety as a concept does not explain away the "national religion" promoted by Billy James Hargis. Billy Graham and *Christianity Today* are two of the most

political institutions in America today, according to a leading evangelical from Latin America.

Even more problematic is the concept of rhetorical conflict. At best, it has to be seen as a transitional stage on the way to a "better" solution, since rhetoric is defined as "begging the question." If the "balance" is the relative power of two spheres maintained by rhetoric, then it can be assumed that one or both parties to the agreement are basically or covertly cynical about the relationship, and I suspect that this is close to the truth. I think Hart would have been more cogent if he had directed himself to Bellah's more important essay, "Religious Evolution," especially that section on modern religion where Bellah says, "Modern religion is beginning to understand the laws of the self's own existence and so to help man take responsibility for his own fate" (p. 85 in W. A. Lessa and E. Z. Vogt, eds., *Reader in Comparative Religion* [New York: Harper & Row, 1965]). If Bellah is correct, then modern religion is in fact becoming less institutional and objective, and increasingly "each individual must work out his own ultimate solutions and . . . the most the church can do is provide him a favorable environment for doing so, without imposing on him a prefabricated set of answers" (ibid., p. 86).

The rhetorical paradigm of the religion-nation relationship makes some sense if a static institutionalism is assumed for both of the protagonists, with the above proviso (begging the question) in mind. But if religion is indeed changing as Bellah suggests, then rhetoric might better be seen as softening the demise of religious influence.

The Awareness Trap: Self-Absorption Instead of Social Change. By Edwin Schur. New York: Quadrangle Books, 1976. Pp. viii+213. $7.95.

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During the past 10 years personal growth groups, awareness trainings, and Eastern consciousness-raising disciplines have attracted a significant number of middle-class Americans. More than a million people have taken part in activities like Transcendental Meditation, est, and Silva Mind Control. Transactional Analysis and gestalt techniques pervade psychotherapy. More than 200,000 couples have attended weekend "marriage encounters" under the auspices of several religious denominations. These activities constitute part of a "human potential" movement. Human potential techniques have been applied in drug rehabilitation, women's and men's consciousness raising, elementary education, corporate-management training, theater workshops and "alternative" styles of marriage. While much has been written of the therapeutic value of this movement, its impact on the wider society has received little discussion. With the exception of Kurt Back (*Beyond Words: The Story of the Sensitivity Training and Encounter Movement* [New York: Russell Sage, 1972]), sociologists have paid scant attention

to this cultural drift. By surveying a variety of human potential techniques and looking closely at their underlying assumptions, Edwin Schur provides a valuable contribution.

Schur discusses the central ideas of this "awareness craze" in the first part of his book. He reviews an impressive number of books about a wide variety of disciplines including encounter, Transactional Analysis, biofeedback, bioenergetics, primal scream therapy, gestalt awareness training, Transcendental Meditation, reality therapy, and Zen. According to Schur, the common purpose of these activities is to encourage openness and expressiveness and to facilitate a return to the "natural spontaneity" of childhood, life in the "here and now," discovery of the "real" self, attainment of peak experiences and intuitive modes of consciousness.

Schur goes on to criticize the social implications of this movement. He contends that one of the movement's dominant themes, "we must all accept 'responsibility for ourselves,' " promotes complacency for those who have succeeded and resignation and self-blame for those who have not (p. 4) He warns that this ideology's preoccupation with the individual and emphasis on personal growth could "dilute our already weak feelings of social responsibility" and "push our society in highly undesirable directions" (pp. 7, 2). "While the movement provides the middle class consumers with an attractive new product," he writes, "attention is diverted from the more serious problems that plague our society—poverty, racism, environmental decay, crime, widespread corporate and governmental fraud" (p. 7).

In addition to raising the possibility that the movement will convert public issues into private failures, Schur analyzes a number of its techniques, pointing out confusing terminology, logical inconsistencies, and exaggerated claims. He shows, for instance, how vaguely defined words such as "authenticity," "liberation," "natural," and "self-regulation" constitute the core of a confused human potential ideology. At times, Schur adds to the confusion. He does this by reducing the gestalt directive, be aware in the "here and now," to "buy now, pay later," by equating responsibility with causality, and by suggesting that this movement can be understood in terms of the "positive thinking" of Norman Vincent Peale (pp. 52, 76).

The second part of the book covers specific applications of this movement, as in open marriage, drug-abuse treatment, gay "rap" groups, "radical" therapy, learner-directed school curricula, and women's consciousness-raising groups. Schur warns of present and potential abuses, fearing that women may become further stereotyped as experts in emotional-expressive behavior through their participation in the movement. He reminds us that gay pride is ineffectual without gay power. He notes that dependence on Synanon often replaces dependence on drugs (p. 171). He argues that the techniques of "values clarification" as a content-free skill tends to encourage a value relativity in which "anything goes" (p. 187). He blames the awareness movement for condoning self-help books which teach manipulative techniques such as self-assertion through intimidation and the reading of body language to facilitate sexual encounters (pp. 83, 118).

Schur does see some aspects of the human potential movement in a posi-

tive light. He acknowledges that we could all use some loosening up, and men especially could benefit from being encouraged to express their feelings more openly (p. 42). He says that some of the methods "may have specific uses, for example in breaking down stereotypes, or in strengthening self-respect and personal assertiveness," but he goes on to say they "do not begin to deal with institutionalized oppression. . ." (p. 4).

While this book provides useful data and criticism of an important cultural movement, it does not substantiate its major point, that the movement seduces people into self-preoccupation at the expense of commitment to social change (p. 183). Schur supplies little evidence that individuals (or groups) in this movement increase their disregard for social problems or necessarily lack a social frame of reference. He ignores the writings of human potential advocates such as Erich Fromm, Paul Goodman, Wilhelm Reich, and Charles Hampden-Turner, who do have a wider social perspective. But more importantly, he does not examine the attitudes and behavior of participants that would supply evidence for his argument. This might have been done either through participant observation or interviews with participants.

In addition, the book contains two highly questionable assumptions: that the movement does not address any serious needs of its relatively affluent participants, and that participants would return to the serious task of organizing for social change if they gave up their human potential activities. Had Schur asked why this movement has gained such widespread appeal and explored the poverty of everyday life to which participants seek alternatives, he might have concluded that more than just recreational needs are being addressed. As to whether the movement converts potential change agents into pleasure seekers, an examination of people's actions and not just the writings of movement gurus would be necessary.

While Schur does not make explicit his own theory of social change or the psychological dynamics that mobilize it, he appears to assume that middle-class guilt and righteous indignation are key components. He infers that the "value-free" techniques of the movement and the emphasis on replacing collective guilt with a sense of personal responsibility could erode the moral tension that motivates social change. The book does not answer the general question of whether guilt is an effective motivator of sustained commitment or the particular one of whether guilt reduction in the human potential movement reduces the sense of social responsibility.

Perhaps the most fundamental difference between Schur and the advocates of expanded awareness lies in their respective views on whether there is a "real" or "true" self beyond the "social" self or selves. Schur considers the social self the only self, "built up out of an accumulation of responses and counterresponses. . . " (p. 81). Most human potential advocates believe in a "true" self, which is sometimes described as what remains after the social selves have been peeled away or as a universal context in which social selves and ego processes occur, rather than an isolated thing in itself. Schur candidly admits that he "doesn't know exactly what to make of" this "higher" consciousness, though he suspects it may encourage irrationality

and detachment (pp. 54–57). Yet some social movements have been led by persons whose experience of true self and cosmic identity led to a sense of interdependence, compassion, and involvement with the world around them—Gandhi and Martin Luther King, for example. The concrete outcome of experiencing a transcendent or real self remains an open question for sociological analysis.

Despite its shortcomings, *The Awareness Trap* is timely, provocative, and very readable. The clear and fervent writing exemplifies the human potential movement's prescription to be open and expressive. The many astute observations more than make up for a somewhat meandering organization. Schur supplies well-documented references and an index, something often overlooked in books written for lay audiences. He provides a valuable service in bringing this movement into sociological focus. His difficulty in mustering evidence demonstrates the need for systematic research, particularly longitudinal studies of the individuals who come into contact with a movement that continues to pervade American life.

The Consciousness Reformation. By Robert Wuthnow. Berkeley: University of California Press, 1976. Pp. x+309. $12.50.

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All the elements of an excellent book are here—an interesting question, good data, and a sophisticated theoretical perspective. Yet in the end the elements do not add up to a convincing explanation of the sources of contemporary social experimentation.

Based on a 1974 survey of 1,000 San Francisco Bay Area residents (oversampling youth aged 16–30), Robert Wuthnow's *The Consciousness Reformation* seeks the sources of countercultural experimentation in "some deeper shift in the basic understandings of life that give us our sense of meaning and purpose" (p. 32). Decline in two traditional meaning systems —"theistic" and "individualistic"—and the emergence of new meaning systems—"social scientific" and "mystical"—explain the increasing tolerance for various forms of social experimentation.

The argument is theoretically complex. "Meaning systems" are meant to be clearly distinguished from the more usual sociological concern with values, attitudes, and behaviors (pp. 192–93). Drawing on theorists from Weber and Dilthey to Alfred Schütz, Wuthnow argues (in chap. 2, "Consciousness and Meaning Systems") that the fragmented, limited character of everyday reality is supplemented by "symbolic expressions of wholeness and transcendence" (p. 73) that help "maintain the integrity of society" against incomprehensible conditions such as injustice, suffering, and death (p. 78). The analysis of specific meaning systems is well grounded historically. Theism is traced to the Calvinist view of human nature and puritan

social control in colonial America (pp. 88–94), while the discussion of mysticism ranges from Wordsworth and Whitman through the American transcendentalists (pp. 123–34).

Wuthnow's findings are remarkably strong and consistent. Those respondents with social scientific or mystical meaning systems (or a combination of the two called "modern") are dramatically more likely to favor every kind of experimentation—political, economic, religious, family, or leisure—than are those who hold theistic or individualistic world views (p. 148, table 9). At the extremes, only 1% of those with a "traditional" meaning system (equally theistic or individualistic) score high on a summary measure of experimentation, while 72% of the holders of a "modern" meaning system do so. These relationships are only slightly reduced when controls are introduced for age, marginality, "cognitive sophistication" (a measure including education and cultural values), and liberal background (pp. 178, 182, and 187). Indeed, Wuthnow demonstrates a real shift in social values. When marginality is controlled (marginality includes various aspects of an unsettled stage of life, such as being a student, unmarried, or without children), younger people are still more likely to hold social scientific, mystical, and modern meaning systems. Therefore, he argues, the meaning systems now held disproportionately by younger people represent a genuine generational shift in attitudes that will not evaporate as today's young people mature (pp. 154–264).

Yet despite its strengths, the book contains serious flaws which in the end make it unconvincing. The most serious issue concerns measurement of the four meaning systems. Theory and measurement simply do not match. For example, after a sophisticated argument that social scientific meaning systems legitimate social experimentation by denying transcendent meaning to existing social patterns and "locat[ing] evil in social structures which are somehow realities in themselves" (pp. 119–20), Wuthnow measures the social scientific meaning system by agreement with three items: "I believe forgotten childhood experiences have an influence on me," "Suffering is caused by social arrangements which make people greedy for riches and power," and "Man evolved from lower animals" (p. 142). While "face validity" is in the eye of the beholder, and the book's appendix justifies this choice of items precisely on grounds of face validity (p. 240), I am dubious.

Even more damaging is the great similarity between measures of the independent and dependent variables. A mystical meaning system, for example, is measured by having "experienced the beauty of nature in a deeply moving way" and agreeing that "it is good to live in a fantasy world every now and then" and that "new insights" about oneself have had a strong influence on one's life (p. 143). Whether or not these three items measure mysticism, we should hardly be surprised that mystical respondents score high on "leisure experimentation"—an index composed of 10 items, including three about using or advocating drugs (no doubt related to liking to live in a fantasy world now and then), four about "getting to know your inner self" or "your body" and participating in or

being attracted to personal growth groups (clearly related to "new insights" about oneself), and two on valuing nature (obviously related to having had a deeply moving experience of nature). Similarly, individualism includes valuing hard work and blaming poverty on unwillingness to work, obviously negatively related to several measures of political and economic experimentation (being politically radical or liberal or favoring "a guaranteed minimum wage plan for the poor," for example) (pp. 140 and 146). The theism measures all involve belief in God, while "religious experimentation" is measured by four items, of which three tap low rates of church attendance or prayer.

These problems of measurement are exacerbated because the categorization of respondents into four meaning-system categories (seven, counting the combined types) required a respondent to score lower on the three other meaning systems to be scored as a likely adherent of any one. Thus, the measures of the four meaning systems are hopelessly entangled, so that contamination between measures of any of the meaning systems and any of the types of experimentation casts doubt on the entire analysis. Table B-21, appendix B (p. 254) presents the relation between each meaning system and experimentation, holding the other meaning systems constant. Here we see that while each of the meaning systems retains some independent effect on experimentation, the strong effects are between theism and religious experimentation, individualism and political and economic experimentation, and mysticism and leisure experimentation, effects more than accounted for by the overlap in measures of the dependent and independent variables. This book is thus an interesting empirical study of contemporary attitudes and behaviors. But it seems to measure various aspects of experimentation itself, without capturing the larger world views which might constitute explanations of these attitudes.

Artificially strong and consistent empirical findings require strained theoretical arguments to derive from each meaning system a set of logical implications about receptivity to social experimentation. Wuthnow must argue, for example, that since puritanism had conservative social implications, contemporary theism also does so (pp. 88–90), that individualism is a conservatizing meaning system, and that mystical and social scientific meaning systems are inherently conducive to social experimentation. While he notes counterarguments, acknowledging that mystical world views are frequently associated with apathy in the social sphere (p. 128) and that individualism can imply "a spirit of non-conformity, of tolerance for a wide range of social experimentation and diversity" (p. 103), he is forced to make one implication dominant in each meaning system.

This raises one more theoretical difficulty. Wuthnow attempts to explain the flowering of social experimentation in the 1960s and 1970s as the outgrowth of very long-term, gradual shifts in meaning systems. Theism and individualism, for example, have been declining since the 18th and 19th centuries (pp. 94–97 and 107–9). Yet such a model hardly seems to account for the social conservatism of the 1950s or for the dramatic development of the counterculture in the 1960s.

This book is valuable for its findings about contemporary shifts in attitudes and behavior and for the theoretical framework in which it places the problem of the counterculture. But the book's failure to bring theory and data together is disappointing, since the meaning-system perspective offers promise of a new way to conceptualize the cultural patterns lying behind specific values and attitudes. Wuthnow convinces the reader of the need to study meaning systems, the "forces identified as controlling and ordering reality" (p. 192). But his empirical strategy is inadequate to test, or even explore, his theoretical model.

Sectarianism. Edited by Roy Wallis. New York: John Wiley & Sons, 1975. Pp. 210. $13.95.

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The focus of this volume is broader than its title suggests. While the essays in parts 1 and 2 of the book deal with conventional topics in the sociology of sectarianism, part 3 explores the sectarian dimensions of nonreligious contexts, or what is somewhat awkwardly referred to by the title, "Near-Sectarianism in the Secular Domain."

Editor Roy Wallis contributes three of the nine essays in the anthology. What is, perhaps, the most significant is entitled "The Cult and Its Transformation," in which he argues that the distinguishing characteristic of sect organization is epistemological authoritarianism. Cults are described as having no recognized legitimate locus of attributions of heresy beyond the individual cult member, while sects are said to possess such a source of authority. The transformation of a highly individualistic cult into a cohesive sectarian group must involve the expropriation of authority. "This centralization of authority will be legitimized by a claim to a unique revelation which locates some source or sources of authority concerning doctrinal innovation and interpretation beyond the individual member or practitioner, typically the revelator himself" (p. 43).

A number of the papers discuss new religious movements which have been receiving considerable attention of late in the popular press: the Unification Church of Reverend Moon, the Hare Krishna movement, and Scientology. Anthropologist Francine Daner, for example, discusses life in an ISKCON (International Society for Krishna Consciousness) temple in the context of Goffman's concepts of a total institution and delineates four phases in the socialization process of the devotee. Those interested in a more complete discussion of conversion to Krishna Consciousness should consult Daner's recent ethnography, *The American Children of Kṛṣṇa* (New York: Holt, Rinehart & Winston, 1976).

James A. Beckford contributes a comparative study of the Jehovah's Witnesses and the Unification Church. He discusses the mechanisms by which both groups attempt to overcome the problems of what is termed

an "enrollment economy." The Jehovah's Witnesses adopt mass-movement strategies; the Unified Family (the local name for Moon's followers), with its paternalistic leadership patterns, employs a proselytizing strategy which is highly personalistic and communal in thrust.

Another of Wallis's own contributions to the volume is a study of societal reaction to the controversial Church of Scientology. As a result of continuing criticism from the press, governmental agencies, disgruntled ex-members, relatives of Scientologists, and the medical profession, the organization has developed a defensive posture evidenced by frequent litigation. The author describes the authoritarian stance and the ambiguous status of this sect, which has been "viewed in a manner approaching *moral panic*" (p. 86). The group has been characterized as engaging in a form of hypnosis or brainwashing. The severity of societal reaction to Scientology has resulted in some attempts to modify its image. The movement continues to experience persisting alienation, and the author concludes that ". . . there is some ground for believing that Scientology's denominationalization, its rapprochement with conventional reality, may be, as yet, only public-relations officer deep" (p. 110).

In an essay which communicates very little which is new, John McKelvie Whitworth asserts that the life chances of communitarian groups in contemporary industrial society are slim. He reviews the factors which are likely to have an adverse effect on the maintenance and growth of such groups. The focus of his discussion is the counterculture of the 1960s. Unfortunately, the author makes no reference to the relative success of such communally oriented sects as the Children of God and the Unification Church.

Wallis's lead essay is a case study of the emergence and development of a new magicoreligious group, the Aetherius Society. Employing Weber's seldom-used concept of the mystagogue, Wallis traces the career of Dr. George King, the founder-leader of what is essentially a flying saucer sect. King's early psychic and occult inclinations provided the base for a cultic career culminating in his assuming the role of chief transmitter of the messages of the "Cosmic Masters." His success has been attributed to "his ability to monopolize access to his own charismatic legitimation" (p. 30).

The final section of the book is devoted to an analysis of the sectarian disposition of such groups as therapeutic communities, extremist political movements, and organizations consisting of former mental patients. The contributors—Barry Sugarman, Roger O'Toole, and R. K. Jones—are generally successful in demonstrating the similarities to religious sectarianism.

Despite its price, this useful little volume will be of interest to students of social movements as well as to those whose focal concern is sectarianism per se.

Religion in Canadian Society. Edited by Stewart Crysdale and Les Wheatcroft. Toronto: Macmillan Co. of Canada, 1976. Pp. xi+498. $19.95 (cloth); $12.50 (paper).

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It is to be hoped that someday students will rebel against being assigned books of readings in courses they are taking. Such books may have served a useful purpose in the late 1960s when the vast increase in university enrollments imposed heavy burdens upon university libraries and left the instructors of many courses without adequate reading material for their students. Unfortunately, publishers discovered that books of readings made money, and for university instructors it became a relatively easy way of building up a list of publications for their curricula vitae. I have yet to find a book of readings which made really instructive reading.

Certainly, *Religion in Canadian Society* is no exception. I find it difficult to say much that is favorable about this book. In addition to the introduction, there are 33 pieces in the book. Some are no more than seven or eight pages long; most of them run around 9, 10, or 11 pages in length. Though the arrangement of the readings in two parts, and the further division of these parts into sections, appear to give some sense of order to the whole, there is almost no thread of continuity which ties one reading to another, even within the various sections. If any satisfaction is to be derived from reading this book, it must come from what the different pieces have to offer taken by themselves. Some make good reading, though almost all of them were seriously damaged by being abridged. At least a few of the readings were scarcely worthy of being included in a scholarly publication.

The lengthy introduction might have done something to redeem the book. What clearly was called for here is a discussion of the way in which religion and the institutions of religion have developed within the structure of the Canadian society. Among other things, one certainly would have expected the editors to point out the very significant differences between religious developments in Canada and the United States. Instead, the first 41 pages of this introduction are given up to a pedantic review of theories of religion and methods of research, a discussion that might have been appropriate as an introduction to a book on the sociology of religion. In the remaining 15 pages, an outline of the arrangement of readings is offered. In neither the introduction nor the readings is the student given a hint of the major forces shaping the character of the Canadian society and the place of religion in that society, either as the society developed in the past or as it is structured today. It is, indeed, barren ground over which all the various readings in this book are scattered. The student may end up knowing something about "The Kwakiutl of British Columbia," "The Religion of the Micmac Indians of Restigouche," "The Impact of Clerical Attitudes and Liberalism on Ecumenism," "Adolescents, Religious Affiliation and

the Use of Drugs," but he will scarcely know more than he did before reading the book about religion in Canadian society.

Religion and Political Modernization. Edited by Donald E. Smith. New Haven, Conn.: Yale University Press, 1974. Pp. x+340. $16.50.

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This is a valuable and welcome volume on an important, if somewhat neglected theme. It consists of a collection of papers presented at a conference in Honolulu in 1971, and is edited, with an "introduction and overview," by Donald E. Smith. Most of the contributors draw on Smith's analytical formulations on religion and political development for their conceptual orientation. This makes for the coherence of the presented materials, which are accordingly arranged into three sections: "the secularization of polities," "the role of religion in mass politicization," and "the religious legitimation of change."

The analytical framework put forward by Smith is worth sketching. Adopting Pye's characterization of political modernization as the process of increasing differentiation, equality, and capacity (corresponding to "polity secularization," "mass politicization," and "developmental capacity expansion"), Smith relates variations with respect to these three themes to his schema for classifying the major religious systems in terms of a pair of cross-tabulated dichotomies. These dichotomies divide the four major religions under consideration with respect to the structure of religious organization—into "church" and "organic"—and with respect to the ideas contained in the religious doctrine about the course and meaning of history—into "historical" and "ahistorical." According to this classification, Hinduism is an ahistorical organic religion, Islam a historical organic religion, Buddhism an ahistorical church religion, and Catholicism a historical church religion.

What Smith terms "polity expansion secularization"—that is, "the expansion of the political system into areas of society formerly regulated by religion" (p. 8)—emerges as a ubiquitous trend. Everywhere, with the rise of bureaucratic states, major areas of social life, most notably law and education, have been secularized (the major exception being the Catholic educational system in Latin America, which has shown continued vitality and impressive expansion).

Against this universal trend, Smith's formulations regarding the structure of the religious institution help us come to grips with variations in the extent and character of polity secularization and in the likelihood of politicization of religion as a result of mass mobilization. The cross-national evidence supports two of the three sets of inferences drawn by Smith.

The first set concerns the consequences of the separation of religious and political organizations—what Smith terms "polity-separation seculariza-

tion"—on ecclesiastical authority. In "church" systems, "the church (whether Catholic or Buddhist) continues to make sense religiously" after the shrinkage of its societal control, while "organic" religious systems (whether they rest, like Hinduism, on the cornerstone of a sacral social order, or, like Islam, on the operation of a sacred law) have a far greater problem of redefinition and face a greater challenge to their religious authority and prerogatives. The pitiable plight of the religious institution in contemporary Islam can be mentioned in support of this contention: the paper on secularization in Egypt shows how the consequences of the separation of religious and political organizations have been far more disastrous for the religious institution in Islam than in Christianity. While the authority of the Catholic church in the interpretation of religious doctrine has remained intact, in Egypt the state has now come to arrogate to itself the chief responsibility for the reinterpretation of Islam (and, until recently, the formulation of the message of Islamic socialism).

The second set of propositions relates to the topic of religion and mass politicization, which is the subject of the second and the most interesting part of the book. What runs most blatantly against the Parsonian models of modernization is the widespread resurgence of the religious factor in politics in the Third World. Modern societies are political societies which incorporate the masses in some form or other. With the emergence of modern—that is, political—societies in many parts of the Third World, sacred symbols have acquired political significance and have been used as means of mass mobilization by lay and clerical intellectuals. In the West, the (by and large) gradual incorporation of the masses into society went hand in hand with the gradual secularization of culture. In the Third World, the incorporation of the masses has taken place suddenly, while their culture is hardly secularized at all (*pace* Inkeles). Here we have an important cue as to why religion has not been politicized in Latin America, as in other parts of the Third World: the political culture of Latin America has long been secularized under the impact of the Enlightenment. The Enlightenment also gave birth to the phenomenon of anticlericalism, which continues to gather momentum and which, as Smith and others emphasize, has no close parallel in Muslim, Buddhist, or Hindu countries.

Mass politicization usually entails nationalism; and the politicization of the religious component of national identity can give rise to politics of religious nationalism. Colonial experience appears as a decisive factor in the politicization of religious identity, which is found in India, Pakistan, Egypt, Ceylon, and Burma and which is absent or negligible in countries which were not directly colonized: Thailand, Turkey, and the Latin American countries.

Having mentioned secularization of culture and politicization of the religious component of national identity as two specific historical parameters, we can now consider the consequences of the structure of the religious system regarding the likelihood of politicization of religion. According to Smith, "the church systems . . . are better equipped to respond to the whole range of problems posed by rapid social change. . . . Organic systems, with

their relatively low level of ecclesiastical organization, respond much less effectively and coherently; *laymen* must undertake many of the tasks of resisting secularization, organizing political forces, and reformulating social doctrine" (pp. 6–7; emphasis added). To expand the above statements, the secularization of the polity is likely to go hand in hand with the politicization of religion where there is no church (*a*) to enforce ecclesiastical discipline and prevent the entry of the clergy into the political arena, and (*b*) to act as authoritative interpreter of the religious doctrine. Under these conditions, lay and clerical intellectuals are likely to find the appeal to religious beliefs and symbols for political advantage irresistible and, either as state functionaries or as party politicians, draw upon the sources of their religious tradition in order to create political "myths" and ideologies. Where a "church" exists and continues to enforce ecclesiastical discipline, religion is unlikely to be politicized.

In Latin America, the impact of Catholic priests on mass politics has indeed been marginal (p. 201). Catholic political parties have usually been total failures. In the rare instances where they have had a measure of success, this had been due to particular local conditions, and not to their Christian ideology (p. 219). The one serious radical Catholic political program, the "Conscientization" movement, leading to the emergence of "people's priests," has developed outside the framework of the Catholic church. (Its originators were, significantly, a group of laymen under the leadership of Paulo Freire [p. 292].) The conservatism of the Catholic church has resulted in the "almost sectarian quality of New Catholicism" (p. 299); the politically active Catholics "now believe that the real church of the future will be a minority" (p. 296).

Let us turn to the other "church" religion, Buddhism. In Thailand, where independence assured the state's continued control over the Sangha and its enforcement of monastic discipline throughout the colonial period, Buddhism never became a major political issue and the Sangha remained largely unpoliticized (p. 55). Similarly, the uninterrupted government control of the Sangha in Cambodia prevented the political activity of monks (p. 164). By contrast, in Ceylon and, especially, in Burma, Buddhism has been highly politicized. In his paper on this subject, Bechert emphatically attributes the politicization of Buddhism in both instances to the complete removal of government control of the Sangha during the colonial period and the consequent serious impairment of monastic discipline.

In the "organic" religious systems, the politicization of religion, mainly by lay intellectuals, meets with fewer obstacles. Hinduism was effectively politicized by Gandhi. Islam has been politicized both by lay intellectuals with fundamentalist tendencies (e.g., the Muslim Brotherhood) and by the functionaries of Middle Eastern states (the religious elite—the *'ulama*—have remained apolitical) (p. 90).

Finally, Smith's third set of propositions relates the historical/ahistorical character of world religions to the possibilities for "the religious legitimation of change." In my opinion, this last set of propositions is not supported by evidence and seems basically misconceived. Smith states, "His-

torical religious systems have important advantages over ahistorical ones in attempting to develop religious ideologies of social change . . . [and] can build convincing bridges to secular ideologies such as socialism" (p. 7). This view does not allow for the flexibility of such "ahistorical" traditions as Hinduism and Buddhism. In fact, the evidence suggests that both "historical" and "ahistorical" religious traditions are capable of generating such ideologies. Even though the notable case of Buddhist socialism is not discussed, the paper on the *Sarvodaya* ("welfare of all") movement in Hinduism does not seem to support Smith's assertion; and Smith himself admits that "a concept of linear history is not the only basis for a religious ideology of change" (p. 24). More fundamentally, Smith's emphasis on the ideological justification of social change is misguided in that the presumed causal efficacy of such ideologies (as distinct from specific religious beliefs and practices, which do affect men's motivations) in bringing about modernization is highly questionable. The last section of the book is in fact about partial transformations of religious traditions as the political sphere becomes public in the Third World, giving rise to the need for political ideologies and creeds. To present this phenomenon as the contribution of the world religions to "developmental capacity expansion" seems inadequate and misleading.

Despite this one reservation, *Religion and Political Modernization* can be highly recommended. It contains much insight and a good deal of interesting information about a phenomenon of major importance in the contemporary world, cast into an acceptable theoretical framework.

Rebels and Colleagues: Advertising and Social Change in French Canada. By Frederick Elkin. Montreal and London: McGill-Queen's University Press, 1973. Pp. xii+227. $12.00.

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I was born and raised in the province of Quebec, went to school and later taught in that province, and my father sold advertising for the *Montreal Gazette, La Patrie,* and other English and French publications between 1910 and 1950. Such a background may or may not qualify me to review Professor Frederick Elkin's perceptive study of advertising in Quebec and its part in the frequently incomprehensible entanglement of French-English relations in Canada.

When the designers of the goods, the writers of the advertisements, and those who draw the profits are English speakers (whether from Ontario or the United States), and the wage earners and users of the goods are French, some special problems arise. Elkin's book shows how these problems evolved through Quebec's Quiet Revolution. Prior to the 1960s, the main issue seemed to be translation of Madison Avenue slogans. English advertisers complained that French Canadians could not agree with one another on the

right French for such catchy if not elegant English as "crispy-crunch" and "smackeroo." For French observers the translation was often wildly wrong, as when *lavage d'auto* ("car wash") appeared as *lavement d'auto* ("car enema").

More basic than choice of words are cultural differences. French Canadian homes do not serve up pancakes on Mardi Gras as English ones do on the corresponding Shrove Tuesday; plum pudding is no part of the traditional French Canadian Christmas dinner.

By the early 1960s proper speech had become a national goal to French Canadians. They sought to speak "international" French rather than the traditional rural-tinged dialect containing archaisms from the 17th century mixed with anglicisms. "Bien parler, c'est se respecter" ("By speaking well we show self-respect") was one of many expressions of this movement. But in advertising such language questions were swamped by the issue of whether there should be translations at all rather than having the advertisements written in French to start with, and even why it should be advertising of English goods rather than French.

The professionalization that had come earlier to American advertising had its counterpart in French Canada at this time. To convert clever tricks into professional techniques that could be taught and learned, and applied routinely, to move from hucksterism toward responsible, even ethical, rules and procedures was the aim of French Canadians, as of others involved in advertising.

When this had been attained, and original advertisements were being designed in Quebec, there was still the matter of clearing them with the head office, perhaps in Toronto. The chief there might ask a French Canadian secretary who worked in his office to look over an advertising campaign planned by an agency in Montreal; if she made changes in the French there was trouble when these changes got back to Montreal—the professionals there were outraged at the thought of their work being scrutinized in this way. The answer, as in other domains, was pressure toward a more complete separatism within the advertising field.

Results show in the content of ads, which now come to include untranslatable puns. *JELLO à la bouche* is for the French reader a play on *J'ai l'eau à la bouche* ("my mouth waters"); *SHELL que j'aime* a play on *Celle que j'aime* ("the girl I love"). I do not expect these examples of creative advertising to impress the reader more than would the corresponding puns in English and only quote them as examples of the indigenous touch required in Quebec.

The limits on such separatism were shown when an English-owned company, Dow Breweries, produced a beer named Kébec, an old Indian name from which Quebec derives, for the Quebec taste and market and promoted it in advertisements rich in such symbols of French Canada as Jean Talon and the provincial flag. Here if anywhere was an attempt by an English concern to recognize the rising French Canadian nationalism. Imagine the surprise of the advertisers when the response was public outcry (in one statement, "stupefaction and profound disgust") at the use of

sacred national symbols for the profane business of selling beer! Sacred symbols are of course used all the time in advertising; only when they are French and used to sell an English product are the bounds exceeded. Dow Breweries quickly withdrew the offending expressions, but the damage was already done, and shortly afterward *La Bière Kébec* disappeared from Dow's list of products.

Advertising has to attract the attention and enter the minds of average people. It necessarily appeals to greed, social climbing, and the wish to identify with power. The competition among advertisers generates the needed skills, and the resulting devices to capture the common mind are highly distasteful to intellectuals. Yet the news media have to be supported one way or another; the main alternative to advertising is support by readers and viewers, which makes them expensive and elitist, or else by government or a political party. Comparison of the *New York Times* and *Pravda* will quickly tell what is wrong with this last arrangement. Because the media of a free society, and particularly the advertising they contain, have to be sensitive to popular moods, they are an especially good indicator of developing nationalism. Elkin's analysis for Quebec, in the tradition established by Everett Hughes in writings extending over half a century, could well be a model for sociologists studying other new nationalisms.

Chile: Politics and Society. Edited by Arturo Valenzuela and J. Samuel Valenzuela. New Brunswick, N.J.: Transaction Books, 1976. Pp. xv+399. $14.95 (cloth); $3.95 (paper).

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The military overthrow of the Popular Unity government in Chile on September 11, 1973 generated an unprecedented level of controversy. *Chile: Politics and Society,* edited by Arturo and J. Samuel Valenzuela, is unique among recent appearances in that the 10 essays were written, and in most cases published, prior to the military coup d'état. Yet what is remarkable is not the prescience of the individual authors, but the uncommonly useful historicity of their observations. This collection goes further than any other single work in elucidating the structural historical conditions within the Chilean society that provided constraints and ultimately the impasse to the evolution of a socialist society within a democratic framework.

Indeed, this is the editors' intent—to describe "fundamental aspects of the Chilean sociopolitical system" as "background for understanding not only Salvador Allende's accession to the presidency but also the principal constraints to his policies of basic social change within the nation's traditional constitutional framework" (p. ix). Their point is that the Allende years constituted a *coyuntura* in which a combination of strains coalesced: structural forces stemming from Chilean institutional and social history

and politicoeconomic forces arising from fear of displacement or annihilation on the part of certain groups. The editors imply that it is precisely because the Popular Unity government operated within these traditional constraints that the usual difficulties plaguing a coalition President in Chile were exacerbated by those elements of society who viewed socialism as a personal or ideological threat—hence the conflagration.

The general picture drawn by the authors is that of a highly politicized country in which the rules and benefits of electoral politics are strongly supported, private and public institutions are thoroughly linked with political parties, and the legitimacy of suffrage is unquestioned. Within the context of a highly inflationary economy with great inequalities among classes, these political characteristics made it difficult for any government to implement long-term strategies. When the Popular Unity (UP) government assumed power in November 1970, it felt at once the contradiction between eventual accomplishment of its programs and the immediate need to meet economic and social demands, particularly from those sectors of the population which provided its greatest electoral support.

Salvador Allende, in his first message to Congress, stressed the need for "new juridical norms" to meet the requirements of a people building a new society—but norms set by constitutional means. His expectation that the existing institutional structures (which reflected norms of capitalism) would reform themselves was quixotic: an impossible expectation of a President who never controlled the Congress. From his inauguration henceforth, the prospects, the very feasibility of "the pluralist way," depended on increased electoral strength in the legislature to enact constitutional modifications or in the populace to support his measures through plebiscites. Electoral strength could be increased only through short-term gains or modified consciousness of the voters.

Five of the articles in this volume consider these sources of electoral support and the role of ideology and class as mediating factors: Robert Ayres's "Unidad Popular and the Chilean Electoral Process," James Prothro and Patricio Chaparro's "Public Opinion and the Movement of Chilean Government to the Left, 1952–72," Alejandro Portes's "Occupation and Lower-Class Political Orientation in Chile," J. Samuel Valenzuela's "The Chilean Labor Movement: The Institutionalization of Conflict," and James Petras's "Nationalization, Socioeconomic Change and Popular Participation." A rather simplistic statement of ideological positions is sketched by Paul Sigmund in "Three Views of Allende's Chile." The full-fledged entry of the peasants into the political arena and the effects on urban politics are examined by Brian Loveman in "Transformation of the Chilean Countryside." The increased consciousness and mobilization of the peasants, whose repression had been essential to political stability, and the enforcement of existing legislation through agrarian reform interrupted the traditional trade-off of sacrifices in the countryside for urban programs. A background for these analyses is provided by Arturo Valenzuela in "Political Constraints to the Establishment of Socialism in Chile," with a discussion of the internal institutional constraints on long-term strategies,

including coalition politics, a public sector heavily intermingled with private groups, and the key role of the Contraloría.

The ease with which the UP programs could be combated in the economic sphere is suggested by Maurice Zeitlin and Richard Ratcliff. In "Concentration of National and Foreign Capital in Chile" they point out that control of net capital assets of nonfinancial corporations was heavily concentrated. Among the top foreign firms, American corporations controlled 85% of the assets—a fact of vital significance for the orchestration by the United States of external constraints to the regime, because private national capital held monopolies over key industries, and national and foreign capital was sufficiently mixed that a coalition of interests readily formed. Finally, in the single article dealing exclusively with externally imposed constraints, "The Invisible Blockade: The United States Reacts," Elizabeth Farnsworth et al. suggest that the forces hostile to socialist ideology could take advantage of the particular historical moment in which the depressed economy of the United States magnified the Chilean expropriations to real threats to the "defense of democracy" and the future of capitalism in the Western Hemisphere.

In a tragic sense, the articles taken together present a picture of a society that no longer exists: a vibrant democracy in which the participants and the institutions most responsive to Allende's call for "the pluralist way" have been ruthlessly curbed, if not crushed, by military rule through repression. That the forces of repression immediately and brutally struck labor, the universities, marginal urban groups, peasants, and, more generally, political parties and Congress is the measure of the important roles these institutions and classes played in Chile prior to September 1973. The discontinuity in Chilean historical tradition, so often attributed to Allende's election and program, lies in neither, but rather in the policies of the present military authorities which continue savagely to dismember the democracy of nearly one and a half centuries.

Precisely because of its openness (and vulnerability, as Camelot and recent events have demonstrated), Chile provided a fertile field for survey research, interuniversity programs, and cooperative projects for North American and Latin American scholars. The authors draw upon this considerable body of materials in their analyses. Regardless of their conclusions, the significance of this collection lies in the breadth of presentations, the unearthing of little-known surveys, and attempts to root a post mortem in historical, not merely sectarian, ground. While replete with theoretical considerations, the articles by and large avoid obfuscating the historical events and circumstances. Unfortunately, since the studies consider events only to 1972 at the latest, the hypotheses cannot be fully examined. Precisely for this reason, it would have been useful to focus the bibliography on more recent analyses. Strikingly absent from the volume are studies of the institutional constraints on Chilean sociopolitical reality posed by the Church, mass media, and the military. Certainly the armed forces as the ultimate constraint deserve separate, detailed examination; only three pages even consider their role (pp. 22–23, 132). Finally, outside the scope of this

collection is an analysis of the economic constraints counterposed against development. To complement these political and social analyses, one has to turn to Stefan de Vylder's *Allende's Chile: The Political Economy of the Rise and Fall of the Unidad Popular* (New York and London: Cambridge University Press, 1976) and Ian Roxborough et al., *Chile: The State and Revolution* (New York: Holmes & Meier, 1977)—the only volumes thus far to discuss in depth the economy and obstacles to structural transformation.

Chile: Politics and Society is a valuable introduction to the social forces of the Allende era and to the necessary and ongoing analysis of the Marxist experiment.

Japanese Urbanism: Industry and Politics in Kariya, 1872–1972. By Gary D. Allinson. Berkeley: University of California Press, 1975. Pp. xvi+276. $16.75.

Susan H. Marsh

Providence College

Kariya is a city south of Nagoya in Aichi Prefecture in the Tōkai region. In 1972 it had more than 90,000 inhabitants, and most of its wage earners were employed by the several large firms of the Toyota enterprise, which dominated the city. This study by Gary D. Allinson centers on the development of the Toyota enterprise from a modest textile and textile machinery concern in the 1920s to one of the world's largest automobile-manufacturing enterprises. In the author's view, the industrial evolution of Kariya presents in microcosm the salient features of Japan's modern economic development (p. 6), even though, as he himself points out, Kariya in 1972 was "atypical" because it was more heavily industrialized than most cities its size (p. 8). Allinson articulates the theme that economic growth altered social structure and political behavior and casts doubts on (1) remarks such as the one made of Japan by a political economist that "no nation of the modern world offers more striking testimony to the autonomy of politics for overriding economic determinism" (p. 207, n. 43), (2) the convergence theory (p. 261), and (3) the designation of Japanese society at the present time as postindustrial (p. 245, n. 1).

The author is at his best when he explains electoral politics in Kariya and the ways in which national and local politics influenced each other. Some of his findings and substantiations of others' findings by means of Kariya data are as follows.

For about five or six decades after the Meiji Restoration, the leading political figures in Kariya were drawn from virtually the same families which had governed the area in the preceding period; generally, status and wealth guaranteed political position.

The key aspects of post-World War II Japanese society actually took

shape in the 1930s. "Events between 1932 and 1945 in Japan were irrational aberrations which came to a sudden halt with Japan's surren Instead, . . . those years should be viewed as an integral and enduring of Japan's historical experience" (p. 90, n. 1). For instance, in the 19 central planning became a prominent feature in Japanese economy, out of the 1930s came economic institutions and practices which have b prominent features of the postwar economy in Japan. Furthermore, yo men who had been socialized in the management field in the 1930s ros command the enterprises in the 1950s (pp. 88–99). There was more tinuity between the war years and postwar years than most authors h acknowledged.

As to why the political parties "went down" in the early 1940s despite t national successes in the previous decade, the author, by relating nati events and personalities to Kariya and its vicinity and by an excellent a ysis of the *jiban* (secure electoral bases) of the several politicians dealt w indicates that the political parties in the prewar days did not always m with the realities of economic and political life in rural Japan (p. 11 The Diet and political parties had become essentially central organs, Tokyo was the arena in which they played. And, in local politics, polit struggle was not a contest among groups within Kariya for resources a cated by its local government. Instead, the community often acted concert to achieve political ends shaped by its leaders. This solidarity not due to social causes but was a logical response to a structure of po which forced communities to compete among themselves for the scarce sources distributed by the state. Politics of the progressive and opposi parties in Kariya differed in nature from that in Tokyo. Political str gies adopted by the progressive parties at the national level generally tarded the progressives locally.

In the context of electoral politics, it was the numerical strength of industrial labor force and the rising status and power of the Toyota m agers that contributed to the change of community power structure. In final analysis, large-scale migration into the city of young men from to and villages outside Kariya shattered the city's political solidarity. Char in political support patterns were thus not due to national policies or pa programs, but were a product of the social transformation which the trict itself was undergoing. What resulted was an environment a good more fragmented socially and competitive politically. The intrusion of Toyota enterprise shifted the balance of power in favor of a conserva coalition supporting local industry. Most of the city's organized indust workers were a part of this coalition.

This regional industrial urbanism in Kariya and nearby towns has been a cosmopolitan urbanism with extensive cultural activities. Des its castle-town origin and heavy industrialization, Kariya was a mere c pany town in 1972. Toward the end of the book the author compare with another industrial city, Ypsilanti, Michigan, as studied by Robert Schulze in the 1950s, to bring out some uniquely Japanese features in p

tics and stratification. The sequence of economic change, corporate absenteeism, differing career patterns in American and Japanese corporations, and the structure of industrial organization are among the factors which seem to account for the fact that Kariya's economic dominants played an extremely different overt role in local politics in recent years from that played by their counterparts in Ypsilanti.

The author succeeds remarkably well in presenting a holistic vision of Japanese history. His is not a synchronic study examining the distribution of power only at a single point in time. His endeavor encompasses the domains of industrial sociology (the way in which the social features of the Japanese factory impeded organized political protests), historical demography (demonstration that the relative deprivation of the samurai showed up in the smaller size of the samurai households), urban politics, the strategy of Diet elections, the study of political elites, the family (the role of adoption and intermarriage in maintaining family standing and prosperity), and more. In sum, the historical context of urbanism is stressed much more in this book than urbanism itself.

There is no rigorous testing of hypotheses. Instead, each facet of the local life in Kariya is looked at and explained. Like so many pieces of a mosaic, together they make an informative picture. Contingency tables give the results of laborious archival research and interviews and show the changes over time. There is little statistical manipulation in the form of multivariate analysis.

Unfortunately, places such as Moto Kariya, Kuma, Okazaki, Hekikai County, Takatsunami, Oyama, Shigehara, Ichinomiya, Ikazaki, Seto, Gamagōri, and others, which appear more than once in the text, cannot be located on the only map supplied by the author (facing p. 1). Detailed maps showing the settlement patterns of Kariya in 1972 as well as the changes of the city's settlement patterns over time would aid the reader considerably in following the author's descriptions and analyses.

Japanese Industrialization and Its Social Consequences. Edited by Hugh Patrick, with the assistance of Larry Meissner. Berkeley: University of California Press, 1977. Pp. x+505. $6.95 (paper).

Byron K. Marshall
University of Minnesota

Conference volumes devoted to a single society always risk being read only by other area specialists. Happily, the audience for this volume should prove both larger and more cosmopolitan. It is the product of an SSRC-sponsored conference whose contributors stressed empirical research on topics that permit fruitful comparison between industrial societies. Thus the insights offered by these papers, especially those dealing with aspects of the emergence of the Japanese industrial working class, should be wel-

comed by sociologists and social historians regardless of their usual geographical preoccupations.

One prime example is the article by Robert E. Cole and Ken'ichi Tominaga, who describe the formation of the modern occupational structure and give evidence for its saliency as a basis for social status as well as income distribution. Cole, whose previous research on the so-called Japanese employment system has already made him one of the most respected of Western sociologists in this field, here again insists that the differences between the Japanese and Western cases have been seriously exaggerated because of the neglect of careful comparative research. Although he does not deny that occupational consciousness is generally lower in Japan, he remains at odds with those who continue to overstress size and prestige of firm as crucial variables in accounting for worker behavior and attitudes.

In the middle section of the book, "Industry Patterns and Problems of Scale," five articles by economists treat subjects related to Cole and Tominaga's. To the social historian perhaps the most useful is Yaukichi Yasuba's "The Evolution of Dualistic Wage Structure," which traces wage differentials by size of firm as far back as the turn of the century and suggests a number of social as well as economic reasons for their existence. Like the Cole-Tominaga contribution, Yasuba's underlines the significance of shifts in the sex and age composition of the work force for any study of industrial relations. Hiroshi Ohbuchi's article on population trends, while not directed specifically at these issues, nevertheless affords an overview of the demographic transition that accompanied industrialization. Unfortunately, the only paper that examines in any detail the consequences of industrialization for women is Gary R. Saxonhouse's study of the Japanese cotton-spinning industry. It alone, however, is sufficient to bring out the critical importance of the fact that the earliest, and for decades the largest, of Japanese factories in the private sector employed mainly young females who rarely remained in the mills for more than two or three years.

The exploitation of child and female labor is just one of many facets of the picture presented here that should give ample cause for abandoning some of the more tired clichés about the Japanese success story. Income inequality and other forms of social deprivation generated by industrialization are the subject of the three final papers. Akira Ono and Tsunehiko Watanabe, in "Changes in Income Inequality," survey the past 90 years. In "Poverty in Modern Japan" Masayoshi Chūbachi and Koji Taira concern themselves not merely with income differentials, but also with the life styles of the poor and the evolution of perceptions of poverty. Prior to the First World War the attention to poverty was focused primarily on the problems of urban ghettos, and government reaction was largely limited to urban renewal projects. Only in the 1920s, with the intensification of industrialization and the widening of the gap between the poor and the new factory worker, was it realized that "poverty was a ubiquitous risk for anyone in a dynamic society" (p. 433). Then the approach shifted to the problem of providing social services. (Parenthetically it should be noted that Chubachi-Taira, read along with the first paper in the volume—

Hiroshi Hazama's "Historical Change in the Life Styles of Industrial Workers"—offers fascinating bits of historical ethnography all too rare in recent English-language publications on Japanese social history.) The concluding essay, by John B. Bennett and Solomon B. Levine, focuses on the contemporary period, exploring both the objective reality of social deprivation (including environmental pollution, urban congestion, loss of community, inadequate welfare services) and the thorny subjective question of *"social quiescence"—the "relative passivity of the Japanese population* in the course of industrialization and modernization" (p. 445). In their view Japan is at "the dawn of a new political era" in which passivity is rapidly giving way to a "postindustrial" style of citizen protest (p. 483). It is an essay that might be fruitfully read together with another recent conference volume that examines contemporary tensions, *Japan: The Paradox of Progress*, edited by Lewis Austin (New Haven, Conn.: Yale University Press, 1976).

Space does not permit a closer look at the multitude of issues raised by either the papers or Hugh Patrick's introduction. Taken as a whole, however, the volume suggests a picture of Japanese social change far less orderly—and thereby far more similar to other national experiences—than was painted in the literature of only a decade or two ago. It is thus representative of a new wave of publications on Japan that emphasize social heterogeneity and conflict rather than homogeneity or a consensual tradition.

Urbanization in Latin America. Edited by Jorge E. Hardoy. New York: Anchor Press, 1975. Pp. xix+480. $4.50 (paper).

Bryan R. Roberts
University of Manchester

This collection of 12 articles and a preface deals with a wide range of issues, covering both historical and contemporary facets of Latin America's urban development. It provides a useful introduction to Latin American social science and to the substantial contribution made by Latin American social scientists to our understanding of urbanization.

The common approach in these articles is that of treating urban issues from both a global and a historical perspective. Jorge Hardoy emphasizes the spatial and morphological continuities in urbanization, deriving from the experience of colonial and neocolonial domination. Richard Morse concentrates on the origins of Latin America's urban institutions and contrasts the economic and political role of the city in Latin America and in Northern Europe. Complementing Morse's article is one by Fernando Cardoso correcting the misapprehensions involved in viewing Latin America as basically a rural-dominated society. Cardoso argues that the political (represented by the city as an administrative and control center) has dominated

the economic in Latin America's patterns of development because of the centrality of coercion in the system of production and exchange. It is left to Anibal Quijano to provide an account of Latin American urbanization as part of the development of both the international division of labor and the uneven capitalist development of the periphery.

There are interesting differences in perspective among the contributors which suggest further research possibilities. Paul Singer, contrasting his approach to that of Quijano, presents a clear statement of the need to study urbanization in terms of the local conditions under which capitalism expands, rather than in terms of external forces, such as imperialism. Marginal urban populations or the concentration of urban employment in the service sector are, thus, to be viewed not simply as by-products of a dependent development, but as an integral part of capitalist expansion, through providing a pool of cheap labor that facilitates the growth of the most advanced sector of the economy. In contrast, Quijano stresses the increasing irrelevance of the poor to capitalist expansion in Latin America, arguing that this expansion, based on capital-intensive techniques, tends to marginalize permanently a large sector of the urban population.

These articles encourage new research because they make a clear break with the previously dominant approaches to Latin American development, those of the various versions of the modernization perspective. Morse and Cardoso, for example, stress the diversity of ways through which complex and differentiated economies and polities can be developed. Quijano makes a similar point by emphasizing the interdependence of the world economy which makes "modern" and "traditional" complementary parts of the same economic process. These authors draw attention to the historically specific forces that lead to divergences as well as convergences in the development paths of the Latin American nations.

Other papers provide more policy oriented approaches to urbanization. Oscar Yujnovsky and Hardoy, Basalda, and Moreno present the beginnings of an analysis of urban spatial structure. This is a field in which there has been little systematic and theoretically based work in Latin America, and these papers emphasize usefully the relationship of spatial organization to the different stages and patterns of economic development. The problems of securing a balanced regional development in the face of economic concentration are reviewed by Carlos Romo. Victor Urquidi looks provocatively at the failure of imagination in planning urban development, suggesting that the future of the city may be a short-term one if more radical approaches to urban land reform, transport policy, and tax reform are not adopted.

The other articles in the volume, while showing a similar concern with the macrostructural constraints on urban development, are more empirical. Lander and Funes provide useful data, for Venezuela and for Latin America as a whole, on rates of urbanization, industrial development, income distribution, and urban land values. Luis Unikel gives a rigorous analysis of

Mexico's urban growth. Cesar Vapñarsky discusses primacy and the rank-size rule in Argentina. Vapñarsky's analysis has significant implications for understanding the internal structure of dependent development, showing that primacy, though usually present in an export-oriented economy, does not necessarily entail the absence of a well developed and integrated regional system of smaller towns and cities.

This is by no means a volume without faults. The articles, while interesting in themselves, do not make a coherent collection, because of differences in topic and in form of analysis. Individual articles could also be faulted for the unevenness of their coverage of certain central issues—for example, the workings of the urban economies of Latin America. There is in fact very little analysis here of the social structures of contemporary Latin American cities. Yet, this volume is a badly needed and very useful contribution which should stimulate the research needed to develop further the seminal ideas presented in the various articles.

Urbanization and Inequality: The Political Economy of Urban and Rural Development in Latin America. Edited by Wayne A. Cornelius and Felicity M. Trueblood. *Latin American Urban Research*, vol. 5. Beverly Hills, Calif.: Sage Publications, 1975. Pp. 316. $17.50 (cloth); $7.50 (paper).

Braulio Muñoz

University of Pennsylvania

This book is the fifth volume of the continuing series on Latin American Urban Research, edited by Wayne A. Cornelius and Felicity M. Trueblood and published in cooperation with the Center for International Studies of the Massachusetts Institute of Technology. This volume is the product of an interdisciplinary effort to explore systematically "some of the disturbing questions about the relationship between urbanization and socioeconomic inequality" (Cornelius, p. 10). To this end the editors have enlisted the participation of five economists, three sociologists, three anthropologists, one political scientist, one geographer, and one urban and regional planner. The outcome is a very informative book in which different perspectives meet and complement each other. The topics of the research papers included range from the effects of national governments' policies on urbanization processes (Brazil, Chile, Colombia, Mexico, and Peru) to urban politics among both the middle classes and the poor populations of the urban centers.

The editors have also included a penetrating theoretical paper by John Walton. In "Internal Colonialism: Problems of Definition and Measurement," Walton attempts to salvage the concept of "internal colonialism" from the vagueness which has characterized it ever since it entered the theories of development. After a critique of the various uses to which the concept has been put by social scientists and politicos alike, and building

upon the literature of dependence theory, Walton puts forth his own reformulation. He sees internal colonialism as "a process that produces certain intra-national forms of patterned socioeconomic inequality directly traceable to the exploitative practices through which national and international institutions are linked in the interests of surplus extraction and capital accumulation" (pp. 34–35). The elaboration on this definition takes up much of Walton's theoretical thrust. His efforts toward making the concept of "internal colonialism" amenable to operationalization are highly instructive. In the last analysis, however, he admits, the viability of the concept as a fruitful theoretical tool remains to be empirically tested. One hopes that empirical research along the lines suggested by Walton will be carried out in the near future.

The volume also includes two papers by Eduardo E. Lozano which deal with the urban policies initiated by the socialist government of Salvador Allende in Chile: "Housing the Urban Poor in Chile: Contrasting Experiences Under 'Christian Democracy' and 'Unidad Popular' " and "Regional Strategy of 'Unidad Popular' in Chile." His discussion proceeds by contrasting Allende's government's efforts to solve the urban problem in Chile with previous Chilean governmental efforts, particularly the government of Eduardo Frei which immediately preceded it. The inclusion of this article is particularly important since Chile has been the only country in Latin America, besides Cuba, where a socialist government has actually initiated a concerted effort toward a radical solution of the problems of urbanization. And, as Lozano makes clear, in contrast with other governmental policies in Latin America, the Allende government approached the urban problem from a holistic perspective with urban problems seen as part of a more encompassing problem—the restructuring of the socioeconomic aspects of the Chilean society as a whole. The Chilean experience also shows, as Lozano demonstrates, the difficulties that a socialist regime may encounter in trying to implement radical approaches to socioeconomic development while working within the rules of a basically capitalist political structure.

Among the other papers included, two explore the effects of urban-industrial development on peripheral rural communities. A paper by Robert V. Kemper and George M. Foster analyzes changes in a Mexican village, Tzintsuntzan; Bryan R. Roberts deals with changes taking place in Huancayo, Peru. Both studies analyze data gathered in years of research on these areas and contribute a much-needed detailed analysis of center-periphery relationships in socioeconomic development. Papers by Liza Peattie and Susan Erikstein analyze small-scale economic activity within large urban settings. Particularly interesting is the work of Peattie, " 'Tertiarization' and Urban Poverty in Latin America," which analyzes the socioeconomic processes in the "tertiary sector" of Bogota, Colombia. Taking issue with those who see the tertiary sector as a sphere of "easy entry" in which "free competition" is the rule, she argues that, on the contrary, there are structural factors which do not allow free competition but foster "semi-monopolistic shelters" for those already in the market.

It is not possible to touch on all the articles included. Overall, however, this book is a welcome addition to the research on Latin America's socio-economic development. The papers are well integrated into the volume by the authors' underlying concern for the effects, both intended and unintended, of public policies on social inequality in Latin American urban sectors.

Social Consequences of Modernization in Communist Societies. Edited by Mark G. Fields. Baltimore: Johns Hopkins Press, 1976. Pp. ix+277. $14.50.

Nicolas Spulber
Indiana University

This volume consists of 10 papers presented at a symposium held at Salzburg, Austria, in September 1972, under the sponsorship of the American Council of Learned Societies. The symposium focused on two issues: (1) the "qualitative" nature of social change under communist rule; (2) the extent to which the communist process of managed modernization is akin to, or congruent with, the noncommunist process of spontaneous modernization. Modernization is understood as implying the creation of industrial, urban, and mechanical or impersonal societies. It involves, on the one hand, the expanding use of "productive technology," and, on the other hand, the development of educational-scientific structures able to generate the skill mix required by industrialization (pp. 24 ff.).

Various authors represented state that in attempting to "adopt (and adapt) a process of social change that had already taken place elsewhere" (p. 7), the communist-ruled societies have indeed created a number of phenomena similar to those which exist in the noncommunist societies. As a whole, the collection indicates that: (1) the same kind of "modern personality" has been shaped in both societies, producing men who owe allegiance to the government leaders (rather than to traditional authorities), who believe in science and in the efficacy of knowledge, who are endowed with increasingly complex skills, etc.; (2) the same type of educational system has evolved on both sides of the political divide—a system consonant with the traits of "modern man" (p. 57); (3) the same innovative spirit has been at work in the two types of society—though in the USSR innovation has tended to be confined to the military and aerospace technology and has not been allowed to have full play in the civilian sector; (4) health has been viewed and handled as a "public utility" with the same claim for resources in both societies; and (5) similar phenomena have arisen concerning nonpolitical deviance and social disorganization—particularly with regard to alcoholism, juvenile delinquency, and crime. The volume suggests, finally, that the leading party cannot ultimately continue to rule a differentiated

society and that at a certain stage of development it will have to substitute for this rule democratic associations and procedural rules.

Unfortunately, no data whatever are presented concerning the social structure itself—showing, say, specific changes in the structure of the labor force at some benchmark years, how and why this structure is akin to or different from that of advanced noncommunist countries, the weight of services in the two types of societies, the different ranges of these services, and so on. The central phenomenon of Soviet social stratification—the development of three intertwined bureaucracies (of the party, of the state, and of the nationalized economy)—is not dealt with squarely, but discussed only incidentally in various papers. Only three papers point at sharp differences in the patterns of urbanization of the societies considered and at significant differences within the communist "camp" between the Soviet and Chinese approaches to the role of the towns.

What the participants in the symposium have deliberately ignored is the fact that the communist vision of managed modernization is based on a totally different conception of the "optimal" structure of the society and of the economy from that prevailing in individualist-oriented societies. Phenomena superficially akin may have a different thrust and involve ultimately divergent rather than convergent patterns of change. Given the communist policymakers' inflexibly pursued preferences and the specific pattern of investment they condition, the nature of the Soviet goals and interests with respect to the production of the national output and its mix (viz., accuracy in the execution of central commands rather than efficiency), the desired social structure aimed at (more workers in production than in services, and controlled development of the "intelligentsia"), one may wonder whether the social structures are indeed similar in the two societies. In the United States the crossover to a larger share of the labor force occupied in industry and in services than in agriculture took place for both industry and services in the period from 1910 to 1919. In the Soviet Union the crossover to a larger share of the labor force in industry than in agriculture took place in the early 1960s: services remain deliberately far less significant and far less diversified than in the West. Development—in the Western sense—implies not some set pattern in "productive technology" but a host of concomitantly changing technologies including changing forms of organization of large enterprises and of coordinated systems, evolving patterns of distribution of goods and services on a mass-market scale, new directions in production itself and in urbanization, and so on. There is no guarantee whatever that the Soviet process of managed modernization can or will evolve in the same ways.

This symposium, though quite rich in the range of problems tackled, in the suggestions offered, and in the insights provided, seems rather strangely parochial in its references to the literature. The authors have completely overlooked the substantive literature generated in economics about many of these same issues, particularly in and around the writings

of some well-known people such as John K. Galbraith, W. W. Rostow, and the Nobel laureate Jan Tinbergen.

The New Urban History: Quantitative Explorations by American Historians. Edited by Leo F. Schnore. Princeton, N.J.: Princeton University Press, 1975. Pp. vii+284. $17.50.

Brian J. L. Berry
Harvard University

This book is the third in a series entitled *Quantitative Studies in History*, sponsored by the Mathematical Social Science Board. The focus is primarily upon 19th-century experience. An attempt is made to delineate the forces which promoted the growth of cities and to analyze the effects of urban growth on the economic, social, and political life of the inhabitants, but with an underlying purpose: to foster research in the application of mathematical methods in what some may debate is one of the social sciences, namely, history.

Like so many similar efforts, the book was the outcome of a conference—this one held at Madison, Wisconsin, in June 1970. The original aims of the conference "were quite simple: to bring together a group of younger American scholars who were using quantitative methods on urban-historical problems—at whatever level of statistical or mathematical sophistication, and regardless of disciplinary background" (p. 3). In the succeeding four years the conference contributions were ultimately shaped into a book with nine papers and an "Agnostic Foreword" by Eric Lampard.

Of three papers on the growth and function of cities, two are by geographers (Allan Pred and Martyn Bowden) and one is by a historian (Kenneth Jackson). Three others, dealing with accommodations to the urban environment, are by historians (Kathleen Conzen, Zane Miller, and Gregory Singleton). The three final papers are all by economists (Claudia Goldin, Robert Higgs, and Joseph Swanson and Jeffrey Williamson) and present economic analysis of urban-historical phenomena. Quantitative applications range from exceedingly simple data processing (by historians) to deductive modeling totally devoid of data (not expectedly, by the economists).

Collectively, the papers deal "with jobs, residences, neighborhoods, adjustment, status, accommodation, innovation, and location. They conjecture and measure some of the attitudes and behavior of capitalists, workers, immigrants, and freedmen, and speculate upon the varieties and amounts of resources with which households, firms, and associated social groupings cope with their changing environments, physical and social" (p. 43). Individually, the quality is that of good average journal articles, and there are no surprises. The volume is workman-like, demonstrates that social scientists can work with past urban data as well as those of the present, but keeps open the question as to whether this is "new (quantitative) urban

history." One senses that after crusaders carried through the quantitative revolutions in economics, geography, and sociology in the late fifties and early sixties, the flame was dampened. In Thomas Kuhn's terms, the book conveys a sense of the everyday routine of normal science, and none of the revolutionary fervor suggested by the title.

An Econometric Model of Residential Location. By Michael Granfield. Cambridge, Mass.: Ballinger Publishing Co., 1975. Pp. xi+134. $16.00.

Bryan Ellickson

University of California, Los Angeles

The aim of this book is to develop a three-equation model of urban housing markets based on economic theory. The parameters of the equations are estimated using moderately large data sets from Buffalo and Milwaukee, and the resulting estimates are then used to simulate the dynamic effects of various social policies. The conclusions drawn seem provocative: neighborhood racial and income composition are more important determinants of residential location than accessibility to workplaces; suburbanites behave differently from central-city residents; nonwhites do not appear to have shorter time horizons than whites; enforcement of open-occupancy laws would be more effective than income supplements in breaking up the ghettos; and the flight to the suburbs may slow down dramatically.

These claims are based upon a rather shaky foundation. Economic theory plays only a minor role, essentially justifying only the broad assumptions that housing expenditures should depend on household income, that location of residence should depend on the value of travel time, and that higher-income households should prefer single-family homes. The three dependent variables are housing expenditures, distance from residence to workplace, and the type of residence chosen (single family, two family, multifamily). Housing expenditures depend on household income, neighborhood income, and a dummy variable for white-collar workers. Location depends on housing expenditures, race, neighborhood income, mode of travel (auto versus mass transit), and the cost of commuting. The type of house chosen depends on housing expenditures, location, and family size. Economic theory contributes little to this specification of the model. The only reason a sociologist might not write down the same set of equations is that this form of model building is more common in economics.

The equations are estimated using ordinary least squares and reestimated using two-stage least squares, with the three dependent variables treated as endogenous. The failure to treat neighborhood income and perhaps choice of transportation mode as endogenous seems a serious drawback. Only the first equation explaining expenditures on housing fits the data well, and this may be an artifact (expenditure on housing was not available in the original data sets and was imputed from census data using a procedure that probably enhanced the correlation with household income).

There is little justification on the basis of economic theory for believing that the specified equations should be stable over time or from one urban area to another. In fact, the parameter estimates obtained for Buffalo and Milwaukee differ rather substantially, and it comes as no surprise that different parameter estimates are obtained when central city and suburban samples are treated separately. I find little reason to believe that these equations would remain stable if open-occupancy laws were imposed or blacks received income supplements, the two subjects of the "dynamic simulations." And in fact the dynamic simulations are not dynamic at all: the parameters are all estimated using cross-sectional data, and the dynamics is actually just a comparison of the model predictions using different values for the exogenous variables—not true dynamics at all.

The main problem with this book is that, despite its relatively recent issue, it is quite dated, being drawn from a dissertation written several years ago. The most recent citations to the literature are from 1969, about the time that economists began taking urban economics seriously. At the time this research was conducted, it represented a novel and rather ambitious approach to the subject. But the author's review of the literature gives a very misleading view of the current state of the art. The reader seeking to find what economists are doing in this area would be better advised to dip into some recent issues of the *Journal of Urban Economics* or one of the several books on urban housing markets published by the National Bureau of Economic Research. Sociologists should be pleased to find that in that literature, as in Granfield's study, there is considerable evidence that neighborhood characteristics are a much more important determinant of residential choice than accessibility.

Educational Reform with Television: The El Salvador Experience. By John K. Mayo, Robert C. Hornik, and Emile G. McAnany. Stanford, Calif.: Stanford University Press, 1976. Pp. xi+216. $11.50.

Jane T. Bertrand

University of Chicago

Among the members of the so-called international Development Set, there is ever increasing sensitivity as to the manner in which development projects with foreign assistance are carried out. Many of the "collaborative projects" of the 1960s proved to be nothing more than a source of data for ambitious U.S. academics whose strong interest in social development was all too closely related to potential academic publications. As part of this phenomenon, local collaborators were often forced to accept pre-determined models for implementing a project or to adhere rigidly to an experimental design for the sake of scientific accuracy, while sacrificing the true goals of the program.

Despite the apparent lack of Salvadoran input into the actual writing of the book, *Educational Reform with Television: The El Salvador Experi-*

ence reflects a sensitivity to past abuses and attempts to avoid repeat them. The authors make it clear from the start that control of the proj studied did indeed rest with the Salvadorans. They cite instances of Salvadoran will prevailing over the foreign advisors' recommendations (s cifically, in directing the educational reforms to grades 7–9, where expected impact might best benefit the goals of the nation). Moreo they acknowledge shortcomings in the experimental design—the result subordinating scientific rigor to programmatic interests in a number cases. Such factors lend a flavor of reality to the research and sugg adherence to an international code of academic ethics.

The project under study is the Salvadoran educational reform, launc in 1969 during the presidency of Fidel Sánchez Hernández and sp headed by Minister of Education Walter Béneke. The introduction of instructional television system (ITV) was a key innovation, but one wh was carefully integrated with other reforms: facilitating enrollment, training teachers, updating curricula and methods of evaluating stud performance, to name a few. If this project were not ambitious enou it was intended that it should be accomplished without increasing the student cost of education.

This five-year educational plan (scheduled for 1968–72) was beset fr the beginning with a series of predictable problems: legislative bottlene delays in obtaining adequate facilities, intergroup rivalries, inexperie in certain technical tasks, etc. This resulted in a delay of almost two ye in putting the ITV system in motion. Those behind the reform wis to see its effects within the five-year presidential term, which left c a three-year period.

Indeed, the system did show effects within three years, as demonstra by the results of general ability, reading, and achievement tests admi tered to three successive cohorts of incoming seventh-grade students du this period. In all three cohorts the ITV students gained from 15% to 2 more on the general ability tests than did their non-ITV peers (p. 82). ' ITV students also gained more on the achievement tests administered c ing the seventh grade. In contrast, there were no clear-cut gains in reac ability or on some of the achievement tests administered to these coh in the eighth and ninth grades. Throughout the analysis the authors ex ine these results in the light of sociodemographic factors which allo further inquiry into the workings of the system. They found, for exam that ITV did not serve to broaden the knowledge gap between the m advantaged and less advantaged students.

It is impressive that while these results were realized the cost per dent of education actually decreased. In effect, the costs associated w the ITV system were effectively offset by increasing teachers' hours class size, an enviable achievement.

As mentioned earlier, the experimental design was compromised in eral respects for pragmatic reasons. The fact that the system was to extended throughout the country in the course of the reform period ef tively eliminated the possibility of "controls" in the experiment. Moreo

no attempt was made to include a comparison group consisting of nonstudents of similar ages against which to measure the scores obtained by the reform group. Thus, in the absence of actual control groups (students not exposed to the reform over the three-year period investigated) or even comparison groups (nonstudents of similar ages), it is more difficult to evaluate adequately the increases realized over the three-year period. For example, the authors discuss the need to interpret the practical implications of their data in relation to the tests for basic skills learning; to this end, they present the percentage gains in general ability for the different cohorts. Yet the question remains unanswered, What gains would be expected in the three-year period even if a child had no schooling? Though the comparison group of nonstudents needed to answer this question would provide an underestimate of the gains to be expected on the part of students, it would nonetheless give more perspective for interpreting the findings.

Another problem was the inability of randomly assigning students or classrooms to a given treatment group. Although it was necessary to accept this compromise, the authors might have gone into more detail in describing the comparability of groups based on subsequent analysis of sociodemographic variables.

The authors are candid in acknowledging factors which unquestionably favored the outcome of this experiment: the size of the country, which facilitated communication and access to the capital, the leadership of Walter Béneke in seeing the project through, etc. Thus, it would be inappropriate to generalize the results from El Salvador to another country where equally favorable conditions are not present. Nonetheless, the results of this study will undoubtedly serve as a source of encouragement for those contemplating a similar endeavor. And indeed, the authors are to be congratulated for the coherent manner in which they present not only the cut-and-dried findings, but rather the entire project with all its triumphs and frustrations.

Mass Communication and Political Behavior. By Sidney Kraus and Dennis Davis. University Park: Pennsylvania State University Press, 1976. Pp. 308. $16.50 (cloth); $7.95 (paper).

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Empirical evidence and speculative essays regarding politics and the mass media have been accumulating for almost four decades, but scholars have hesitated to undertake a comprehensive review of this material. Kraus and Davis, armed with a grant from the Markle Foundation, have inventoried some 3,000 sources in the fragmented political communication literature. Their initial product, an appropriately interdisciplinary compilation of abstracts and bibliographic references, occupies 100 pages of this volume. The abstracting effort leaves much to be desired, as key studies are over-

looked, some summaries are flawed, and few post-1973 entries appear. Nevertheless, the painstaking collation of classic and recent research materials provides a valuable resource for researchers studying the role of mass communication in political processes.

The remaining two-thirds of the book presents a critical synthesis of this complex morass of findings, conceptualizations, and commentaries. Unfortunately, the authors are less than successful in this ambitious endeavor. The most pervasive weakness is the lack of coherent organization of the issues and evidence both within and across the topical chapters. In addition, unsupported assertions are offered frequently instead of the effective marshaling of scientific data.

The basic theme emphasized by Kraus and Davis is that the mass media are a politically influential force that the social science establishment has failed to recognize, owing to misleading interpretations of pioneering investigations conducted in the pretelevision era. In particular, they feel that the "null effects" perspective popularized by scholars at the Bureau of Applied Social Research has been inordinantly influential in the sociology of knowledge on this subject. Regrettably, their refutation provides neither an incisively critical reassessment of classic research nor a perceptive integration of current behavioral evidence. It is true, however, that many sociologists and political scientists have not given mass media the serious attention and respect that other sources of political influence receive.

The book begins with "Mass Communication and Political Socialization," a chapter that bemoans the traditional research preoccupation with family and school agents at the expense of media inputs. The authors contend that TV is really a highly significant influence on adolescent development of political orientations, but their case is not well documented.

Next comes "Mass Communication and the Electoral Process." Kraus and Davis survey a wide variety of information about mass media and voting, but do not offer a fully coherent arrangement of the material. Many conclusions regarding effects are invalidly inferred from content analysis or exposure patterns instead of behavioral data. In some cases, they inexplicably ignore the most pertinent and convincing available findings in buttressing conclusions. Furthermore, there is skimpy coverage of several crucial topics, such as TV news and political advertising.

The chapter entitled "Mass Communication and Political Information" contains a good overview of various models and concepts drawn from general theories of mass-communication effects. It would be considerably strengthened if a sharper distinction were drawn between informational and persuasive processes. The authors urge a renewed emphasis on transactional theory, but do not offer a carefully developed framework to guide research.

This chapter is followed by an interesting essay which focuses on linkages between communication and political sociology. The central discussion of media use and political activity is marred by an overemphasis on the political alienation concept. Kraus and Davis briefly propose a tentative systems theory model with potential heuristic value.

The book also includes a section called "Methods of Political Communi-

cation Research." Instead of presenting a conventional methodological assessment of measurement and design techniques, this chapter features a lucid discussion of model construction and evaluation. The authors do include some routine yet laudable suggestions for more sophisticated investigations. Given the primitive state of research in political media effects, their advocacy of increased reliance on elaborate multivariable relationships is well handled.

An overall evaluation of this work yields several basic strengths and weaknesses. The authors provide insightful treatment of the intellectual and cultural forces that have shaped the development of the political communication research field. They offer valuable suggestions for future research priorities at numerous points in the book. Their style of writing is lively and intelligible.

On the negative side, there are troublesome structural, conceptual, and pragmatic deficiencies. The organizing principle is obscure, and it is especially difficult to ascertain the rationale behind placement of various topics. Certain ideas and arguments are presented in a redundant fashion, while other major issues are slighted. Different types of cognitive, affective, and behavioral effects are not clearly defined and interrelated. Finally, the information is not likely to be useful for professional practitioners and politicians.

Despite these problems, Kraus and Davis perform a unique service with their broad-ranging collation of significant knowledge about a highly important subject. This volume is a valuable reference source for academic researchers and a comprehensive introductory text for students in political communication courses.

Social Structure in Divided Germany. By Jaroslav Krejci. New York: St. Martin's Press, 1976. Pp. 262. $17.50.

National Consciousness in Divided Germany. By Gebhard Ludwig Schweigler. Sage Library of Social Research, vol. 15. London and Beverly Hills, Calif.: Sage Publications, 1975. Pp. vii+281. $12.00 (cloth); $7.00 (paper).

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Comparative study of the German Democratic Republic (DDR) and the Federal Republic of Germany (BRD) flourishes, and yet important questions remain to be asked about the meaning of the differences between the two societies. It is frequently pointed out by Western analysts that Germany was a united and relatively homogeneous country until 1945; whatever differences now exist between the two German societies, it is argued, can be ascribed to the differences between the capitalist Western European system and the socialist Eastern European system; ergo, Germany provides

an unparalleled opportunity for "intersystemic analysis" (Krejci, pp. 7–21). In *Social Structure in Divided Germany,* Jaroslav Krejci attempts to synthesize a great deal of empirical research and data on the two systems as they are exemplified in the DDR and the BRD. Unfortunately, however, empirical research on the DDR is relatively scarce, and, furthermore, data are generated in the DDR according to different criteria from the surfeit of statistical data on life in the BRD. The differences in the categories of data collection themselves seem worthy of investigation. Indeed, this is an important part of the first unasked question: How is practical social thinking structured in the two societies? According to what principles, to what ends, and with what consequences? The data for answering such questions about mentality in the DDR are manifold, being precisely the stumbling blocks in the paths of empirical researchers interested in comparable data about behavior.

Krejci bypasses such issues in order to "find those variables which . . . can be considered as parameters of the social system and consequently be tested in any other intersystemic analysis" (p. 13). He naively claims to have no a priori position and to want to let the data speak for themselves, only roughly correcting for the fact that the data have been created by the exponents of the two systems he is trying to compare. He is an empiricist and holds that "neither conceptual rigidity nor epistemological elegance can be considered a virtue" (p. 11). But what he and most of the writers he draws upon really want to know are the answers to such moralistic and impractical-practical questions as: Is it better to live in the DDR or the BRD? And Krejci's analysis suggests a high correlation between the moralistic motives behind so much intersystemic analysis and the lack of conceptual rigor.

The chapter "Income and Wealth," for example, is an attempt to compare the degree of alienation in the two German societies. Using both the Marxist and the classical economic definitions of labor value, Krejci calculates that the surplus value which accrues to the state in the DDR is proportionally greater than the profit on capital in the BRD. In other words, workers are more alienated in the DDR than in the BRD! This dramatic conclusion is unconvincing because Krejci does not consider whether the fact that the surplus value accrues to the state instead of to owners of private property alters the meaning of exploitation in the DDR, nor whether the greater employment security in the DDR ameliorates it. In "Stratification and Mobility," Krejci fails utterly to provide a conceptual umbrella that would make sense out of the apparent contradictions among the assertions of Schelsky, Janowitz, Dahrendorf, and others. Instead, he contents himself with the bland and improbable conclusion that there are "more similarities than differences" in the organization of status in the two countries and that education is the principal channel of mobility in each. This seems to demonstrate again Weber's contention that it is even more imperative in questions of status than in questions of economics to know what the members of a society think about the subject. It seems obvious that status has radically different content in the two Germanies: certainly

the components of status—type of employment, kind and quantity of education, and wealth, to say nothing of party membership—are weighted very differently. This would not be difficult to demonstrate if one were not preoccupied with deciding which of the two societies embodied greater status inequality and permitted less mobility. But it is in the political chapter entitled "Freedom and Power" that Krejci's compulsion to adjudicate between the DDR and the BRD most thoroughly obscures interesting questions. Although he acknowledges the differences in definition of freedom in East and West, the thrust of his argument is to counter Peter Christian Ludz's contention that the DDR is no longer "totalitarian" but merely "authoritarian" (*The Changing Party Elite in East Germany* [Cambridge, Mass.: M.I.T. Press, 1972]). There is no discussion of political symbols or even of the very interesting social psychology of politics in the DDR.

Krejci's book is a work of summary and reflects the concerns of an important part of the scholarship on the two Germanies, even in its moralistic dimension. Fortunately, some of the most significant publications on the DDR—most notably those of Ludz—depart from these concerns dramatically. We may take these as signs that as the sense of horror at Eastern European society wears off in the West, Western social scientists generally will find themselves able to take East European mentalities seriously. Then it may be possible to analyze East European societies as foreign cultures with their own coherent systems of laws, social categories, solidarities, symbologies, etc., and not merely as alien territories on which completely cynical leaders oppress masses and pose economic and military threats to the West. Of course there will remain much for Westerners to object to in East European culture, but as things have stood, it has been easier to study the so-called premodern Third World empathically than this Second World. In intersystemic analysis the cultural meaning of differences between the First and Second Worlds has been largely ignored. Strangely, journalists have been quicker to recognize the existence of this cultural distance than social scientists; see for example Marion Donhoff et al., *Reise in ein fernes Land* (Hamburg: Die Zeit, 1964), the remarkably subtle first contribution to the genre of serious travel literature on the DDR which is now voluminous. Social scientists naturally have a more sophisticated task, but the tools of cultural analysis have not been wanting so much as the will to use them on socialist Eastern Europe. Where prejudice against socialism has not done so, the prejudices associated with modernization theory have blinded social scientists to the cultural dimension of the differences between East and West.

National consciousness is one of the matters concerning which social scientists have considered the differing mentalities of the BRD and the DDR. They have only recently begun to recognize the existence of patriotism, pride, and national consciousness in the DDR, however. Even Ludz has been reluctant on this point, although he has tried to discourage hope of reunification (see *Deutschlands doppelte Zukunft* [Munich: Hanser,

1974]). Recognition of such sentiments in the DDR has of course come in the wake of the realization that whole-German (*gesamtdeutsch*) national consciousness is disappearing even in the BRD. The great service of Gebhard Schweigler's *National Consciousness in Divided Germany* is to synthesize the literature on this subject and settle the question: in the minds of most people in both the BRD and the DDR, there are now two Germanies. The practical consequence is that the various fantasies of reunification formerly cherished by many Germans are now finally being dropped from political consideration. But was this not another impractical-practical question from the beginning? This is not the 19th century, and there has never been any likelihood of new wars of liberation or unification. On the other hand, 19th-century questions still pose some of the most profound problems to the social sciences. The nation, for instance. Theoreticians in East *and* West would like to have transcended the age of the nation-state, but, to the extent that internationalism exists as a social fact, it is defined as community of nations. The two Germanies are particularly instructive in this regard—belonging to two different communities of nations and being constituted according to two completely different symbolic systems. One might think that this had been clear for decades, but, for those to whom it has not, recent developments in the DDR virtually force the fact upon an observer's attention.

The new constitution of the DDR (1974) and the writings of SED theoreticians Hermann Axen and Alfred Kosing announce that the DDR is now a nation unto itself. This represents a dramatic reversal of Walter Ulbricht's affirmation of the unity of the German nation (*Das nationale Dokument* [Berlin: Dietz, 1962]). In the BRD, however, official policy still maintains that there is but one nation divided between two states. This disagreement was already a crucial dimension of the negotiations by which the two states established diplomatic relations in 1972, but it also poses the question of what a German nation is. The question goes well beyond the one Schweigler tries to answer in *National Consciousness*. In order to argue convincingly that there are now two separate nationally conscious populations (and not just two states) in Germany, Schweigler defines the nation social-psychologically. Rejecting the traditional distinction, codified by Friedrich Meinecke, between the political nation (*Staatsnation*) and the cultural nation (*Kulturnation*), he defines the nation in terms of "social consciousness" (*Bewusstseinsnation*), after Karl Deutsch (*Nationalism and Social Communication* [Cambridge, Mass.: M.I.T. Press, 1953]) and others. This tactic enables him to answer his well-delimited "German question" quite practically. But it is somewhat circular, and, more important, it ignores the very interesting question of what kind of a nation the DDR presumes to be and what the cultural differences are which account for the existence of two supposed German nations. The social-psychological definition is a purely formal one, having to do with channels of communication and the boundaries of solidary groups rather

than with what is communicated or the symbols of solidarity or any other aspect of mentality.

It may seem paradoxical that Schweigler could demonstrate convincingly that the two German populations have different national consciousnesses without describing what either of them is nationally conscious of. But in another sense it is perfectly appropriate and symptomatic. Most Western sociologists live in a postideological world and can hardly be expected to recognize a cultural system in Eastern Europe when they deny the existence of anything but a culture of utility in the West. At best Western sociologists are able to see culture in ethnicity and national character, but they have not developed rigorous concepts for describing it. East European social theorists are in a more fortunate position in this respect. Both because of the ideological exigencies inherent in their position in the Second World, and, somewhat ironically, because Marx constantly reminds them of the *Überbau* of culture rising above any set of material relations, Eastern thinkers take culture seriously, even when dealing with complex and industrial societies. Alfred Kosing (*Nation in Geschichte und Gegenwart* [Berlin: Dietz, 1976]), for instance, is not at all embarrassed by the opportunity of explaining the differences between the capitalistic and socialistic national cultures of the BRD and DDR, or even by the necessity of sorting out ideological from ethnic factors in these systems. Of course few Western sociologists would admit that Kosing's book is sociology; it is too ideological. Nevertheless, Kosing's ideology permits him to see things that neither Krejci nor even Schweigler permits himself to see.

It is plain delusion to think that the differences between the societies of the BRD and the DDR are all a matter of relative economic, social, and political privation, or that the differences in national consciousness can be explained formalistically. Krejci is right that the two Germanies provide an ideal opportunity for intersystemic comparison. But more important than determining the relative wealth, mobility, and freedom of the two populations is understanding how they think about society—their social mentality. Surely this is the greatest challenge of coexistence. Western sociology embodies cultural features (e.g., the ideology of the end of ideology) which make the task more difficult than it needs to be, but not impossible. As I have pointed out, Western social scientists have not found it impossible to penetrate the cultures of non-Western societies with which there was never much question of coexisting on an equal or competitive basis. And recently several students of non-Western societies (e.g., Louis Dumont) have turned their attention to the complex industrial societies of Western Europe and America. The analysis of the various national cultures of Eastern Europe which define their relationship to the West in competitive terms might now be recognized as a crucial part of this only recently recognized challenge to understand the anthropology of complex societies generally.

The Peasants of Languedoc. By Emmanuel Le Roy Ladurie. Translated by John Day. Urbana: University of Illinois Press, 1976. Pp. 370. $16.00 (cloth); $4.50 (paper).

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As a general rule, sociologists have not taken a great intellectual interest in regional historical studies, often finding them too "particularistic" for their broader comparative tastes. This trend is unfortunate, because it is often in regional studies which confront historical archival materials directly that the sociologist can observe the detailed working out of a specific theoretical thread. Although many regional studies remain so close to their sources as to prove theoretically insignificant, the school of French historians that have gathered around the journal *Annales: Economies, sociétés, civilisations* have produced a set of historical pieces that are both historically concrete and specific, while remaining informed by larger social theory.

Emmanuel Le Roy Ladurie, currently a coeditor of *Annales,* is one such historian, with whom comparative sociologists researching the historical development of capitalism in Western Europe should become more familiar. The recent translation of his major work, *The Peasants of Languedoc* (first published in French in 1966), may at last make Le Roy Ladurie's comprehensive historical research and illuminating theoretical insights accessible to a wider audience of social scientists.

Through a careful reconstruction of his data (supplied by village records of the payments of the *taille* and the local *compoix* records for Languedoc), Le Roy Ladurie sets out to search for the dynamics of the transition from feudalism to capitalism from the late 15th through the early 18th centuries. But instead of uncovering the mechanisms of a transition from one mode of production to another, he details a dynamic and active society confronting larger demographic and agrarian productive trends. With seismographic precision he records the subtle trends in agricultural productivity, population growth, prices, and wages, and unfolds a "great agrarian cycle" instead of the more linear transition to capitalism.

The central arguments of *The Peasants of Languedoc* can most easily be discerned by briefly outlining Le Roy Ladurie's periodization of the history of the region. The first phase (roughly the 15th century), characterized by an abundance of land and a shortage of population (he calls it the era of the "rare man"), led to a consolidation of agricultural holdings into larger estates so as to maintain production levels. The second phase (16th century) ushered in an economic advance based on "the simple accumulation of endogenous factors" (a series of good harvests, introduction of new precious metals into circulation, new trade routes, urban growth) (p. 290). The contradiction of this phase (the "dynamic elasticity of the population" coupled with the "stubborn inelasticity of agricultural produc-

tion") led to land rationing through the "accelerated subdivision of tenures" (p. 291). Simply put in Malthusian terms, population increased in response to favorable economic conditions provided during the first phase. In the third phase (17th century), the agrarian development matured, and, as agricultural production began to increase slightly, the population of Languedoc declined slightly (in contrast with the rest of Europe, which witnessed a slight increase). In the fourth and final phase, a long recession was ushered in as aggregate production declined and population declined more dramatically than in the previous phase. With the fourth phase, then, a two-century-old process of land subdivision and population growth came to an end.

Le Roy Ladurie's account of the periods of the great agrarian cycle is not a simple agricultural history, however. He is most interested in the consequences of the larger economic and demographic developments on the structural position and mentality of the peasantry in Languedoc.

He describes a process of "pauperization" of the peasantry during the 16th century, noting that, "with population growing rapidly and aggregate income hardly at all, average per capita income . . . tended inevitably to contract" (p. 81). A similar process developed among wage workers, whose pauperization, "with work harder to find and real income shrinking, paralleled that of the peasants whose holdings were breaking up and contracting in size" (p. 114). Interestingly, he discovers that the lag in the wages of women workers was most dramatic and concludes that women were the major victims of the process of pauperization (p. 112).

The discussion of structural positions of the peasantry and wage workers is highly significant, for it leads Le Roy Ladurie into the contemporary debate concerning the development of a capitalist mode of production (or "world-economy," in the words of Immanuel Wallerstein) through the commercialization of agriculture in the 16th century. Although his discussion is informed by both Marxist theory and Weberian understanding of the significance of culture, Le Roy Ladurie takes issue with Wallerstein's thesis that the commercialization of agriculture leads to capitalism by galvanizing the rural population into classes of agricultural wage earners and capitalist farmers. While he acknowledges that rural capitalism was a nascent force in 16th-century Languedoc, the region was not penetrated by a capitalist world economy of the type that Wallerstein describes. Le Roy Ladurie argues that this region was able to generate surplus value only by the reduction of wages and not by the more capitalist measures of land concentration (land was still being subdivided) or increases in commodity production (aggregate production was not increasing at all). The economy, he states, "remained incapable of secreting a capitalistic social system—capitalism is not built on poverty" (p. 144).

Of course, capitalism *is* built on poverty; but on a specific type of poverty that was not evidenced in Languedoc. The peasants were not becoming incorporated into a relationship based on the contradiction of wage labor and capital, but were instead the "cultivators who had assumed the working management of their domains" (p. 115). Similarly, the identifiable

bourgeoisie were not commercializing rural production for any motives that might be considered capitalist; rather, the bourgeoisie, "to the extent that it accepted the role of *rentier,* disavowed its capitalistic character at the same time that it adopted the aristocratic life style of the 'gentleman of leisure' " (p. 115).

Following the lead of his *Annales* mentors, Marc Bloch and Lucien Febvre, Le Roy Ladurie also attempts to understand the mentality of the peasant population of Languedoc during these centuries of transition. Here, the sophistication of his careful linking of long-run demographic and agrarian trends with the structural positions of various classes is marred by his embrace of mass-psychological explanations of peasant activity. His mass psychology leads him to conclude that the basis for collective action and revolt among the peasantry was irrational. During the peasant revolts, he argues, "one witnesses in the behavior of the popular masses the recurrence of a *pensée sauvage*" (p. 210). Another revolt is characterized as "typical of a certain mass psychology and of a more instinctive than rational reaction to the rural crisis" (p. 269). In this type of explanation, he discounts peasant activity as potentially rational responses to fluctuations in the prices of certain staple commodities, a perspective which George Rudé has proposed to explain urban riots during the era of the French Revolution.

However, in spite of this overpsychologization of the irrationality of the peasant reactions, Le Roy Ladurie argues forcefully the impact of the clash of two cultures in Languedoc. While the traditional peasant culture gravitated toward sorcery, witchcraft, and Satanism, Huguenot culture introduced not only the national language and high levels of literacy but also Protestant asceticism as a grounding for economic action. As a corollary to his general Weberian understanding of the connection between religious beliefs and economic activity, Le Roy Ladurie demonstrates that Huguenot culture and rationalizing economic action appealed most to the weavers, spinners, and tanners; that is, to artisans and craftspeople. Protestantism did not achieve universal appeal in Languedoc (nor, we would suspect, anywhere else) but appealed most strongly to those groups in an economic position which stood to gain most directly by the adoption of the Huguenot credo.

The study is liberally sprinkled with anecdotes and abbreviated biographical sketches. The subjects of these sketches are seen as modal or typical of a certain pattern of behavioral responses to the larger-scale agrarian and commercial developments. Thus, they add a significant human dimension to such a broad history.

Le Roy Ladurie's central thesis of a large-scale agrarian cycle as a function of both population and productivity (in this sense both Malthusian and Ricardian) is designed as applicable to a wider number of cases; indeed, it is interesting that he labels what we might consider a regional history a "comparative study." However, with few exceptions, the comparisons are implicit and require a well-grounded historical knowledge of the agrarian structure of England and of France's north and Midi. (In

a brief but eloquent passage, however, he does draw some parallels to neighboring Catalonia.) But Le Roy Ladurie has also shown the historical uniqueness of Languedoc on two important levels. The nobility in Languedoc did not appear to exhibit the same level of power and dynamism as the nobility in the north of France. Consequently, as their structural power was less to begin with, they did not engage in a seigneurial reaction characteristic of the northern nobles as their power was eroded by the monarchy. Second, the tightly knit autonomous peasant communities capable of defending themselves against noble encroachment, which Bloch observed in his studies of French rural society in the north, are not as evident in Languedoc.

Altogether, Le Roy Ladurie has written a brilliant history (masterfully translated by John Day) that synthesizes large-scale agrarian and demographic trends with a social history of peasant activity and consciousness. Moreover, he has remained remarkably faithful to Marc Bloch's famous dictum regarding the use of comparative historical methodology: to paraphrase, without giving undue causal weight to local pseudo causes, he has remained sensitive to specific historical differences.

Affirmative Discrimination: Ethnic Inequality and Public Policy. By Nathan Glazer. New York: Basic Books, 1975. Pp. vii+248. $10.95.

Troy Duster

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This book is an unapologetic attack on affirmative action by a well-known Harvard sociologist long associated with a circle of influential opinion leaders on the Eastern Seaboard. Nathan Glazer has often teamed or jointly authored with social science notables like David Riesman (*The Lonely Crowd*) and Daniel Patrick Moynihan (*Beyond the Melting Pot*). But he is, in his own right, a respected essayist and contributor to *Commentary* and *The Public Interest.* He frequently plays the dual role of social analyst and political advocate, and the separation of these two roles is vital to a review of this book. I shall devote much of my time to his social analysis, but I would be remiss if I did not set these remarks in the larger context and the sociopolitical frame that generated the book. The Supreme Court's ruling on affirmative action in the Bakke case (unknown at this writing) will very likely join *Dred Scott, Plessy* v. *Ferguson,* and *Brown* v. *Board of Education* as one of the four great landmark judgments on racial and ethnic equality over two centuries. Despite occasional claims from the legal profession about the transcendence of the Court, the social and political environment best explains how and why the Court decided in 1857 that a black was *to be* three-fiifths of a white; why in 1898 blacks were *to be* "separate but equal," why in 1954 racial and ethnic minorities were *to be* "integrated," and why in 1978 members of racial and ethnic

minorities are *to be* treated as if racial and ethnic discrimination were a thing of the past.

There is considerable political and intellectual disagreement as to why and how the Bakke decision will be made. I disagree with Glazer about how its context should be analyzed, but I shall try to represent his basic position accurately and fairly.

The United States, says Glazer, has made a fundamental national commitment to the rights of the individual. The path has been ragged and uneven, he says, but was basically correct until the past decade ushered in affirmative action. This fundamental commitment, says Glazer, has been to ignore the ethnic or racial membership, or any other group membership, of an individual and to emphasize only personal attributes when the individual seeks employment, advancement, or entry into a course of study or professional training. (He acknowledges the major exceptions of blacks in slavery and Reconstruction, even up through the post-World War II era; he acknowledges also the major exception of the treatment and near genocide of the Native American population.) He cites admittedly "interpretable" evidence to buttress his view of the long evolutionary process that, he says, has even incorporated blacks in the past half century.

Now he is deeply troubled, because he has noted that in the past decade the nation has embarked upon a program that contradicts this earlier and correct course. By requiring prospective employers and educational admissions committees to take note of the ethnic and racial and sexual status of individuals, the new affirmative action guidelines bring in a group measure of quotas and proportions, independent of individual attributes. Glazer strenuously objects to the use of statistical proportions as a criterion for assessing whether or not there has been discrimination, because, he says, statistics cannot tell us whether there has been discrimination against the individual.

The core of Glazer's analysis, then, I believe it is fair to say, is the concern for individual rights to the near exclusion of an analysis of the social stratification in which individual rights exist. A central problem for any society is the degree of consensus achieved on where the sovereignty of the individual ends and where the interests of larger units begin. As one reads through the major parts of this book, however, there is only the slightest glimpse of people moving through institutions and communities, kinship groupings and tribes, profoundly committed to each, profoundly influenced by each. (A postscript on white ethnic reaction is the exception.) Glazer would have us consider only individual effort and individual attributes. This is a remarkable reversal for the coauthor of *Beyond the Melting Pot*. The principal argument of that monograph set forth the model of groups pressing their group interests and the advancement of persons ethnically identified. In that earlier work, Glazer and Moynihan saw blacks as relatively weakened precisely because of the relative lack of group leverage they could muster in the local political arena. Italians, Irish, and Jews used local government, sometimes with benign favoritism, sometimes with unsavory graft and corruption, to advance the condition

of their respective ethnic groups and group members. Cut off by the stranglehold of these ethnic interests at the local level, blacks, Chicanos, Puerto Ricans, Asian-Americans, and, to some extent, women now attempt to use the federal wedge. The quotas of these latter groups are therefore out in the open for public scrutiny, not under the table or behind closed doors or in the cigar-smoke-filled back rooms at City Hall where police captains and fire chiefs are appointed and where local business and construction deals get made in this nation so committed to individual merit.

As the empirical starting point for the formulations of the present book, Glazer makes two critical assertions: (1) that blacks are making greater gains than whites in the occupational sphere (p. 42), and (2) that, holding constant father's occupation, highest grade in school, and achievement in school measured by tests, there was (he endorses a quotation) ". . . the nearly complete elimination of racial bias in the way that the labor market assigned individuals to occupations . . ." in the 1960s (p. 43). On his first point, Glazer reports that when blacks and other minority races are combined, they were 4.9% of professional and technical workers in 1963, but 8.2% in 1973. This is a 67% gain. He compares the size of this gain with a more modest gain by whites, from 12.6% to 14.2% during the same decade. But I must point out that, if whites had a parallel 67% increase among professional and technical workers, they would have jumped from 12.6% to 21%. Quite simply, if one group starts out with a great advantage over another, and both groups maintain similar rates of increase, the advantaged group not only will always keep its advantage, it will increase it.

However, it is not at all clear that blacks are making gains on whites in the full occupational sphere, and this relates to both of his central points. He cites figures that hold constant father's occupation and education. For certain analytic purposes, holding constant is an elemental and important device. However, to hold constant in this area of inquiry is to misdirect the analysis, for it compares only well-to-do whites with well-to-do blacks. Certainly, if individuals start out with similar advantages and resources, they have some chance at ending up at similar points. To hold constant socioeconomic and educational factors is to conveniently skirt the problem revealed in the Secretary of Labor's report for the summer of 1977, when the highest unemployment rate ever was recorded for black youth, accompanied by a simultaneous lowering of the unemployment rate among white youth. If we follow Glazer's analysis and emphasis, we are led to try to understand these rate differentials by attending to the individual meritorious attributes of white youth. Yet other empirical studies have shown that when education is held constant, whites do much better economically than blacks, Chicanos, and Asian-Americans. Indeed, Paul M. Siegel, in "On the Cost of Being Negro" (*Sociological Inquiry* 35 [Winter 1965]: 41–57), showed something that should be astonishing to Glazer, given his analysis: according to Siegel's data for the 1960s, the better blacks do with education, the greater the differential between blacks and whites in economic success. Whites have the striking advantage. Glazer is quite correct, of course, in his insistence that attention to rates, statistics, and

proportions ignores the individual. On that I have no quarrel. But he refuses to employ his social analysis to account for the systemic and recurring rate differences in unemployment by race or, more dramatically, for the ethnic and racial stratification in the total employment picture.

It is the task of the sociologist to point out that, while individuals may believe that they possess only individual attributes, the social consequences of those attributes may constitute great social privilege or great social disadvantage. Contrast two ways of looking at the father's occupation. One is to treat it as the son or daughter often does, as an individual phenomenon, something to "be like" (or unlike) when one grows up. Yet, even in Glazer's version of the United States, father's occupation is among the most important determinants of one's social and economic position in later life. There is therefore something about "father's occupation" that is more than just a mere attribute of the individual, more than an individual "merit" that explains one's eventual place in the social order.

So it is with sex, ethnicity, and race. Women wind up as secretaries, nurses, and cocktail waitresses systematically. Men wind up as lawyers, top corporation executives, and professors systematically. A social analyst could hardly propose that these distributions reflect merit based upon individual attributes rather than patterned pressures that structure economic and social organization.

Yet Glazer's analytic preoccupation with individuals leads him to the following extraordinary position (for a sociologist qua sociologist): "It is one thing to be asked to fight discrimination against the competent, hardworking, and law-abiding; it is quite another to be asked to fight discrimination against the less competent or incompetent and criminally inclined. The statistical emphasis leads to the latter" (p. 67). For Glazer, it is an individual matter to be "criminally inclined." For Glazer, "incompetence" is an individual attribute. Professor of Education and Social Structure at Harvard University, he provides little analysis of the social structural frame in which incompetence and criminal inclination may be better understood. Consider, for example, the recorded homicide rate among blacks, more than double that of whites in the United States. We would not take seriously a sociologist who argued that this is because of the individual criminal inclinations of particular blacks. Bohanan long ago dispelled the racial theory of black homicide in America by his comparative study of African tribes and their exceptionally low rates of homicide. The rate must therefore be explainable by the social, political, and economic context in which blacks find themselves in America.

I began by making a separation between Glazer as social analyst and Glazer as political advocate. In this light, it is important to note that he is not characterizing ethnic or racial group membership as unimportant in human affairs. He is saying that the nation should develop a public policy as if it were unimportant. This is a strong normative statement, not a descriptive account of the world as it is. It is primarily political advocacy, and for those who want resonance with the anger and resentment that affirmative action programs have generated, this book has its many moments.

It is hardly a sociological analysis showing clearly why that anger has been confused and misdirected. Such a book has yet to be written, but the explanation probably rests with the notion that the wedge for group interests is now federal instead of local, out in the open and not behind closed doors, and threatens interests already established and privileged.

Black Ohio and the Color Line, 1860–1915. By David A. Gerber. Urbana: University of Illinois Press, 1976. Pp. xii+500. $14.95.

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Black Ohio and the Color Line is a brilliant examination of race relations, black thought, and black community development in a single state between the Civil War and World War I. Focusing mainly upon the growing black presence in Ohio's major cities, David Gerber, a historian, carefully analyzes the political, economic, and social ramifications of urbanization. The result is an illuminating and informative case study that will enrich courses in intergroup relations and urban and political sociology.

In Ohio's racial traditions, the repression of blacks was similar to that in most other Northern states—in some ways it was even more onerous. Because the state bordered on the South, not surprisingly its earliest white settlers came from Dixie and retained their close economic and social connections with that region. Moreover, in the southern and central portions of Ohio, where most blacks lived, conditions were especially harsh and often tense. Yet northern Ohio, particularly the northeastern counties of the Western Reserve, had been settled by a heavy contingent of New Englanders and was known for its racial liberalism. For example, only in the northern area were blacks not relegated to a Jim Crow education—they were welcomed in the Cleveland public schools in the 1840s by a board of education that hired black teachers for mixed classrooms. And only in the northern counties did blacks obtain the ballot before the Civil War.

Gerber notes that during Reconstruction the federal government's temporary embrace of equalitarian ideals forged an alliance with blacks in the South which ushered in "the most significant ameliorative trend in the history of American race relations" (p. 279); this development had important repercussions outside the South. In Ohio race discrimination, while continuing to exist especially in the job market, became less respectable; blacks, less isolated than ever before, pressed for entrance into the mainstream. Gerber demonstrates that for a brief period—less than a quarter century after 1870—the color line in Ohio wavered as white elites offered sympathy and support for the aspirations of their black counterparts. Contacts and social relationships between blacks and whites increased. Moreover, some of these influential whites helped blacks launch careers and aided them in legal and extralegal struggles against the color line. In this

climate of increasing social acceptance, it was not uncommon for black businessmen to have white trade or for black lawyers and doctors to have white clients and patients. During this period Ohio's black leaders were optimistic and assimilationist, as they saw themselves only a short distance away from full acceptance into the American mainstream. In their view the country was beginning to live up to its equalitarian principles and was beginning to create a color-blind society that would allow blacks to compete freely and fairly with whites. Industriousness and respectability were being rewarded by acceptance and upward mobility.

Aside from new-found white friends, Ohio's blacks had a new weapon —the ballot—and they began using it with increasing sophistication. Among their achievements were a state public accommodations law (1884, with amendments in 1894) and a bill repealing both the Jim Crow school law and an old law forbidding marriages across the color line (1887). Though hardly more than 3% of Ohio's electorate during this period, blacks profited from the fact that they supplied the margin of victory in numerous election contests which were regularly very close.

At first the Republicans, for whom they voted overwhelmingly, tried to take them for granted, but things began changing in the late 1870s when some Afro-American leaders threatened to defect to the Democrats, who were coveting the black vote. To nip this discontent the Republicans increased patronage among the race, but that only made the Democrats redouble their efforts, and in 1883 a Democrat became governor with some black support. Following that election blacks pushed the Democrats to sponsor a civil rights bill, and the new governor gave his enthusiastic backing. Not to be outdone, the Republicans embraced the issue, and in 1884, with both parties introducing their own measures into the legislature, a civil rights bill was passed unanimously.

Running for reelection the following year, the governor placed his weight behind those who sought to repeal the Jim Crow school law and the antimiscegenation law. To counter the Democrats, the Republicans nominated more blacks to the legislature than ever before and won with overwhelming black support. Thereupon the blacks, in pressing the GOP hard to repeal the two hated laws, held countless mass meetings and inundated the state capitol with petitions and lobbying delegations. In 1887 their campaign got the statutes stricken from the state code. But instead of prefiguring the color-blind revolution in Ohio that the blacks believed was imminent, their victory actually occurred at a time when race relations nationally were skidding into a "precipitous decline"—in the South where the federal government's withdrawal cost blacks their citizenship rights and in the Northern states like Ohio where the Southern model showed that blacks could be subordinated again. All over Ohio, including the Western Reserve, racist public opinion and segregation increased. By 1910, for example, rare was the Cleveland hotel, restaurant, or theater which did not turn blacks away or insultingly attempt to Jim Crow them. Even the Cleveland YMCA,

one of the very few in Ohio which had ever admitted blacks, now closed its doors to them.

Racial tensions were exacerbated in this period by the growing black migration to Ohio's cities, which the whites saw as a threat. (The state's blacks were predominantly urban after 1890 and tended to live in or near the largest cities.) Full-blown ghettos did not yet exist and blacks were still scattered in various neighborhoods, but after 1890 black clustering grew substantially, with street after street becoming increasingly black. In all the urban wards of the state, blacks remained a numerical minority, but these enclaves would become the base for later ghettos. Actually, these demographic factors should have been converted into more political power for the race, except for the fact that after the mid-1890s the Republicans began winning elections by such wide margins that courting blacks was no longer necessary and the value of their votes plummeted.

This "alarming deterioration of race relations" (p. 271) jarred Ohio's old-line black leaders. While they did not surrender their dream of the American mainstream, they had less confidence in its imminent realization. At the turn of the century they were challenged by a younger generation of upwardly mobile racial spokesmen who argued that, in the increasingly hostile climate, it was clear that since full equality could hardly come soon, a new racial strategy was imperative. Drawing upon an ideology of black capitalism popularized by Booker T. Washington, these younger critics urged a shift in emphasis away from integration to a program stressing race pride, self-help, and solidarity. They called upon blacks to capitalize on their increasing population and develop their own inner resources, embracing voluntary separation to build vibrant racial institutions. For many in the older black elite, such counsel was heresy, and they condemned the younger generation as "Jim Crow Negroes" who were tools or dupes of bigoted whites. Yet under pressure of these separatist arguments and the bitter realization that now there were fewer white allies, some of the older leaders reluctantly modified their views and embarked on "a quiet, voluntary, and partial retreat from the integrationist ethic of the past" (p. 329). Thus, for example, in several Ohio cities certain members of the black upper class who had been connected with white religious denominations withdrew to form their own church congregations. In city after city, the wave of the future was with the younger leaders whose dream was the creation of a great black metropolis. The bitter racial climate gradually forced the two generations of race leaders to work together to create separate institutions such as black Y's that served the impoverished black migrants. Only the great depression of the 1930s would tarnish the separatist dream and set the stage for new black challenges to the color line.

In selecting Ohio for his case study, Gerber has indeed chosen well. Typically American, Ohio, with its northern and southern exposures, mirrored the breadth of American race relations and the black experience, not only within its geographical variations but also over time. *Black Ohio and the Color Line* is a useful corrective for those sociologists who tend to view

American race relations as characterized by an unchanging pattern of white racism. The volume is also instructive because of the sophisticated way in which Gerber analyzes the complex interrelationships between these societal changes and black ideologies and tactics. With his extraordinary depth research the author has exploited a mine of information to produce a monograph that should be welcomed by a whole range of behavioral scientists.

The Democratic Party and the Negro: Northern and National Politics, 1868–92. By Lawrence Grossman. Urbana: University of Illinois Press, 1976. Pp. xi+212. $9.95.

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This is an interesting book about the uses the Democrats (and the Republicans) made of blacks and issues concerning them during the post-Reconstruction years. Detailed sources and arduous work are evident. Congressional records, historical collections and letters, newspapers, and other primary and secondary sources are skillfully used.

Grossman shows that issues of current and recent concern were also current 100 years ago. For example, white politicians in 1875 referred to blacks as "Sambo"; the children's tale alluded to was withdrawn from libraries in the 1960s. Recent arguments for federal protection of black rights had their counterparts in the 19th century. Grossman discusses failed and successful state and national civil rights acts before 1900.

Some of the interesting facts brought to the reader's attention may be surprising even though they are also recorded in other books. In the late 1800s repressive laws against blacks still existed in Ohio. Some blacks in the same period argued that their children should go to schools with black teachers so that they might have adults of their own color to emulate. The governor of Ohio argued against this view, stating that if integration displaced some black teachers it would do so only in the way that abolition of slavery deprived some black trusties of their positions.

Grossman's central mode of analysis is essentially political, not sociological or socioeconomic. A single reference to sociological theory, somewhat of an afterthought, occurs at the end of the book, where Merton's latent-manifest paradigm is cited. Almost all the discussion focuses on the maintenance of political power or competition for it. The actions of the white politicians are seen as efforts to use the black vote for party power or for the sectional interests of the whites in each party. The author provides several useful tables in the book, all based upon political data—roll calls in congressional votes, divided by national regions and by party.

Socioeconomic factors receive slight treatment. Chapter 3 discusses the shift of the black vote from the Republicans to the Democrats, previously

perceived as the enemy party, and grapples with the question of the reason for this change. Some attention is given to personalities of the chief actors in the transition. Chapter 4 largely concerns Grover Cleveland, whose desire for political power appears to be the chief independent variable.

The book is not so much about politicians *and* the Negro as about politicians *on* the Negro. Attention is given to the activities of black politicians, but mostly in terms of reactions of whites to them. The conditions affecting the opinions of the electorate, black and white, Northern, Southern, Western, etc., are not analyzed in detail.

Persons interested in the mixed position of the Democratic Party—liberal in the North and not so liberal in the South (a pattern only recently changing, if it is)—and those interested in the demise of the Republican Southern influence (until its recent resurrection) will find this book interesting.

This is an excellent, readable, scholarly presentation of an important period in the history of this nation, especially with reference to the actions of Democratic leaders in regard to blacks. Historians, political scientists, sociologists, and others interested in both the present and the past will find the book valuable.

White Liberals in Transition: Current Dilemmas of Ethnic Integration. By Judith Caditz. New York: Spectrum Publications, 1976. Pp. 187. $12.50.

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Judith Caditz's book is an examination of the social phenomenon of white liberals "retreating from their general [avowed] principles when confronted by [specific] interracial situations" (p. 1). It is based on an empirical study of a random sample of 204 white men and women defined as liberal because of their membership in a nationally recognized liberal organization and their general expressed beliefs in integration. The sample of 97 women and 107 men was selected from the Los Angeles County section of the national organization (never identified), and the interviews were conducted between October 9 and December 15, 1969. Open-ended interviews were used to determine the respondents' attitudes to six controversial interracial situations: (1) busing in order to achieve racial integration in schools, (2) the quota system as a basis for increasing minority participation in institutions of higher learning, (3) blacks moving into the respondent's neighborhood, (4) rental of apartments to blacks in white-occupied apartment buildings hypothetically owned by the respondent, (5) the hiring of blacks, and (6) the entrance of blacks into the respondent's occupational field. Caditz finds that despite high educational levels and high income and occupational levels a large number of the respondents expressed reservations concerning blacks. Their attitudes to the six interracial situations were found to fit the Guttman Scale. They ranged from completely favorable through condition-

ally favorable and ambivalent to completely unfavorable. The order of the six situations in terms of increasingly unfavorable reactions was (1) occupation, (2) hiring, (3) apartment, (4) residence, (5) busing, and (6) quota.

The independent variables that Caditz found salient in distinguishing favorable attitudes from unfavorable ones were the degree of emphasis on or concern with achieving social status and maintaining it through one's offspring, the degree of identification with one's ethnic group, and the extent of interaction outside one's ethnic, religious, or racial group. From tabular analyses she found, among other things, that liberals who are highly status conscious, who identify highly with an ethnic group, and who interact primarily within their own ethnic, religious, or racial group are more likely to harbor negative or ambivalent attitudes concerning the six controversial situations. Caditz's conclusions are somewhat contradictory. On the one hand, she sees the data supporting Weber's notion of society comprising status groups always competing for power and recognition. On the other hand, she seems to take optimistic comfort in the fact that "even though particular [white liberals] send their children to private schools when busing programs are instituted, they will continue to fight for integration at the institutional level, in the legislatures and the courts" (p. 170).

While I disagree with some of the conclusions, particularly Caditz's optimism over the effectiveness of liberal organizations despite individual retreat from liberalism, I find the book very interesting and the topic timely. I also appreciate the author's attempt to relate the empirical results to existing theoretical models.

There are several flaws in presentation. The text is based largely on previously published articles by the author, and the organization and flow show this painfully. The book is uneven and does not seem to hang together as a whole. For instance, in chapter 3, in presenting the breakdown of attitudes to the six interracial situations, the author does not always give the various percentages of those who respond completely favorably, those who respond conditionally favorably, those who respond with ambivalence, and those who respond completely unfavorably. So the reader is not clearly presented with the overall distribution of the responses. In general, Caditz would have done well to incorporate more tables illustrating her findings, perhaps in the appendix, instead of referring the reader to relevant tables in her previous articles. It would have been helpful, too, to include the research design and the interview schedule. It would have been useful to elaborate more on the criteria used in selecting membership in the national organization as the operational definition of the concept "white liberal."

Despite these drawbacks, I recommend this book for undergraduate and graduate courses in race relations and social psychology, especially courses that try to include empirical texts along with theoretical ones. The reading is easy enough for undergraduates, the statistical discussions are manage-

able, and the book is generously interspersed with direct quotations from the respondents, which makes the reading livelier.

Race and Suicide in South Africa. By Fatima Meer. London: Routledge & Kegan Paul, 1976. Pp. 319. $15.50.

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The past decade has seen the emergence of "suicidology" as a specialized field of study in its own right, with its own professional association, the American Association of Suicidology, and its own journal, the *Bulletin of Suicidology*. The same decade has seen the emergence of race relations in South Africa as a specialized field of study, with its own journal, the *Journal of Southern African Studies* (*JSAS*). Fatima Meer, by focusing on both of these controversial areas of sociology, has insured widespread interest in her book.

Seeing suicide as both a personal and a social phenomenon, Meer tries to synthesize these two dimensions in her study. Her methodology is Durkheimian in her use of the Inquest Court Record and phenomenological in her use of case studies. Thus, while she accepts Jeremy Jacobs's phenomenological position that suicide notes are a highly meaningful form of communication from the deceased (she presents a fourfold classification of suicide notes in chap. 20), she does not see these notes in themselves explaining suicide. To explain, she argues, the researcher would have to go outside the notes to the court records (Jeremy Jacobs, "A Phenomenological Study of Suicide Notes," *Social Problems* 15 [1967]: 60–72).

However, official statistics clearly have their limitations. Meer argues that they report fewer cases than actually occur because magistrates tend to give suicides the benefit of the doubt, and Africans in particular suffer from ineffective recording machinery. She cites the migrant nature of the population, the difficulties of locating relations and sometimes witnesses, and the many instances of bodies being found days after the event as explanations for the ineffective recording of African suicides. Consequently, she adds to the figures given by the magistrate, increasing the number of suicides between 1940 and 1960 among Indians, Coloureds, and whites in Durban from the official 707 to 830. (My calculation of her table 5 gives a figure of 930, not 830.) Since no criterion for the change in number is given, the reader is left uncertain why he should be any more ready to accept the author's statistics than those of the magistrate.

The main body of the statistical research pertains to 2,238 cases of suicide occurring in Durban during the period from 1940 to 1970; they are analyzed in terms of numerous variables. The observed suicide patterns in relation to sex, age, race, religion, residential area, income, and occupation do not corroborate the association of higher suicide rate with high status as proposed by Durkheim and later suicidologists. On the contrary, the

Durban data establish a relationship between high suicide rate and poverty. The findings challenge the idea that suicide conforms to certain universal principles. They emphasize the crucial role of particular socioeconomic conditions leading to a common emotional syndrome among suicides. Thus, paradoxically, white affluence and black poverty work in different ways to drive certain people to commit suicide. The author concludes, in Durkheimian fashion, that suicide is a problem of "integration," social and personal.

At times the implications of the data are inadequately explored. For instance, Meer observes in passing a possible relationship between rapid urbanization among Africans in the Second World War and a rapid rise in the African suicide rate. However, she dismisses any attempt to correlate historical change with suicide as "futile." Similarly, she notes the uncommon tendency for homicide rates to be much higher than suicide rates among Coloureds and Africans. She suggests that Coloureds and African men are more prone to externalizing aggression than to internalizing it (see Thomas F. Pettigrew, *A Profile of the Negro American* [Princeton, N.J.: Van Nostrand, 1964], p. 78, for a similar argument on American blacks). However, these data on violence need to be related to a theory of the social structure and the manner in which violence is institutionalized in South Africa's distinctive racial capitalism.

This study provides new and interesting data on suicide. However, unless the data are located historically and a picture of how conflict is structured is given, the implications will remain inadequately explained.

Sociobiology and Behavior. By David P. Barash. New York: Elsevier Scientific Publishing Co., 1977. Pp. xv+378. $9.95 (cloth); $4.95 (paper).

Lee Ellis

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"If biology seems arrogant in seeking to include humans within its scope, think of the greater arrogance of a social science that refuses help when it is offered" (p. 277). Around this theme, David Barash has written an extraordinary document for understanding social behavior. Predictably, the reaction of sociologists to sociobiology has been mixed; but even the most ardent critic willing to read Barash's book with an open mind will surely judge it to be a well-documented, rigorously logical scientific achievement. For those willing to consider a sociobiological approach to the study of social behavior, there is probably no clearer basic outline available today than *Sociobiology and Behavior.*

Barash begins his book with an outline of the concepts under which sociobiology may be understood, which are, to a large degree, the same as those used in neo-Darwinian evolutionary theory. The terms and concepts are very clearly presented and illustrated throughout the first three chapters. These chapters make for somewhat laborious reading; but without

them, the meaning of much that follows is lost. With the conceptual foundation laid, the next six chapters are devoted to often fascinating accounts and analyses of the following forms of social behavior: (1) altruism; (2) grouping, sociality, and basic communication behavior; (3) sexual selectivity and pair bonding; (4) parenting behavior; (5) social competition and stratification; and (6) spatial competition and territoriality. A special final chapter is devoted to considering the implications of sociobiology for the study of human behavior, although many other implications are introduced in the previous six chapters as well.

The basic tenet of sociobiology is that social behavior is, to a considerable extent, the function of evolutionary forces impinging upon individuals, families, kinships, and, to some extent, even broader social groups to select those individuals and breeding populations well suited for a given habitat. Barash explains and illustrates very clearly that often genotypic traits that may be detrimental from the individual's standpoint (e.g., those inclining an organism toward altruism) may become more prevalent, not because they "benefit" the individual per se, but because they improve the chances of reproductive survival for conspecifics—some of whom would also be likely to carry the altruistic-tendency genotype. Survival of such genotypes would be especially likely if the altruistic tendencies were on behalf of organisms with similar genetic makeup. Of course, this is exactly what one observes in many species.

In the last chapter, Barash directly confronts the most politically and ethically sensitive aspects of sociobiology—what may be seen as (1) its racist and sexist overtones, and (2) its tendency to lend support to laissez faire conservatism or even genocide. Of course, within a sociobiological framework, it is impossible to deny that genetic factors may underlie differences in behavioral tendencies between races and the sexes; sociobiology would, in fact, predict such patterns. Cautiously conceding as much (pp. 283 and 310–11), Barash first emphatically iterates that hypothesizing genetic influence is not the same as arguing genetic determination. Behavior that is genetically influenced is generally quite immutable to environmental redirection once the significant environmental variables are manipulated. Second, he argues that just because a particular form of behavior might have evolutionary roots (he cites racial prejudice as a possibility—pp. 310–11), it cannot be said to follow that such behavior is "good," or that society is improper in seeking to suppress or environmentally override such a tendency.

My only significant criticism of Barash's book involves his distinction between "ultimate" and "proximate" causes (first explained on p. 37). He argues that the actual physiological and biochemical mechanisms of which any phenotype (e.g., behavioral trait) is a function should be considered the proximate causes. The ultimate causes of a phenotype are the natural selection factors operating to secure its existence in a given place and time. This perspective is contrasted with the most prevalent social science theories which assume that at least human social behavior is "ultimately" the function of individual or societal purposes (e.g., teleological). I agree with

Barash that evolutionary theory needs to have a prominent place in explaining the emergence and perpetuation of social behavior. However, the "ultimate" theoretical explanation of social behavior must account for *how* such behavior occurs and not merely *why*. In this regard, it is important to keep in mind that the elegance of a scientific theory is assessed mainly on the basis of its predictive power and that such power is demonstrated most convincingly in terms of control. Control is achieved via theory when the factors leading up to an event are described in sufficient detail so as to be manipulated, and thus the predicted behavioral outcome is achieved. Modern evolutionary theory offers an extremely powerful framework (or paradigm) within which to study the interacting influences of genetic, physiological, biochemical, and environmental forces upon social behavior. However, evolutionary theory does not describe how any specific forms of behavior are demonstrably controllable; and this, I submit, should be considered the ultimate theoretical explanation of social behavioral phenomena. I raise this criticism to make clear that the ultimate theoretical task for disciplines involved in studying social behavior is not achieved by viewing social behavior (human or otherwise) within an evolutionary context. I am sure that Barash would readily concede this point, but his choice of terms may mislead some readers and unnecessarily antagonize others. In no way do these remarks detract from the essential accuracy and sometimes almost startling insights achieved in his book. Because the book is such a carefully argued treatment of a very complicated subject, I will not attempt here to summarize any more of its major points.

Overall, Barash has obviously written an important book. It would serve as an excellent text for an introductory and, I think, mandatory, course in sociobiology. It demonstrates that the tremendous scientific interest given sociobiology since Edward O. Wilson first broadly proclaimed and defended its formulation in 1975 (*Sociobiology: The New Synthesis* [Cambridge, Mass.: Harvard University Press]) is amply justified.

Justice and Revolutionary Coalitions: A Test of Two Theories[1]

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Perceived injustice in dividing a group award is identified as an in portant cause of revolutionary coalition formation. Two theories justice processes, "the exchange theory" and "the balance theory are described and applied to experimental studies of coalitions. I these cases, three specific predictions may be made: (1) coalitic attempts will be more frequent in unfair cases than in fair ones, (2 members of a revolutionary coalition will attempt to restore equit under specified conditions, and (3) the precise response to inequit depends on which theory of justice is used to define "fairness." coalition experiment was conducted to test the predictions. Data co firm predictions (1) and (2); with regard to (3) they provide bett confirmation for the balance prediction than for the exchange pr diction.

Revolutionary coalitions—those in which subordinate members form a alliance to wrest control of their fates from the most powerful member- have long been of interest to sociologists and political scientists. On th small group level, such investigators as Caplow (1956), Vinacke and Arko (1957), and Gamson (1961) founded a major branch of research and theo by studying coalition formation processes. For the most part, studies in th tradition place all members in competition against each other and focus o bargaining processes between potential coalition partners as they offer var ous divisions of the winnings from a successful coalition.

We hope to extend this body of knowledge to include a study of structur conditions which predispose members to seek redress through forming a alliance against a group leader in a situation of initial cooperation. Th is, given an orientation toward teamwork, what conditions increase the lik lihood that a triad will decompose into a dyad and an outsider? Furthe

[1] Experiments were conducted at the University of South Carolina as part of Smith M A thesis, and this report was prepared during a faculty research leave granted t Webster. Edward J. Lawler, W. Edgar Vinacke, and two anonymous *AJS* reviewe gave helpful advice on earlier versions of this paper.

given that such a revolutionary coalition forms and coalition members win the power to affect the division of rewards to all members (not just the division among coalition partners), how will they redistribute the wealth?

As a start toward answering these questions, we examine two groups of studies, one on coalition games and the other on bargaining experiments; we focus on allocations, perceptions of justice, and attempts at revolution.

In a typical coalition game, three players are in competition for winnings (either money or points), and they begin with "status weights" which give them unequal chances for success. Game type is named by the status weights of members; for example, a 4-3-2 game is one in which member A has a weight of 4, B of 3, and C of 2. If no coalition is formed after players bargain, the member with the greatest weight wins, in this case member A. But if two members agree on a coalition, it gets their combined weight; in this case, a BC coalition would have a weight of 5 and would win. Members of a coalition split the winnings, and it is over division of winnings that the coalition bargaining takes place.[2]

Generally, the "cheapest winning coalition" is the one formed most often; in a 4-3-2 game, that would be a BC coalition. The reason seems to be that in bargaining with each other over potential coalitions, players expect to divide the coalition winnings in proportion to their weights. That is, a 3-2 coalition would divide the winnings by assigning 60% to person B and 40% to person C.[3]

This proportional division of winnings presents a curious issue, for the weights in no way reflect the importance of coalition partners. As Vinacke et al. (1966) and Kelley and Arrowood (1960) have pointed out, in a triad all members, regardless of their weights, are equally important: Any coalition will win; without joining a coalition (except in special cases such as a 3-1-1 game) an individual is almost certain to lose because the other two individuals will form a coalition. However, even in Kelley and Arrowood's experiment, in which this fact was emphasized to subjects, a strong tendency toward forming the cheapest winning coalition is evident.

Anderson (1967) proposed a distributive justice explanation for the pro-

[2] Many early games followed Vinacke and Arkoff's (1957) design, in which triad members were to play a board game with number of moves determined by their status weights. However, the game was almost never actually played, since either the weights or the coalition formed completely determines the winner. Thus most later research eliminates the board game and focuses attention of both researchers and subjects on bargaining over point division among coalition members.

[3] The first theory of coalition formation was Caplow's (1956, 1968), which explained coalition choices on the basis of attempts to maximize dominance over one's coalition partner and to minimize the partner's dominance over oneself. But the Vinacke and Arkoff (1957) data provide better support for Gamson's (1961, 1964) idea that coalition partners are more concerned with proportional division of winnings. Komorita and Moore (1976) review some recent studies of coalition processes and argue for a bargaining model to predict coalition formation and point division.

portional division of winnings. According to his analysis, in most cases the weights are all that differentiate players, so they treat the weights like other status characteristics when deciding who should get how much. That is, the weights take on more significance than simply a game advantage; they are desirable characteristics of actors, and according to justice conceptions, actors with more desirable properties should get a bigger share of rewards. Anderson supports his analysis with two types of data. A study by Emerson (1964) in which members were exactly equal (a 3-3-3 game) shows strong evidence of attempts by subjects to ensure equality of total winnings among members. A study by Hoffman, Festinger, and Lawrence (1954) in which members were differentiated not by weights but by perceived IQ shows remarkable evidence of attempts by subjects to "give" more points to a confederate member when he is described as having a superior IQ than when he is described as having a normal IQ. If we assume that both status weights and IQ function as evaluated characteristics in this situation, it looks as though players are trying to produce what they see as a fair division of overall winnings. Their conception of fairness may be described as follows: "Winnings of a player will be perceived as *fair* if (a) among players undifferentiated with respect to evaluated characteristics winnings are approximately equal; and (b) among players who are differentiated by some evaluated characteristic, winnings are approximately proportional to the distribution of evaluated characteristics" (Webster 1975, p. 264).

Anderson's analysis and this definition of "fairness" in coalitions receive good support in a study by Weil (1970). There, the proportional division of rewards was fixed at either 50-50 or 60-40. Both for players in a 3-3-2 situation and for those in a 3-2-2 situation, coalitions formed most often in accord with the permitted distribution of winnings. That is, when winnings must be split 50-50, individuals in a 3-3-2 game usually form 3-3 coalitions; those in a 3-2-2 game usually form 2-2 coalitions. When winnings must be split 60-40, individuals in both games usually form 3-2 coalitions.[4] Note that the 3-3 coalitions formed when winnings must be distributed 50-50 are not the cheapest winning coalitions in a 3-3-2 game, so this part of Weil's study is particularly important for support of a justice interpretation of coalition bargaining.

A second issue, especially important in natural settings such as political conventions and business organizations, is to determine structural conditions which are likely to produce revolutionary coalitions. As we noted above, most small group research on coalitions has been conducted in a special type of situation, one in which all group members are explicitly set in competition. But in many cases individuals begin interaction either cooperatively as a team, or at least not in direct opposition to each other. Without the

[4] Weil's unpublished study, available through Stanford University Library, is summarized in Webster (1975, pp. 266–69).

structural predisposition to coalition formation provided by a competitive game, we would in general expect the group to have no internal factions unless some additional disturbing influence were present.

Some research on bargaining, usually not conducted in a coalition framework, shows certain responses to perceived inequity in allocation of team awards.[5] In one representative study (Leventhal, Weiss, and Long 1969), subjects worked in dyads, one member of which was a confederate. The experiment had two phases. In both phases, dyad members worked on a set of arithmetic problems, for which they were paid $1.20 or $1.40. In phase 1, the subject was allowed to set the division of pay between himself and the (confederate) other member, and most subjects divided the pay evenly. This accords with a justice prediction, since dyad members were not differentiated by skill, power, or other characteristics. In phase 2, the confederate allocated the reward and the subject had an opportunity to effect a minor reallocation (5¢) if he desired. Consistent with a justice prediction: when the confederate divided the pay evenly, 100% of subjects agreed to his allocation; when he divided it unevenly, 79% of subjects reallocated; and of those reallocating, 90% reallocated toward an even distribution. Some other features of this study will be discussed below. But here we note that this experiment can be interpreted as showing a strong concern for justice, with attempts to reallocate to produce it, even though the amount of money that could be reallocated was too small to achieve justice (an equal distribution).

In both natural and experimental coalition situations (for example, Phillips and Nitz 1968; Barker and Janseiwicz 1970; Michener and Lyons 1972; Lawler 1975; Messe, Vallacher, and Phillips 1975), there is evidence to support the idea that a leader's unfairness can have a strong effect in promoting a revolution among subordinates. That is, if the superordinate member of a triad appropriates too large a share of the group's reward to himself, subordinate members are very likely to form an alliance against him. This is true even in the face of attempts by the leader to co-opt potential revolutionaries (Lawler and Thompson 1975). Of course we assume in addition that structural conditions do not make a revolutionary coalition hopeless—as they would, for instance, in a 3-1-1 game, which models a very stable authority structure.

Thus the picture that emerges from these two lines of inquiry shows that justice concerns play an important role in both. Coalition research shows that perceived unfairness of a leader promotes a revolution against him. A justice analysis proposes that this occurs because of the moral outrage associated with feelings of injustice, and it predicts that, if given the chance, members of a revolutionary coalition will try to reallocate group rewards to

[5] For an extensive review of this literature, see Leventhal (1976).

produce fairness. Bargaining research shows that when outcomes are not split equally among undifferentiated actors, actors will reallocate them toward more equality. A justice analysis proposes that they do so because fairness requires equality in this case and predicts that, if actors could reallocate enough money, they would try to produce equality. It further predicts that when actors are differentiated by some characteristic, they will attempt to reallocate to produce a consistent, proportional inequality of rewards.

In a real revolution, it is not always clear whether partners in the winning coalition actually are trying to restore a just distribution or whether they will take everything for themselves. What makes the difference, we claim, is whether there exists a stable set of norms defining fairness. Only when such a normative structure exists—whether given by the situation or inferred by the actors—are justice processes to be expected. We discuss this point more fully below. However, there are two major theories of distributive justice, and both predict that individuals will usually try to produce justice. In addition, both theories provide precise ideas of how much reward is "fair." We turn to a brief description of these theories.

The best-known and most widely applied theory of distributive justice processes is the exchange theory, developed by Homans (1958, [1961] 1974), Adams (Adams and Rosenbaum 1962; Adams and Jacobson 1964; Adams 1963, 1965), and most recently by Walster (Walster, Berscheid, and Walster 1976). It focuses on rewards and costs and upon a pair of actors in interaction. Justice is a property of the relationship when actor P's "inputs" (such things as skill, training, personal characteristics, length of tenure) and "outcomes" (such as salary) are proportional to actor O's inputs and outcomes. Thus justice is defined as follows:[6]

$$\frac{\text{outcomes}_P}{\text{inputs}_P} = \frac{\text{outcomes}_O}{\text{inputs}_O}$$

Any inequality between the ratios is defined as injustice; either to P's relative benefit or to his relative deprivation. In either case, justice is assumed

[6] This equation first appears in Adams (1965), though it is described verbally in Homans ([1961] 1974). Walster, Berscheid, and Walster (1976, pp. 2–4) identify an inadequacy, namely, that this formula is meaningless when one actor's inputs are negative They show that the definition of justice should be as follows, where O is "outcomes," I is "inputs," and exponents k_P and k_O are +1 or −1, depending on the sign of the actor's inputs:

$$\frac{O_P - I_P}{(|I_P|)^{k_P}} = \frac{O_O - I_O}{(|I_O|)^{k_O}}$$

We thank an anonymous *AJS* reader for this clarification. Because in the situations we deal with all inputs are positive, we use the simpler Adams definition, equivalent for these cases, in our exposition.

to be rewarding, and injustice, costly.[7] So we have a precise definition of justice in terms of inputs and outcomes and a theory which predicts that individuals will try to achieve justice in relations where it does not occur. This theory provides a rigorous explanation for all findings described above.

An alternative theory of justice processes, which we will call the balance theory, has been proposed by Berger, Zelditch, and their colleagues (Zelditch et al. 1970; Berger et al. 1972). For all situations discussed to this point, the exchange and the balance theories would make exactly the same predictions. However in some situations, including the experiment we report below, they differ.[8]

The balance theory of justice is described in terms of actors, their characteristics (which function similarly to inputs in the exchange theory), and goal objects (similar to outcomes). The theory has two parts, a referential structure and a local system. The referential structure is the relationship believed to exist in the world between characteristics and goal objects. For instance, if the focal actor P believes that people with the high state of the characteristic "education" generally also get the high state of the goal object "income," and vice versa, this information is contained in his referential structure. A referential structure is a set of norms which tell how rewards are distributed in the world.

Figure 1 shows such a structure, using balance diagrams following conventions developed by Harary, Norman, and Cartwright (1965).[9] The focal actor P is at the center. He perceives two groups of people, identified as X and Y. People in group X have characteristic A, and people in group Y have characteristic B. The characteristic can be anything with preferred and nonpreferred states, such as race, skill, sex, etc. Below, we shall refer to coalition status weights as the characteristics. Notice that P differ-

[7] In 1961 Homans identified anger as the emotion associated with unjust underpayment and guilt as the feeling associated with unjust overpayment. In the revised edition of the same work (1974, p 39), he identifies overpayment with *pleasure*, which would not be a motivation to restore justice. Whether this change represents a fundamental development in his thinking, or whether he had in mind other circumstances in which overpayment does not produce guilt, is not clear. The majority of sociologists probably would take overpayment as unpleasant, a view supported by the Adams (1965) experiments. This question is not crucial to what follows here, since we deal only with unjust underpayment.

[8] The 1970 paper is preliminary and identifies theoretical difficulties with the exchange theory: (1) it cannot deal effectively with such inputs as race and sex, which are difficult to quantify; (2) it does not account for the moral feelings such as outrage which accompany injustice; (3) it deals only with pairs of actors at a time, thus omitting many important situations of comparisons with referent actors; (4) it does not distinguish types or degrees of injustice. Our research provides data relevant to points 3 and 4.

[9] Balance diagrams do not appear in the articles cited. However, they appear to be consistent with the verbal propositions of the theory, and they convey the same information more clearly, in less space.
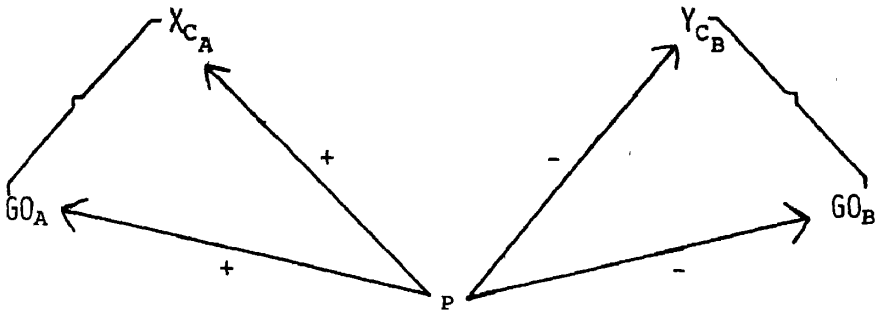
FIG. 1.—Balanced, unitary referential structure

entially evaluates the characteristics, indicated by the signs on the bonds between P and C_A, and P and C_B. In the referential structure, P perceives that states of the characteristics are regularly associated with states of goal objects (GO_A and GO_B). This association is shown with unit relations (indicated by braces; see below) between the actors' characteristics and the goal objects. Furthermore, the goal objects are differentially evaluated.

The referential structure thus is *balanced* because each cycle in it is positive. It is *unitary* in the sense that characteristic A is uniquely associated with goal object A and characteristic B is uniquely associated with goal object B. In other words, P does not believe there are significant exceptions to the rule: People with the A state of this characteristic get the A state of the corresponding goal object, and vice versa. *Justice processes occur only when there is a balanced, unitary referential structure.*

The second part of the theory is the local system, which shows interactions between P and other individuals. Figure 2 shows the complete diagram for this theory of justice, both referential structure and local system.

Two types of relationships are shown in figure 2. Straight lines are used for affect relations (evaluations of characteristics and goal objects), with the A states of each positively evaluated and the B states negatively evaluated by P (indicated by + or — on the lines from P to characteristics and goal objects). Braces are used for unit relations, a residual category comprising all perceived connections without affect: similarity, part of the whole, causal connection, relevance, etc. Unit relations linking characteristics and goal objects in the referential structure to characteristics and goal objects in the local system (the vertical braces) indicate that P perceives them to be "part of the same thing." For example, if GO_A in the referential structure is "high salary" and go_a in the local system is "$60,000 per year," the vertical unit relation indicates that P thinks of $60,000 among his associates as being a high salary. Local system characteristics and goal objects are specific instances of the general cases in the referential structure. Unit relations within either the referential structure or the local system which
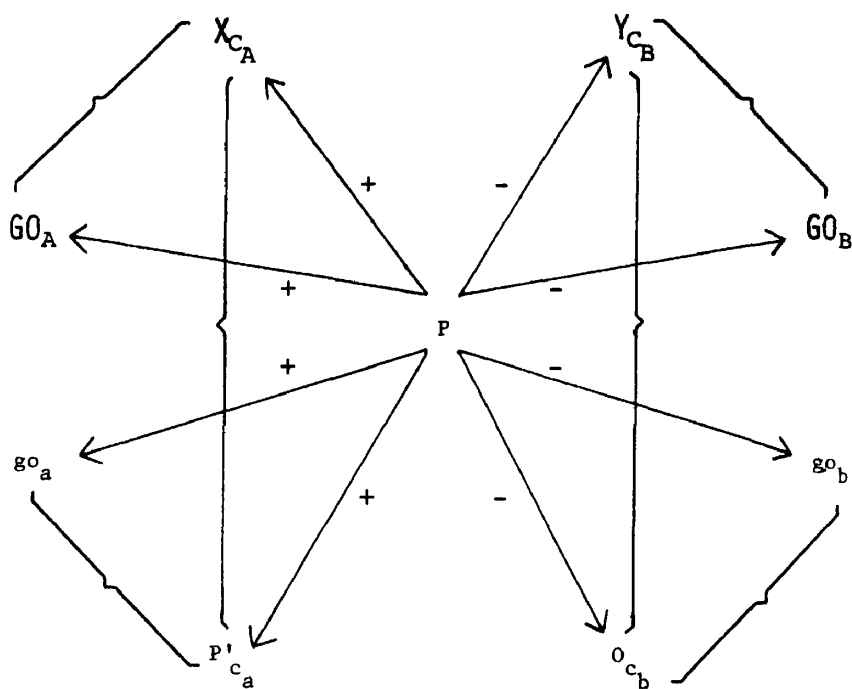
Fig. 2.—Referential structure with balanced local system

link characteristics to goal objects (the slanting braces in fig. 2) indicate norms of occurrence. For example, the slanting brace at top left could represent a belief that people with high education (C_A) regularly get a high income (GO_A).

To determine the balance of a structure, it is necessary to calculate whether all of its cycles (routes which start and end at the same point) are positive. The sign of a cycle is calculated by multiplying the signs of its paths; affect relations (straight lines) are positive or negative as indicated, and unit relations (braces) always are positive. Both cycles in the referential structure of figures 1 and 2 are positive, the one on the left having all positive paths and the one on the right having two negatives and one positive; thus, the referential structure is balanced. The same is true of the local system in figure 2.[10]

In the local system, moral aspects of the norms indicated by unit relations become especially salient. That is, if X-type people with characteristic C_A

[10] This description of balance diagrams is necessarily brief. Diagrams are intended to represent ideas of Heider's (1958) balance theory in a convenient form. A standard reference on balance diagrams is Harary et al. (1965). Readers unfamiliar with this type of theory may wish to consult either of these sources.

usually get a certain goal object GO_A in the referential structure, then they ought to get a comparable goal object go_a in the local system. So in this theory, justice concerns arise when actors try to produce the same relations in their local interactions as they perceive in the world. By contrast with the exchange theory, the balance theory makes the referential structure essential to justice conceptions. Without it, local system comparisons are neither just nor unjust—they are anomic.

According to the balance theory, justice is defined as a balanced local system. Notice that, given the referential structure of figure 2, the relations pictured there for the local system are the only possible ones which allow it to be balanced. If, for example, P′ (P as viewed by himself) had the *a* state of the characteristic and the *b* state of the goal object, the P'_{ca}–go_b–P cycle would be negative (imbalanced), because the *a* state is evaluated positively and the *b* state negatively. Note also that fairness or unfairness depends on relations in the referential structure, not on relations between P′ and O, as it does in the exchange theory. Either P′ or O (or both, or neither) may be unfairly underrewarded, fairly rewarded, or unfairly overrewarded. Moreover, the fairness situation of P′ is independent of whether O's characteristics and goal objects are in a fair relationship.

Like all balance theories, this one predicts that balanced situations are stable and tend to persist; imbalanced situations are tension-producing and tend to change in the direction of balance. In other words, this theory predicts that any type of injustice—which is shown as any type of imbalance in the local system—produces psychological discomfort and attempts by actors to produce justice or balance.

AN EXPERIMENTAL TEST

The foregoing selective review of literature suggests answers to the questions we began with. Perceived unjust allocations by a leader should be one important cause of revolutionary alliances against him. Moreover, members of a revolutionary alliance are expected to try to redress the inequity, assuming they have some "input characteristic" or "referential structure" basis for knowing the leader has been unfair. But exactly how alliance members will try to alter the leader's distribution depends, in some situations, on which theory of justice better describes the origin of ideas of fairness. To evaluate these ideas, we designed an experiment in which a triad worked together, with the leader able to distribute group earnings and the two subordinates having the option of forming an alliance to overrule his decisions.[11]

[11] Although begun independently, our work was informed later by reading Lawler and Thompson (1975), whose experiment has some similarities to ours.

In outline, the experiment was designed to create a 4-3-3 triad, with person A (with a weight of 4) given the right to distribute group earnings for working on problems. Sometimes A distributed winnings fairly (that is, 40% to himself and 30% each to persons B and C) and sometimes unfairly (more than 40% to himself). On each distribution, B and C had the right to form an alliance and take away as much of A's award to himself as they wished. Exactly what happened to any money taken from A depended on which condition of the experiment subjects were in. In the redistribution condition, they could divide the money between themselves in any way they agreed upon. In the blockage condition, money taken from A went back to the experimenters and B's and C's awards remained as A had decided.[12]

The justice theories described above lead directly to three predictions. First, we expect that subjects will be more likely to form revolutionary coalitions when A distributes awards unfairly (when B and C are unfairly underrewarded) than when A distributes fairly.

Second, both theories predict that subjects will, when faced with an unfair situation, take steps to restore equity. That is, given the chance, subjects who form a revolutionary coalition against A will try to restore a 40-30-30 division of awards. Note that they can do this only in the redistribution condition of this experiment. However, both theories of justice predict that individuals in this situation will not act completely selfishly by taking everything away from A.

Third, comparing the blockage and redistribution conditions permits some differentiating predictions to be drawn from the two justice theories. Imagine a trial in the blockage condition on which the team has been awarded $1.00 and A has divided the team winnings 70% (or 70¢) to himself and 15% (15¢) each to B and C. Since that is unfair—A has taken almost twice as much as he should and has given B and C only half as much as they "deserve"—we predict a revolutionary coalition is quite likely. However, if the coalition forms, B and C can only take away winnings from A; they will be limited to what he has given them in any case. The exchange prediction is that, when B compares himself with A, B will decide it is fair for A to get only half of what he deserves, just as B does. The following equations show this.

In a 40-30-30 triad, using weights as inputs and winnings as outcomes,

[12] Redistribution is probably the more common result of revolutionary alliances, such as those resulting in land reform or the forcible redistribution of money in a robbery However, blockage tactics are important in such cases as reporting a cheater on an exam or beginning a slowdown as a labor negotiating strategy. In blockage cases, coalition members are able to reduce the perceived unfair rewards to another individual, but without direct benefit to themselves. Philosophically, this is a retributive view of justice. Emerson (1964) discusses such coalitions, and Lawler (1975) has used this variable in a coalition experiment.

the following relations would be fair:

$$\frac{30¢_B}{30_B} = \frac{40¢_A}{40_A}.$$

But as the blockage condition of the experiment is defined, B can keep only the award A has decided upon, in this case, 15¢. Thus the exchange theory prediction is that fairness will require a proportional lowering of A's winnings to 20¢, to maintain equality of the ratios:

$$\frac{15¢_B}{30_B} = \frac{20¢_A}{40_A}.$$

The balance theory makes a different prediction. The local system imbalance of our example, as it looks to person B, is shown in figure 3. Notice that both cycles, the one involving P′ (person B) as well as the one involving O (person A), are negative and hence imbalanced. (Evaluations are relative in balance diagrams; thus when the subscript of a goal object is greater than the subscript of the characteristic, as they are for person A in figure 3, the sign of the P–go evaluation is + and the sign of the P–c evaluation is —.) The ideal solution for B would be to change both actors' winnings so that both local cycles are positive, but he cannot do so in the blockage condition. All he can do is to change the cycle involving A by altering A's awards. If he lowered A's award to 20¢ here, that would again be imbalanced: A then would be unfairly underrewarded by comparison with the referential structure. This is shown in figure 4*a*. The balance prediction is that B will prefer one imbalanced cycle in the local system to two, and thus that he will restore fairness (40¢) to A's relations. This is
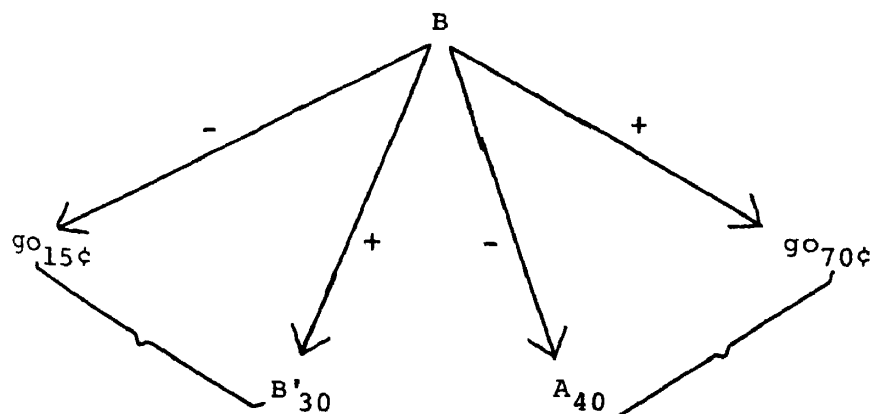
Fig. 3.—Distributive injustice: A overrewarded; B underrewarded
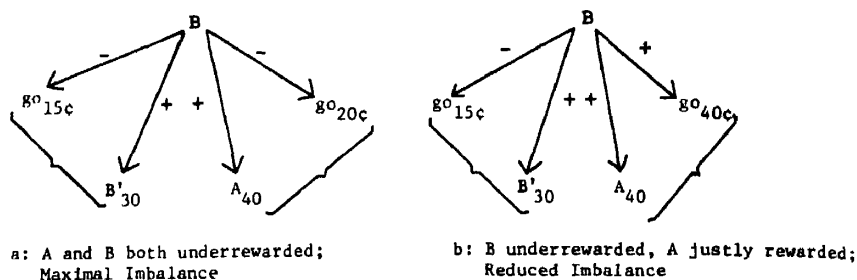
FIG. 4.—Possible resolutions

shown in figure 4*b* as a local system with only one negative cycle (the one containing B, B′, and $go_{15¢}$).

So for our experiment, the exchange and balance theories make quite different predictions about the process of restoring justice. The exchange theory predicts that when an individual is stuck with an unfair underreward, he will reduce the leader's outcome proportionally to his own; in other words, B will allocate less to A in the blockage condition than in the redistribution condition. The balance theory predicts that allocations to A will be the same in both conditions, since the same referential structure exists in both cases.

Subjects for the experiment were white male college students, volunteers recruited from introductory sociology classes. Because naturally occurring status characteristic differences (such as age, sex, race) could be used by individuals as a basis for allocating rewards, a uniform subject population is desirable here. Subjects were scheduled by telephone for "a study of social interaction" and randomly assigned to either the redistribution or the blockage condition. As soon as they arrived for their appointments, they were taken to individual rooms, each containing a card saying "person B," and were asked to await instructions through a closed-circuit TV monitor. Thus every subject in the experiment was in position B. All succeeding interaction was through the TV and other communication equipment. The experimenters, as well as "person A" of the group, were on videotape.[13]

In phase 1, subjects were told they were members of a triad composed of male students at the university and that the experimenters were interested in measuring their "contrast sensitivity."[14] They also were shown a

[13] Laboratory and technical assistance were provided by James E. Driskell, Donia Gardner, Homer Steedley, and James Edward Bradshaw. The experiments were financed by grants from Graduate Dean George M. Reeves and Provost Keith E Davis of the University of South Carolina. We thank all for this help.

[14] The contrast sensitivity problems were developed for experiments on performance ability (e g., Webster, Roberts, and Sobieszek 1972). The task is to judge whether black or white covers more of the area of a large pattern containing 100 smaller rectangles. A second set of slides contains two patterns, and the task is to tell which

table entitled "National Standards," purporting to tell how to interpret their scores by comparison with a national sample of college students. Next they judged 20 contrast sensitivity slides and were told how well each person in the group had performed at them. Person A got 16 correct out of 20, which was interpreted as a "superior" performance. The subject, person B, got 12 right, an "average" performance. Person C also got 12 correct. That was the end of phase 1. In phase 2, each subject was told he would be working with person A and person C as a team, judging a second set of contrast sensitivity slides for money. This time, the team would receive payment if any two of them got a slide correct. Phase 2 consisted of 50 slides, and after each group of five slides the experimenter would interrupt to tell the group how much they had earned. Correct team answers were worth 30¢ apiece, so team earnings after each block of five slides could range from 0¢ to $1.50.

Before beginning the slides, the experimenter announced that person A would be the "team leader" and would have the right to allocate team earnings among members. Choice of person A to be leader was justified on the basis of his higher contrast sensitivity score in phase 1. Phase 1 scores for all three members were translated into "percent of team's contribution," which was said to be an estimate of how helpful each of them was likely to be in judging the slides. Person A's contribution was shown to be 40%; person B's, 30%; and person C's, 30%. This distribution of status weights makes a 4-3-3 triad, one which is particularly susceptible to revolutionary coalitions.

Next, norms of fairness were emphasized. Gamson (1961), Anderson (1967), and Weil (1970) have established that individuals are likely to decide a 40-30-30 division is fair in a 4-3-3 triad. Building upon this, both of the theories we are considering predict that individuals will try to achieve "fairness" in their own group. Thus our experiment does not replicate the Anderson (1967) or Weil (1970) tests of *what* distribution will be considered fair. Rather, it tests the prediction that individuals in a cooperative triad will attempt to achieve fairness through alliance when the leader allocates unfairly. The experimenter addressed person A (with the actual subject, person B, looking on) and told him that "most people" thought it fair to divide team earnings according to team members' relative contributions: 40% to A and 30% each to B and C. However, the experimenter emphasized, person A could follow that distribution rule or not, as he wished.[15]

contains more white area. Though impossibly difficult, the tasks are involving, and scores assigned are believable. In this experiment, the one-pattern slides were used for phase 1 and the two-pattern slides for phase 2.

[15] Relevant portions of the instructions are as follows. To define fairness: "This means that most people in situations like this believe it is fair for a person with a score of 40% to receive 40% of the winnings and for each person with a score of 30% to receive

Then the procedure for overruling A's distribution was explained to person B and the hypothetical person C. Each time A announced how he wanted to distribute the award, B and C would write down his division and indicate whether they wanted to accept it. If they did, then A's decision governed award distribution for that trial. If not, they also wrote down the award division they wanted. A successful revolutionary coalition would be formed only if both members wanted to form an alliance on that trial and if they agreed on how to distribute awards.[16]

Key features of the instructions were repeated, then the 10 trials were conducted. Following them, each subject was interviewed separately to be sure he understood important parts of the experiment (the manipulations of independent variables), that he believed his contrast sensitivity score, and that he believed the other members of his triad were real. Following this, the experiment was explained to him, all deception was corrected and the reasons for it were explained, any questions he had were answered, and he was thanked and paid for his time.[17]

A total of 56 subjects participated in this experiment. Data from 19 of them, 34%, were excluded before analysis, leaving $N = 20$ in redistribution and $N = 17$ in blockage. Reasons for the exclusions will be discussed after presentation of the data, since they suggest modifications of the experimental design and some ideas for further research. Here we simply note that

30% of the winnings. However, as I said, you all are members of a group, and members of a group usually make their own decisions about things like this." To establish free choice in allocation· "But person A is free to divide the reward in any way he chooses. He does not have to follow this rule. Neither do persons B and C have to follow this rule . . . Persons B and C are free to propose any redistribution they wish " In addition to these instructions, subjects had the experience of watching person A allocate rewards fairly 50% of the time and unfairly overreward himself 50% of the time, which may have emphasized freedom of choice about whether to conform to the norms.

[16] Subjects checked either of two statements: "I accept person A's division," or (in redistribution) "I do not accept person A's division, and I propose an alliance dividing the award as follows: ——% to person A, ——% to person B, and ——% to person C." (In blockage the second sentence was: "I do not accept person A's division, and I propose an alliance to reduce A's share to ——%.") Requiring agreement on award distribution is a feature of all coalition studies. For comparability, and also to serve as a check on "individualized" behavior, it was included in the design. Strictly speaking, however, it is not essential to a test of the justice theories. This point is discussed below.

[17] Two features of this design deserve mention. First, unlike most previous coalition experiments, this one has subjects making allocation and alliance decisions repeatedly This procedure yields many more data per group and permits some assessments of time and experience dependencies in justice situations. Second, the unfairness was produced by another subject, person A, not by the experimenter. This is especially important because when the experimenter produces injustice it probably activates a number of different responses in different subjects: redefinition of their abilities, retaliation against the experimenter or the other subject or against the inferred experimental hypotheses, withrawal from the situation, fatalistic resignation, etc. (see Cook 1975).

this rate of exclusion is within the range often seen in experimental studies, especially ones using an entirely new research design, though it is a higher rate than we would like. Generally, subjects were excluded on two grounds: (1) gross failure to understand important features of the instructions; or (2) failure to understand the use of status weights in defining "fair allocation of awards," including belief in special circumstances which countermand the distribution rule for their particular group.

For every group of five slides, the experimenter announced that the group had earned between 90¢ and $1.50 and then asked person A to decide how to allocate that money. Table 1 shows person A's allocations for each of the 10 trials. The allocation was fair on trials 1, 2, 5, 7, and 9 and unfair on trials 3, 4, 6, 8, and 10.

The first prediction is that coalition offers will be more frequent on unfair trials than on fair trials. Table 2 shows the proportion of subjects offering coalitions on each trial. These data strongly support the prediction. Over all trials and subjects six coalitions were offered on fair trials and 92 on unfair trials in redistribution ($t = 16.46$, $P < .01$). In blockage, 16 coalitions were offered on fair trials and 70 on unfair trials ($t = 13.13$, $P < .01$).

The second prediction is that subjects will, given the chance, attempt to produce a fair distribution of awards to all members. The test assumes that individuals will use the fairness norms earlier described: 40%-30%-30%. Table 3 shows subjects' actual distribution of awards to A for both condi-

TABLE 1

A'S PROPOSED DIVISION FOR 10 SETS OF SLIDES (%)

	Trials									
	1	2	3	4	5	6	7	8	9	10
Person A	40	40	80	60	40	50	40	60	40	100
Person B	30	30	10	20	30	25	30	20	30	0
Person C	30	30	10	20	30	25	30	20	30	0

NOTE — Amount allocated by A was $1 50 for all trials except 4 ($1 20), 6 ($.90), and 10 ($1.20).

TABLE 2

PERCENTAGE OF SUBJECTS OFFERING COALITIONS PER TRIAL

	Trials									
	1	2	3	4	5	6	7	8	9	10
Redistribution ($N = 20$)	5 0	5 0	100 0	95.0	5 0	70.0	5 0	100 0	10.0	95.0
Blockage ($N = 17$)	0	11.8	88.2	88 2	11.8	58 5	11 8	88.2	23.5	88.2
$\bar{X}$	2.7	8.1	94.5	91 8	8 1	64.8	8.1	94.5	16.2	91.8

TABLE 3

PROPOSED PERCENTAGE ALLOCATION TO PERSON A

	Trials									
	1	2	3	4	5	6	7	8	9	10
Redistribution:										
$\bar{X}$	39.0	39.0	37.2	39.2	39.0	39.3	39.0	35.7	38.0	27.0
σ	4.5	4.5	9.1	9.0	4.5	11.4	4.5	8.2	6.2	18.7
SE	1.0	1.0	2.0	2.0	1.0	2.6	1.0	1.8	1.4	4.2
Blockage:										
$\bar{X}$	40.0	39.0	41.2	38.8	37.2	40.3	38.4	37.6	36.6	28.9
σ	0	2.9	17.0	14.1	7.6	10.7	5.0	13.5	7.7	33.0
SE	0	0.7	4.1	3.4	1.8	2.6	1.2	3.3	1.9	8.0

NOTE.—SE is standard error of the mean.

tions. (No subject proposed anything other than an equal division between B and C; thus showing in table 3 the mean proposed allocations to B and C would not add to the information presented. The equal B-C split is predicted by both justice theories, since B and C have equal status weights.)

In redistribution, where subjects have the most control over all awards, the data provide good support for the prediction that they are trying to achieve a 40-30-30 distribution on all trials ($\bar{X}$ to A for all trials = 37.2%). Except for trial 10, which we discuss in more detail below, mean allocations to A are quite close to the predicted 40% in every case. For fair trials, these data are produced most often by subjects' accepting A's proposed division; for unfair trials, data are the result of subjects' choosing to change A's allocation to 40%. It seems reasonable to conclude that these data give good support to the second prediction, that individuals will try to change allocations in the direction of fairness when they are given the chance.

Blockage data show much the same mean allocations to A as those from redistribution ($\bar{X}$ to A = 37.8% for all trials), although for most trials (excepting 1, 2, and 6) the variability of response across subjects is considerably greater. But the similarity of allocation is important, for, as discussed earlier, the blockage condition permits comparison of contrasting predictions from the exchange and the balance theories of justice.

The balance theory prediction is that allocations to A should be the same in blockage as in redistribution—40% in all cases. The exchange prediction is that awards to each subject should be proportional to their status weights, but since the awards to B and C are fixed here—they cannot be changed from what A has awarded—in order to maintain equality of the proportions, the award to A (the numerator of his ratio) must be decreased on unfair trials. That would mean giving A an award determined by his status weight in proportion to the ratio of B's status weight to his award. Given the awards to person B shown in table 1, proportional awards to person A

on unfair trials would be as follows: trial 3, 13.3%; trial 4, 26.6%; trial 6, 33.3%; trial 8, 26.6%; trial 10, 0.

Inspection of mean data in table 3 shows a much closer correspondence between the blockage data and the balance prediction (40%) than between those data and the exchange predictions (above). However, for two reasons further analyses are desirable here. First, the number of subjects and the amount of variance in the data mean that the exchange prediction *on each trial* is within a 90% confidence interval for the blockage data. Given that we are comparing two theories, not assuming "no information" about what the data will look like (see Webster, Roberts, and Sobieszek 1974), and given that the data are much closer to the balance prediction for all trials, we do not regard this as the crucial reason compelling further analyses.

More important, it is conceivable that the actual numbers calculated from the exchange theory do not represent accurately the subjective values of outcomes individuals would consider fair. An argument could be made that when person A gave B and C each 10% on trial 3 instead of the 30% which would be fair, they decided he deserved less than his (otherwise) fair level of 40%, but not necessarily only 13.3%. In other words, subjective utilities of the awards may not be scaled like the number system representing money.[18] This issue may have been part of the concern expressed by Berger et al. that exchange theories "fail to give meaningful and precise definitions of either justice or injustice" (1972, p. 144).

To control the problem of subjective values, we formulate and test three differentiating predictions which do not make any assumptions about subjective scaling. The main assumption we still make is ordinal: If actors are forced to receive an award less than 30%, they will define "fairness" for person A as something less than 40%. That assumption appears essential to any exchange theory of justice comparisons. Thus here we are comparing three "weaker versions" of the exchange theory of distributive justice with the balance theory. The statistic for these comparisons is Hotelling's exact T^2 (Kirk 1968, p. 262), which compares curves drawn from data in table 3 with predictions of each theory (calculations in Timm 1975, pp. 244–50).

The first test is for differences in allocation to A between conditions of the experiment. The balance prediction is that there will be no difference; both redistribution and blockage are predicted to show the same awards to person A. The exchange prediction is that awards to A will be lower in blockage than in redistribution. Results do not support the exchange prediction; there is no significant difference between groups in allocations to A:

[18] For identifying this issue, and especially for consultation about data analyses that follow, we are greatly indebted to Jerry Busemeyer, of the USC Social and Behavioral Sciences Laboratory. For other advice and help with the data analysis, we thank Katharine Heald and two anonymous *AJS* readers.

$T^2(1,35) = .03$; for significance at $P = .10$, $T^2 = 2.855$; for $P = .05$, $T^2 = 4.12$.

The second test focuses on the shapes of curves predicted for the blockage condition by the two theories.[19] The balance theory predicts a flat curve, with person A being awarded the same amount on each trial. The exchange theory predicts a curvilinear relation, with A receiving somewhat less than he awarded himself on trial 6 and much less on trials 3 and 10. These two predictions are shown as graphs in figures 5*a* and 5*b*, respectively. The result is again consistent with the balance prediction: No significant differences in awards to A appear across trials in the blockage condition: $T^2(9,35) = 13.13$; for $P = .10$, $T^2 = 21.87$; for $P = .05$, $T^2 = 26.25$.

The third test is for a trials $\times$ condition interaction. The balance prediction is that awards to A will be constant across trials for both redistribution and blockage. The exchange prediction is that awards to A will be flat for redistribution but curvilinear for blockage (as described above). These predictions are shown in figures 6*a* and 6*b*, respectively. This result also accords with the balance prediction: No significant difference exists between the shape of redistribution and blockage curves: $T^2(9,35) = 5.57$; for $P = .10$, $T^2 = 21.87$; for $P = .05$, $T^2 = 26.25$.

All three tests give better support to predictions of the balance theory than to those of the exchange theory. Because balance theory predictions are identified with the null hypothesis in all three, it is strictly correct to say only that these tests do not enable rejection of the balance predictions.
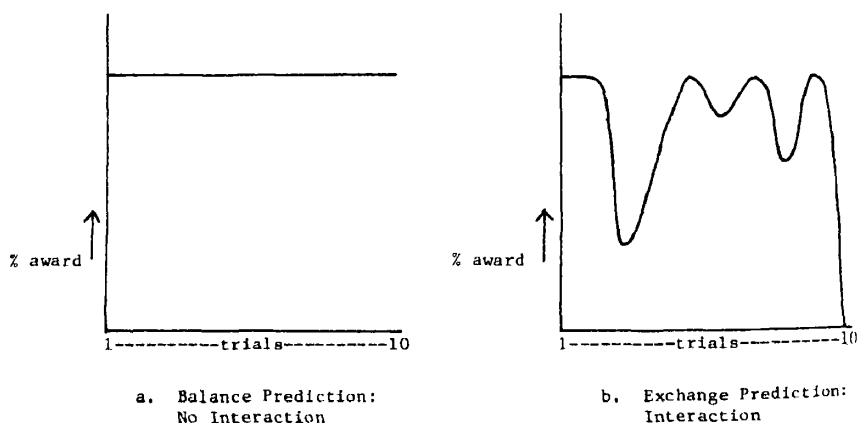
FIG. 5.—Balance and exchange predictions, shape of award curves, blockage condition.

[19] This test and the following are calculated using pooled variances from both conditions. This is appropriate, given the lack of difference demonstrated by the test immediately preceding. However, calculation using only variance for blockage does not alter the results: $T^2 = 13.39$ for this test, and $T^2 = 5.29$ for the following test.
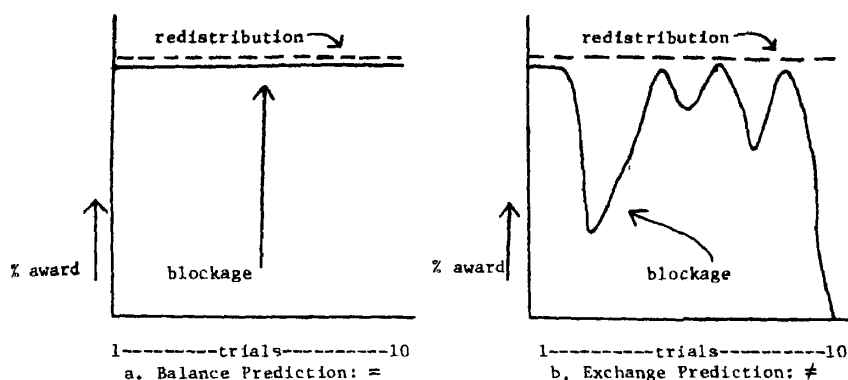
FIG. 6.—Balance and exchange predictions, shapes of award curves, both conditions

Somewhat less cautiously, it may be noted that calculated values of T^2 are far from those needed to establish confidence in the exchange predictions for these data. In addition, as noted above, these tests apply to weak versions of the exchange theory predictions, certainly more difficult to reject than the exact ratios of Adams (1965), presented above. Overall, it seems fair to conclude that the data provide better support for the balance theory of justice than for the exchange theory.

DISCUSSION AND FURTHER WORK

The most general result of this work has been to show that justice concerns play an important role in coalition formation among initially cooperative actors. Failure of a leader to follow norms of justice in allocating group awards was sufficient to produce revolutionary responses against him in the majority of instances.

A second general result is that theories of justice were able to predict which allocation of awards would be considered fair and to describe allocation choices of partners to the coalition. In this regard, it is particularly significant that coalition partners did not engage in completely punitive or selfish allocation. That is, in most cases they tried to change A's allocation toward his fair share, defined as 40%, not toward zero. And they did not, in most cases, try to take 100% of the group awards for themselves even when they could have done so in the redistribution condition.

Before examining some additional implications of this work and suggesting further research, it may be useful to examine certain unanticipated results. Some caused exclusion of subjects, and others point to possible refinements of theories of distributive justice.

Subjects were excluded from analysis for three specific reasons: (1) failure to accept the norms of fair allocation; (2) failure to understand a

crucial independent variable; and (3) deliberate disregarding of important features of the instructions. Of the 19 subjects excluded: (1) three in blockage and six in redistribution did not think the referential norms applied to their group. These cases may indicate weakness of experimental design and the operation of a competing "equality norm." (2) Eight in blockage mistakenly thought they would get the money taken from A. This misunderstanding may indicate an important cognitive or behavioral process. (3) One in blockage tried to *increase* A's allocation to self, and one in redistribution knew our taped accomplice person A, did not like him, and said he tried to cut his rewards for that reason. These two cases we regard simply as random error, but the first two groups deserve more attention.

(1) Exclusions for failure to adopt the fairness norms seem to have been caused most of the time by the example we used to establish them. We said that if one person painted half a house and another painted a quarter of it, it was fair that the first person should be paid more than the second. Subjects who didn't accept this pointed out that painters are generally paid by the hour, and since all of them put equal effort and time into the experiment, they should all receive equal rewards. While we think a better choice of example would help reduce these exclusions, we also note that an equality norm is quite likely to show up with any student subject population. It has been remarked upon in other contexts with adults and college student subjects (Leventhal 1976; Cook 1975), high school students (Bobrow 1972), and grade school children (Webster and Entwisle 1976).[20] The equality norm may well be an important phenomenon, deserving of research in its own right, but it is not the same as a concern with distributive justice.

(2) The eight individuals excluded from blockage for thinking they were going to get the money they took away from person A may show a phenomenon of greater theoretical importance. In effect, these subjects thought they were in the redistribution condition of the experiment, despite the fact that instructions explained clearly, four times, that they would not get any money taken from A. Do people in our culture believe the world is a zero-sum game? Here we simply note this possibility. Other experiments, accompanied by intensive interviewing, would be required to determine

[20] The "equality norm" says that everyone should receive equal goal objects (status, pay, and school grades, in the papers cited) and that no characteristic of individuals is relevant to differentiation. Leventhal (1976) shows that the equality norm becomes more salient for wages when they are public and that pay is more often based upon merit when wages are secret Webster and Entwisle (1976) present evidence that the equality norm appears more strongly when individuals are not greatly concerned with good performance. Greenberg (1978) sees adherence to the equality norm as inversely related to the Protestant ethic, a personality variable.

whether such a phenomenon exists and, if so, what conditions are sufficient to activate it.

Allocation data for excluded subjects, comparable to data in table 3, are shown in table 4. The numbers are not large enough for statistical tests to be meaningful, but on inspection these data do not appear to differ importantly from those concerning subjects in the main analyses shown in table 3. Subjects espousing an equality ethic (the first group) may show some tendency to try to approach equality (33%) after trial 2. As noted, we conclude that these individuals are probably responding to a different set of norms than those of justice. If they have rejected status weights as a legitimate differentiating characteristic of individuals, both theories predict they will seek an equal award (33.3%) for each actor and the data do not help in comparing the theories.

An unanticipated outcome which may help in refining the theory of distributive justice has to do with ways subjects responded to unfairness on two trials. During interviews it became apparent that a minority of subjects perceived trials 6 and 10 as quite different from the other three unfair trials; this difference is shown behaviorally in table 3 as higher variance than for most other trials. As shown in table 1, trial 6 was marked by the least unfairness (a 50-25-25 distribution) and trial 10 by the most (100-0-0). Trial 6 was also the trial on which the team was told it had earned the least money (90¢), and several subjects said they thought it might be their own fault the team earned so little. Trial 10 was maximally unfair, and some subjects (six in redistribution, seven in blockage) wanted to reduce A's share to zero. That is what produced the unusually small mean allocation to A and the unusually high variance on trial 10, shown in table 3. Some ideas on why this occurred may be considered.

One hypothesis is that by the time trial 10 appeared, the interaction had developed a history, with person A having shown himself to be fairly untrustworthy. However, that idea is not supported by the data, for it would require that the test for change across trials, described above, show a sig-

TABLE 4

PROPOSED PERCENTAGE ALLOCATION TO A, EXCLUDED SUBJECTS

	Trials									
	1	2	3	4	5	6	7	8	9	10
Failure of referential structure (N=9)	37 0	36.3	31 0	33 9	34 9	33 8	34 9	32 8	34 9	30.6
Blockage/redistribution confusion (N=8)	37.0	40 0	38.1	43 3	40 0	39 5	40 0	42.9	40.0	43.3

nificant effect (a steady increase in proportion offering coalitions, and a decrease in proposed awards to A), and no such changes were observed.[21]

The same tests also do not support a second hypothesis, namely, that subjects were attempting to produce justice overall, not just on each individual trial. Emerson (1964) found evidence of subjects' doing this sort of thing, attempting to produce justice over a long series of trials. However, in our experiment, in contrast to those of Emerson, subjects were not informed of success at coalition attempts, nor did they keep a cumulative tally of winnings of any member. While cumulative attempts to produce justice are probably important in many enduring social interactions (such as reciprocal dinner invitations), the evidence is that subjects did not pursue such a goal here.

A third possibility seems most likely: Trial 10 angered some subjects. Some said they thought that on earlier unfair trials, person A was being a little bit greedy, but that his proposed distribution for trial 10 overstepped acceptable limits. So trial 10 may have been unique. Perhaps very great injustice is perceived differently from ordinary injustice, and processes other than distributive justice may be aroused in these extreme cases. Some naturally occurring situations of unfairness also are perceived as extreme by participants, so it is important to recognize the possibility that these may currently be outside the scope of theories of distributive justice. Extreme cases may provoke a different type of injustice response, or they may add anger and retribution to justice responses. These issues, along with the need for a precise definition of what constitutes an "extreme case," seem worthy of further theoretical clarification.[22]

The broader implications of our work revolve around the balance theory of distributive justice and the fact that, so far as we know, this research is the first direct test and confirmation of that theory. While most results show better support for the balance than for the exchange theory, we do not regard the latter as invalidated. This is only a single test, and it is conceivable that another experiment would produce stronger evidence for the exchange theory. It is possible, if not very plausible, that subjects redefined their inputs (or A's) on each unfair trial of this experiment in such

[21] Given that the blockage and redistribution conditions do not differ significantly, it is not customary to perform separate analyses upon conditions. However the following may be instructive. For blockage, ANOVA shows no effect of trials, $F(9,160) = 988$, N.S. Redistribution shows an effect of trials for all trials, $F(9,190) = 3.44$, $P < .025$, but the effect is not apparent if one analyses only trials 1–9, $F(8,171) = .566$, N.S. This suggests that where time dependence is apparent, it is produced solely by the final trial. Thus any "cumulative outrage" interpretation is not supported.

[22] Extreme inequity was used in one condition of the Leventhal and Bergman (1969) experiment, but the range of possible subject responses (±5¢) was too restricted to observe any extreme response to it. Cook (1975) argues that underpayment from the experimenter can lower subjects' beliefs about their abilities, and this appears to have happened in our trial 6.

a way that the observed allocations to A were considered fair. This process, for which we have no experimental evidence either way, would support the exchange theory as well as the balance theory. Some of our results (those in table 2) support both theories equally well, and for such cases an investigator might prefer to use the simpler and repeatedly verified equations of Adams.

What we wish to emphasize is the essential nature of the referential structure of norms defining justice. In cases where our data give better support to the balance theory than the exchange theory, it is precisely because the latter focuses on two-person exchange to the exclusion of any larger social comparison processes. From our perspective (as well as that of Berger et al. [1972]), this is unreasonable, To know what constitutes a fair level of goal objects it is essential to have some standard for comparison beyond the immediate interaction. Even Adams's exchange-based experiments compared subjects to a generalized other, "most other workers," in defining their ratio of outcomes to inputs.

Recent findings of Jasso and Rossi (1977) give strong support to the idea that a referential structure concerning wages does in fact exist in our culture. These investigators explicitly compared the exchange and balance theories of justice in a survey of adult residents of Baltimore and found clear evidence for a superindividual structure of ideas about fair earnings for individuals with different sorts of characteristics. In language more evocative than ours, they describe a similar conclusion: "We suggest that if referential standards [were] idiosyncratic, then an empirical theory of distributive justice [would be] but a drollery" (1977, p. 640).

In natural settings, the balance theory would lead us to expect that revolutionary coalitions are likely to form in any case of perceived injustice. More interesting, we begin to be able to specify when a successful coalition will try to produce a fair redivision of goal objects. The referential structure is essential for any justice process to occur; without it, comparisons are anomic and there is no way to tell who deserves how much of which goal objects. If revolutionaries do not see any personal characteristics of individuals as relevant to the allocation of goal objects, the effect of revolution is more likely to be the breakdown of order than restoration of justice. In anomic cases, an equality norm, a war of all against all, or even some other response might emerge. But in cases where there is a referential structure, we can predict the somewhat surprising result that even those actors who are unalterably underrewarded will prefer a fair allocation for others.

An immediate next step in this line of research might be to repeat our experiment without explicitly establishing a referential structure for subjects. Here, we assumed that previous research had adequately demon-

strated that subjects use status weights to decide how much reward is fair, and we concentrated on using this idea to test predictions that they would choose to act fairly. So long as status weights differentiate actors, the exchange theory predicts they will be used as the basis for determining fair award distributions. Thus its predictions are the same as those tested here: 40-30-30 in redistribution and a proportional division in blockage. The balance theory relies on an existing referential structure, so whether actors try to produce a 40-30-30 division (or even whether they try to form a revolutionary coalition) would depend on whether they believed "most people" used status weights to determine fair divisions. If they did infer a balanced, unitary referential structure, the balance theory again makes the predictions tested here. But if they did not—for example, if they knew that many previous individuals in this situation had not used status weights—justice predictions would not be possible, and subjects might follow equality norms, complete selfishness, or some other pattern.

It might be argued that in our experiment subjects were not so much trying to produce justice in reallocations as they were trying to get a reciprocated alliance with person C and assuming that person C wanted justice. Such an idea could be tested in our experimental design by removing the requirement that reallocation alliances be reciprocated in the way they are in coalition studies.

Finally, it would be worthwhile to explore consequences of unjust overreward in this situation. Overreward provides some of the most compelling support for justice theories in wage situations (such as Adams 1965), and it would be relatively easy to produce overreward in our experimental situation. However we might not expect revolutionary responses in such cases, we might get deprecation of A or improved self-expectations among overrewarded subjects. Longer-term projects are needed to study the hostility and outrage-arousing aspects of extreme injustice and to study other patterns of response besides revolutionary coalitions.

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Group Formation and the Cultural Division of Labor[1]

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A structural theory of the relationship between class and status group formation is presented. The approach postulates, first, that differences in the solidarity of any objectively defined groups are independently determined by the extent of stratification among these groups and interaction within them. These expectations are confirmed by an analysis of variation in the solidarity of 17 American ethnic groups in 1970. Second, the relative importance of class as against status group divisions in societies as a whole is held to depend upon the degree of hierarchy and segmentation of their respective cultural divisions of labor. Supportive evidence is found in the examination of differences in the strength of class voting among five Australian states in 1964.

Perhaps the last important contribution to the theory of social stratification in industrial societies was made long ago by Max Weber. In the famous essay "Class, Status, and Party" Weber did a great deal more than merely revise existing Marxian categories of stratification. Instead he proposed, not for the first time but in the clearest terms, a radically different type of category for the analysis of stratification: the *Stand* or status group.

The validity of this category does not depend upon the conceptual adequacy of what Weber refers to as "class" in his essay. Since it has often been pointed out that his use of this term is quite different from Marx's own (Giddens 1973), the matter need not be pursued here. The significance of the concept *Stand* is that it countenances a basis for group formation—and consequent stratification—that is analytically independent of the relations of production. This is a possibility that Marxian theory had seemed to preclude. True, the distinction between *Klasse an sich* and *Klasse für sich* gives the Marxian concept of class some analytic flexibility. But it is essential to recognize that the class principle bonds individuals into groups solely on account of their common position within the existing relations of production. By contrast, the aggregation principle for a status group is ultimately some kind of cultural commonality (Hechter 1976*b*). A status

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group typically includes individuals of different classes, whereas a class typically includes individuals of different status groups. Class and status thus provide separate bases of group formation in complex societies.

Each principle of group formation tends to be associated with a characteristic form of political mobilization. A society divided into two hostile classes is presumably ripe for revolution, but a society divided into two hostile status groups—nations, for example—is threatened by secession. The consequences of class mobilization are therefore quite different from those of status mobilization. In order to understand the social structural basis for diverse phenomena such as revolution and nationalism, class and status must be recognized to be the Castor and Pollux of any system of stratification.

What determines the strength of these different principles of group formation in industrial societies? This is a question that has seldom been raised—and hardly investigated empirically. Most writers have tended to look at the world through either class or status lenses. Those interested in class have assumed that status factors act to inhibit class formation; while those interested in status have assumed the inverse. This paper argues that the analysis of stratification in industrial societies is better served by discarding these unidimensional concepts and turning instead to a concept that incorporates each dimension into a new whole: the cultural division of labor.

Marx, the preeminent class theorist, recognized that status groups such as the medieval guilds and estates existed in feudal Europe. However, he felt that as industrial capitalism developed group formation would increasingly be based upon class. Most 19th-century sociologists agreed with him. Marx's expectation that the class structure of capitalist societies would be divided into two hostile camps has not been fulfilled. It is certainly possible to divide the population of a society like the United States into two categories, workers and employers (Wright and Perrone 1977), but this kind of dichotomy probably obscures more than it illuminates. The problem cannot be solved merely by elaborating the class structure to include more than two strata. Indeed, the most serious dilemma for the class theorists is that there is not much evidence that these categories are etched deeply into the consciousness of individuals. Hence these are classes in name alone rather than solidary social groups: *Klasse an sich* has not been generally transformed into *Klasse für sich*. Recent followers of Marx have hardly been oblivious to this fact, and some have tried to specify the obstacles to the formation of class consciousness (Ollman 1972).

But the problem goes somewhat deeper than this. It is all very well to argue that class consciousness is a fragile creature that must be carefully sheltered from cold drafts and pathogens as if it were a newborn infant. But what can the class approach make of the abundant evidence that group formation in mature capitalism persists on another basis entirely, that of cultural similarity? Since the class theorist is primarily attuned to hier-

archical distinctions in the division of labor, his instinct is to somehow reduce a status group phenomenon like ethnicity to one of class. Ethnicity, in this view, must be a disguised and denatured form of class consciousness (Leon 1970). This conception can be meaningful only if all the members of an ethnic group occupy the same class position. But this happens rarely: ethnic groups are typically composed of members of different classes. Why should there be more commonality between the bourgeoisie and proletariat of one group than of another? Polanyi has posed it well: "There is no magic in class interests that would secure to members of one class the support of members of other classes. Yet such support is an everyday occurrence" (1957, p. 153). The basic theoretical problem at once becomes evident: How can the variable significance of class in social forms be accounted for by class itself?

It should be clear that for the same reason analyses couched entirely in terms of status cannot be wholly satisfactory. Not only does class intrude upon status (as does status upon class), but there is evidence that the strength of status group cohesion shifts significantly across groups as well as historical eras. Thus status group sentiments such as ethnic identity cannot be usefully conceived to be universal and ahistorical imperatives of social organization in general. Little can be gained by the invocation of primordial sentiments (Shils 1957; Geertz 1963) to account for changes in the salience of ethnicity in industrial societies.

The solution must be found at the contextual—not the individual—level. Several recent contextual approaches to the problem of ethnic divisions emphasize the differential stratification of ethnic groups within the labor market (Bonacich 1972; Edwards, Reich, and Gordon 1975; Cain 1976). Analysis of the labor market in industrial societies reveals the existence of two distinct sectors: a primary sector composed of relatively high-paying jobs with good working conditions and employment stability and a secondary sector composed of low-paying jobs with poorer working conditions and chronic instability of employment (Piore 1975, p. 126). Incumbents in these different sectors of the labor market may be seen to have different objective interests. They may even receive different wages for performing the same tasks (Bonacich 1972). The fact that certain groups (blacks, Hispanics, and some other minorities in the United States) are found predominantly in secondary employment sectors and often receive lower wages than white workers in the same jobs provides a ready explanation for political divisions between black and white workers: the groups taken *as a whole* have different economic interests. As will be argued, there is a substantial element of truth in this position. Yet the approach is evidently limited in other respects. If the solidarity of minority groups in the United States were merely a function of their location within a labor market segment (or a split labor market), those groups sharing the same labor market segment

should be expected (on the basis of this theory) to coalesce in pursuit of their common interests. Now American blacks and Hispanics are disproportionately represented in the secondary labor market, but neither group seems willing to relinquish its separate identity, and efforts to unite them into "rainbow coalitions" have proven notoriously unsuccessful. The same kind of observation might be made of Jews and Episcopalians.

Mindful of some of these issues, writers on "plural societies" hoped to shed light on differential ethnic solidarity by using comparative analysis. However, for two reasons their approach did not lead to the discovery of principles of stratification having general significance for industrial societies. For the most part they studied Third World states and not industrial states at all. But there was another difficulty as well. Explanations in these comparisons and case studies tended to focus on institutional differences as causal factors, but these very differences between the units of comparison vitiated the likelihood that general theories of any kind might emerge.[2]

This paper proposes a way out of the morass caused by the application of each of these concepts to the stratification of culturally heterogeneous societies. The cultural division of labor offers a more complex perspective. A cultural division of labor occurs whenever culturally marked groups are distributed in an occupational structure. However, since the distribution of such groups can take many different forms, cultural divisions of labor vary with respect to their degrees of hierarchy and segmentation. Variations in these two parameters are critical in determining the relative importance of class as against status group formation in all societies. First I shall discuss the determinants of group formation in general; then I shall attempt to explain variation in the strength of status and class cleavages among different social formations.

DETERMINANTS OF GROUP FORMATION IN INDUSTRIAL SOCIETIES: THE CASE OF ETHNIC GROUPS

Before discussing the conditions determining the relative strength of class as against status group formation it is necessary to consider how groups of any kind are formed. There is a remarkable consensus about the answer to this question. The simplest type of group formation—the limiting case, never approached in reality—would occur among an unstratified set of individuals who are evaluated identically for any social purpose. In a homogeneous aggregation of this kind groups, or self-conscious collectivities having boundaries, will tend to form on the basis of existing networks of

[2] The theoretically modest achievements of the pluralist approach have been admitted by one of the school's leading members: "Indeed, [pluralism] is not a theory at all, but simply a set of sensitizing concepts to aid us in studying the complex reality of multiethnic systems and to steer us away from our concern with the 'society-culture' as a closed system" (van den Berghe 1974, p. 870).

interaction.[3] This means there will be a positive relationship between the intensity of interaction among a set of individuals and the degree to which group solidarity develops (for an important statement, see Homans 1950).

Differential rates of group formation can arise to the degree that spatial factors act as barriers to interaction. If an aggregate is spatially dispersed—peasants in a region of isolated farmsteads rather than nucleated settlements, for example—the general level of interaction within it will tend to be low, as will its degree of group solidarity. Second, distance and geographical factors, such as mountain ranges and other natural boundaries, act to make some individuals more peripheral to networks of interaction than others who are more centrally located. This will consequently produce different rates of group formation within an aggregate which is otherwise unstratified.

Once this assumption about the homogeneity of the aggregate is relaxed somewhat to allow for cultural diversity, a further constraint on interaction is produced. This is because cultural differences between individuals generally impose barriers to communication between them. Language is the most obvious of these cultural differences, but it is by no means the only one. The interpretation of meanings across cultural boundaries is invariably problematic. Thus group formation among equals is determined by interaction rates which, in turn, are affected by the spatial organization and cultural diversity of the aggregate.

Group formation within a stratified population, that is, among a set of individuals having differential ownership of or access to resources, is more complex. For the element of stratification per se introduces an entirely different rationale for the formation of groups in addition to one based on interaction processes. When individuals can be objectively categorized as privileged or nonprivileged, group solidarity may follow from this very categorization. Therefore among a set of stratified individuals group formation can also occur reactively: a boundary emerges between sets of privileged and nonprivileged individuals. Further, since interaction across this boundary heightens the perception of stratification, it is more apt to stimulate hostility than mutual accommodation (LeVine and Cambell 1972, p. 29). Hence, although interaction is a critical element in the formation of groups its effects vary in different circumstances. Among equally privileged individuals interaction promotes an inclusive corporate identification, whereas among differentially privileged individuals it spurs conflict and leads to the formation of two or more antagonistic groups.

These conclusions emerge clearly from research on small groups, but there is no reason to doubt that they hold in most macroscopic settings as well,

[3] Note that by this definition groups may exist in the absence of formal organization. For the best discussion of the conditions under which groups of this kind can develop formal organization, see Olson (1968).

In any complex society, then, patterns of group formation should depend on two separate kinds of factors: the degree to which particular aggregates are differentially stratified (with the caveat that this differential stratification must be commonly perceived) and the degree to which interaction within these aggregates is maximized. To the extent that these conditions fail to be met prospects for group solidarity are diminished.

At this level of generality these propositions should apply to any arbitrarily defined sets of individuals. Thus, they should be capable of explaining variation in the cohesiveness of classes, status groups (including ethnic groups), age grades, or even the sexes. The discussion that follows will illustrate how these principles of group formation can be employed to account for variations in the solidarity of objectively defined ethnic groups in American society. Such groups are composed of individuals sharing one or more cultural markers. Whereas any perceptible sign or marker may take on significance for patterns of interaction within societies, the most important cultural markers are language, religion, and skin color.[4]

Ultimately, the determinants of group formation in any society must be found empirically. The answers can presumably be elicited by examining patterns of stratification and interaction in workplaces, neighborhoods, institutions, and voluntary associations of all kinds. Whereas it is theoretically possible to obtain information of this quality at the national level, the expense of generating it is prohibitive.

The approach adopted here offers a simplified means of estimating these patterns for industrial societies as a whole in the absence of complete data. The division of labor is emphasized for two reasons. First, in industrial societies an individual's occupation gives the clearest indication of his overall position in the stratification system. Second, social relations within the workplace will determine much about the course of interaction in other spheres of social life as well. This is only partially due to the fact that many of the waking hours of most people are spent in workplaces of one kind or another. Occupations also directly shape interaction patterns by influencing residential location (through rents, in the first instance [Hawley 1950, p. 282], and through proximity to the workplace, in the second) and by promoting divergent styles of life and social identities (Kohn 1969, pp. 165–88).

The first factor affecting the strength of group solidarity is the position of the group in the stratification system. In general, the lower its position, the greater the probability that its members will come to think of them-

[4] In an ingenious series of small group experiments Tajfel (1970, 1974) succeeded in creating in- and out-group distinctions among subjects on the basis of the slenderest objective differences between them—for example, whether subjects preferred paintings by Klee or Kandinsky. From these experimental results he concluded that social categorizations of all kinds are easily stimulated.

selves as sharing a community of fate.[5] The reason for this is straightforward. When one's life chances are seen to be independent of membership in a particular group, the psychic significance of membership in that group will tend to recede or to disappear altogether. This can be illustrated easily in the case of ethnic groups. The range of life chances available to white Anglo-Saxon Protestant males in the United States is virtually limitless: no significant occupations—or other rewards—are denied to anyone solely on account of his membership in this category. Because of this, individual success or failure cannot easily be attributed to any characteristics of the category as a whole. The life chances of such persons may well be affected by other characteristics—by differences in their class or geographical origins, for instance—but they are not at all affected by this ethnic one. This is a situation encouraging individualistic orientations to action rather than the collectivistic orientations implied by having a strong ethnic identity.

However, the situation is reversed among groups clustered in the lowest reaches of the stratification system. In societies having an egalitarian ideology it is rather difficult for persons in disadvantaged ethnic groups to imagine that poverty has befallen them entirely by chance. To do so would be an admission of collective inferiority. It is far more likely that such individuals will sense a connection between their own position in the stratification system and that of the group as a whole. Perhaps they will come to think their material disadvantage occurs precisely on account of their ethnic distinctiveness. Often they will not have far to look for evidence supporting this perception because discrimination abounds against all such groups. There are two possible reactions to this dilemma. Some may attempt to conceal their ethnic origins, redefining them if necessary. Others, for whom escape is impossible or undesirable, will tend to identify on the basis of their ethnic distinctiveness; many will come to consider themselves members of a corporate group having similar interests. This sense of corporateness is the mark of high group solidarity.

But this does not mean that all ethnic groups achieving high position in the stratification system necessarily have low solidarity. Such groups may have quite different rates of intraethnic interaction. Even among high-ranking groups other distinctions emanating from the occupational structure may serve to encourage or to inhibit the strength of intragroup interaction, and therefore solidarity. This is because there are many different kinds of occupations at each level of the stratification system. Miners and textile workers may both belong to low-ranking occupations, but differences in the

[5] However, this is not likely to be the case in situations where individuals are assigned to occupations solely on an ascriptive basis. In a society like 19th-century India, for example, members of a high-ranking caste are no less likely to be solidary than those of a low-ranking one because membership in each category is equally determinative of individual life chances. Similarly, in medieval western Europe there is no reason to suspect the feudal nobility to be any less solidary than the dependent peasantry.

nature of each task, its ecological setting, and the conditions of labor are all important in determining the degree of solidarity that should emerge in each group (Kerr and Siegel 1954). The prospects for group formation among a population of miners would doubtless be greater than those among a population divided equally between miners and textile workers.

For this reason it is essential to consider the extent to which given groups are clustered in particular occupational niches. This differential clustering will be termed the occupational specialization of a group. Highly specialized ethnic groups have stereotypical occupations. At the extreme, information about an individual's ethnicity would enable a stranger to predict his occupation. In industrial societies, however, occupational specialization is always a matter of degree. Other differences aside, an occupationally specialized ethnic group is more likely to develop solidarity than one that is unspecialized (Barth 1969; Cohen 1969; Hannerz 1974; Yancey, Ericksen, and Juliani 1976). This is principally due to two factors. First, occupations can be an important domain for intragroup interaction. Personal relations within the work setting tend to reinforce the sense of ethnic similarity, as experience is shared through a common cultural idiom. In such circumstances, associations of workers—be they trade unions or professional societies—take on a heavily ethnic flavor. Second, occupations tend to have distinct economic interests. Thus, to the extent that an occupation is monopolized by a particular ethnic group that group will have material interests in common.

The causes of occupational specialization among ethnic groups in industrial societies are largely unexplored (but for one attempt, see Fauman 1968). Strangely, this is less true for preindustrial societies. Comte once noted that in all societies where a moral tradition is the sole preserver of ideas and practices it is inevitable and necessary for fathers to transmit their occupations to their sons (see Bouglé 1971, pp. 36–37). In any complex handicraft industry, nothing is more precious than technical education. Religious minorities (Jews in Christian Europe; Armenians in the Ottoman Empire) sometimes are licensed to engage in occupations that are denied to those of the majority faith. But neither of these considerations has much effect in contemporary industrial societies. It is likely that occupational specializations developed in societies of large-scale immigration because of a complex calculus of factors including the distribution of opportunities at the time of the group's entry, the level of skill of the immigrants, the structure of kinship and social organization in the place of origin, and policies addressed toward particular groups by the state or organized labor (see Glazer 1958, pp. 138–46). Ethnicity itself can cause specialization since the kinds of jobs people get is in part a function of whom they know.[6] But the task of unraveling these elements lies ahead.

[6] A recent study of patterns of job recruitment among a sample of professional, technical, and managerial "workers" finds that a major determinant of getting a job is an

Once an occupation becomes ethnically specialized—particularly a relatively valued occupation—it is likely to be monopolized by the group for some time to come. The control over job information is crucial for the maintenance of occupational specialization. And once an ethnic group attains a monopoly over a relatively valued occupation, incentives are provided for future generations to identify with the group and thereby to resist assimilation.

Thus, the position of any kind of group in the stratification system and the extent of its occupational specialization should independently affect the degree of its solidarity. Each factor refers to a type of social structural niche which leads to particular kinds of group interests. Arguments have been advanced to suggest that the lower the position of any objectively defined group in the stratification system, the higher its resulting solidarity will be; and the greater its occupational specialization, the higher its resulting solidarity.

These ideas can be tested by using a simple three-variable model. The solidarity of each ethnic group is the dependent variable, while the independent variables are the position of the ethnic group in the system of stratification, and the extent of its occupational specialization. This test uses data on 17 ethnic groups drawn from the 1970 Public Use Samples of the United States census. These comprise 1% representative samples drawn from census interview schedules for the persons and the households, respectively, of the entire American population.

For the purpose of studying ethnic group formation these data have some important advantages as well as significant limitations. The most obvious advantage is the size and representativeness of the sample. This makes it possible to study many groups which are too small to be analyzed in the most extensive sample surveys. While this test is necessarily cross-sectional, projects are now under way at several universities to create comparable Public Use Samples for previous censuses, beginning in the 19th century. When these data have been collected, it will be possible to trace the evolution of contemporary patterns of group formation in great detail. The methods employed here can be applied—with minor modifications—to the

individual's access to relevant networks—through personal contact—which leads to information about where jobs are and how they may be acquired. The author concludes that "information in any society is both costly and valuable; there is no reason why it should be expected to flow easily unless there is direct compensation or a personal tie" (Granovetter 1974, p. 99). To the extent that ethnic groups make up friendship networks (see Laumann 1973) in given localities, occupational specialization is likely to be enhanced by these processes. This is because information costs will be generally lower among individuals within such ethnic groups than between them. The ethnic boundary is quintessentially an information boundary, as research on linguistic code switching has shown (Blom and Gumperz 1971). Thus "ascription involves using an existent, preestablished structure as a resource rather than creating a new specialized structure for the same purpose" (Mayhew 1969).

analysis of similar problems in earlier Public Use Samples. However, the representativeness of the sample is achieved at some cost. As in all census data, there is very little information on the respondents' parents, and none at all on their religion. It is only possible to identify racial minorities and those who tend to be first- or second-generation immigrants. All told these groups constitute 37% of the American population.

Ethnic groups were defined for a subsample of all employed males ($N =$ 56,000).[7] To include data on the largest possible number of ethnic groups three separate cultural markers have been considered: race, mother tongue, and (in the case of English and French speakers) national origin. The final list of 17 was selected so as to maximize the number of distinct groups while preserving their internal homogeneity as much as possible (see table 1).[8]

The first task is to find an indicator of each group's position in the stratification system. Although there has been a long-standing debate about the most appropriate measures of stratification, few options are available when analyzing census data. This is because the census collects much information about occupations but none at all about classes—at least about classes in Marxian terms. Yet there is undoubtedly a healthy correlation between measures of stratification based on class and those based on occupational criteria. For this reason the mean occupational prestige of each group can serve as an adequate indicator.[9] These means vary considerably, from 27 (for the blacks) to 48 (for Yiddish speakers). Yet it would be wrong to assume that low ranking groups have a much greater tendency to be clustered in a single stratum of the labor market, and consequently have greater solidarity on this account, than groups of high rank. The rather high standard deviations for these means attest to the fact that there is considerable stratification within each of these groups.[10]

The indicator of occupational specialization measures the extent to which the set of occupied males in each cultural group deviates from being ran-

[7] In order to eliminate the confounding effects of sex on occupational attainment, the analysis was confined to males.

[8] In the case of one group, composed of individuals with Scandinavian mother tongues, aggregation was necessary due to small sample sizes. Other small groups that could not justifiably be aggregated (Russian, Lithuanian, and Ukrainian language groups among others) were not included in the analysis. Nearly 63% of this sample, Caucasians having English as a mother tongue and both parents born in the United States, is treated as unidentifiable here (and is not included in the following analyses) since third- and fourth-generation Americans of many different ancestries are included in this category

[9] This analysis uses an index of occupational prestige based upon the method devised by Duncan (1961) but specifically adapted to the occupational categories of the 1970 census by Temme (1975).

[10] Thus, it is quite conceivable that for individuals within each group education (and other factors) may well determine occupational achievement. The present discussion is, of course, concerned with the stratification of groups.

domly distributed among the occupational categories.[11] This is a standardized measure that is insensitive to differences in the size of the cultural groups. To determine the extent to which some groups specialize in narrowly defined categories, aggregation of discrete occupational categories has been kept to an absolute minimum. The measure employed here is based on data for 441 occupations. Although approximately 20,000 job titles are cur-

TABLE 1

CHARACTERISTICS OF ETHNIC GROUPS, 1970

Ethnic Group	Occupational Specialization	Occupational Prestige		Territorial Concentration	N*	Index of Endogamy	N†
		M	SD				
1. Black	.138	26.74	14.65	2.93	4,756	987	2,941
2. Asian	155	35.85	19.63	11.84	415	803	253
3. British	129	41.10	17.14	3.86	821	.141	685
4. Irish	118	38.78	16.47	5.45	470	.224	382
5. Other English speaking	.149	40.82	16.91	3.38	2,476	.172	1,939
6. French-Canadian	.126	37.16	14.41	4.24	700	411	567
7. German	160	38.33	16.60	4.30	2,326	.349	2,028
8. Scandinavian‡	178	37.85	16.34	4.60	659	281	551
9. Italian	.126	36.52	15.20	6.28	1,692	505	1,394
10. Yiddish speaking	370	47.62	17.94	11.60	689	.655	584
11. Polish	.116	36.18	14.92	4.74	1,025	.412	821
12. Czech	199	37.34	16.60	3.84	191	363	179
13. Slovak	126	37.30	15.79	6.93	209	318	171
14. Hungarian	.111	37.25	17.62	3.35	181	324	143
15. Greek	273	39.56	16.50	4.13	178	.595	129
16. Dutch	225	39.14	18.08	3.78	134	405	121
17. Hispanic§	153	28.92	15.80	4.91	1,795	.766	1,266
Unidentifiable	101	36.47	16.87	.	36,231	565	26,106
Other nonwhite	145	27.99	15.25		243	599	154
Russian-Ukrainian	.130	39.09	17.27		192	365	159
Lithuanian	193	40.49	16.16		90	.407	74
Cuban	211	34.13	17.24		126	646	98
Non-English-speaking white	.116	38.34	17.34	.	1,001	307	760
Total	.	35.95	16.95		56,600	.	41,505

SOURCE.—U.S. Bureau of the Census, *Public Use Samples of Basic Records from the 1970 Census Description and Technical Documentation* (Washington, D.C.: Government Printing Office, 1972).

NOTE.—Data are for males only.

* Refers to the total number of employed (or occupied) males enumerated in each ethnic category. All variables in cols. 1-4 are calculated from this baseline.

† The total number of married males (employed or not) enumerated in each ethnic category. It is used to create the endogomy variable.

‡ Includes Swedish, Danish, Norwegian, and Finnish mother tongues.

§ Excludes Cubans, since a high proportion of Cuban marriages did not take place in the United States.

[11] This measure is defined as follows:

$$X_1 = 10\left[\sum_{i}^{441}\left(\frac{N_{ij}}{N_j} - \frac{1}{K}\right)^2\right]$$

where N_{ij} is the number of individuals in cultural group j employed in occupation i, N_j is the total number of individuals in cultural group j, and $K = 441$.

rently listed in the United States, the categorization used here is detailed enough to point up the truth of certain stereotypes about ethnic employment.[12] Blacks are overrepresented as janitors; Asians as farm laborers, cooks and waiters, and gardeners. Hispanics are disproportionately employed as farm laborers and unskilled urban laborers. The Irish are more likely to be policemen than any other group; the Italians to be barbers. Eastern European groups are found especially as skilled industrial workers; Germans, Scandinavians, Czechs, and Dutch are overrepresented as farmers, while Yiddish-speaking Jews are concentrated in management, sales, medicine, and accountancy. However, there are also some less obvious findings. The French-Canadians in this sample are most specialized in management. Although Asians are likely to be employed in restaurants, they also have a niche in electrical engineering, and Hungarians are overrepresented as designers.

Last, ethnic group solidarity must be indicated by the strength of sentiments binding individuals into a collectivity; it alludes to the quality of relations existing among individuals sharing certain cultural markers. In the absence of attitudinal data, the measurement of group solidarity must be deduced from behavioral evidence. There is general consensus that endogamy is the ultimate measure of the salience of boundaries for intergroup relations (Merton 1941; Schumpeter 1955; Bromley 1976), and this reasoning has been adopted here. Ethnically endogamous groups are less open to interethnic interaction than exogamous ones, and there is ample evidence of a negative association between exogamy and ethnic identity among American males (see, for example, Alba 1976).[13]

[12] Even so, this measure underestimates the actual occupational specialization of American ethnic groups. This is because the census does not provide enough detailed information for more accurate estimates. Some of this detail may be illustrated from a survey of the occupations of Japanese Americans resident in the western United States who were placed in detention camps in the spring of 1942. Although the Japanese operated only 3.9% of all farms in California and harvested 2.7% of all cropland harvested, they produced 90% or more of the following crops: snap beans for marketing; celery, spring and summer; peppers; strawberries. They produced 50%–90% of the following: artichokes; snap beans for canning; cauliflower; celery, fall and winter; cucumbers; fall peas; spinach; tomatoes. Finally, they produced from 25%–50% of the following. asparagus, cabbage, cantaloupes, carrots, lettuce, onions, and watermelons (U.S. Congress 1942, pp. 117–18). These data led one student of American censuses to believe that "there may in fact be many such partial divisions of labor between native and foreign born and between different national-origin groups, to an extent that is not generally known or suspected" (Hutchinson 1956, p. 70).

[13] An adequate measure should take into account the proportions of each group in the population as well as its age-sex composition (see Price and Zubrzycki 1962; Romney 1971). For the purpose of testing the models of ethnic solidarity here, however, a commonly used index of endogamy that controls only for differential group size is employed (Savorgnan 1950; Hutchinson 1957; Lieberson 1963). The measure is derived from a different population from that used to create the previous measures, namely, the set of

The zero-order correlation matrix for these variables (table 2) reveals that endogamy is moderately correlated with occupational prestige ($r =$ —.534), but appears unrelated to occupational specialization. In general, the more specialized groups have occupations of higher prestige ($r = .549$). The significance of the occupational-specialization variable emerges only in the regression equation. When the rate of endogamy is regressed on both occupational specialization and the mean prestige score, fully 73% of the variance is explained (table 3). Further, the signs of the regression coefficients are in the predicted directions. Thus, the greater the occupational specialization of a group, the greater its rate of endogamy when the effects of prestige are controlled. Conversely, the greater the occupational prestige of a group, the less its rate of endogamy when the effects of specialization are controlled. While mean occupational prestige has a somewhat stronger effect on variation in endogamy ($b = -.973$), the effect of occupational specialization is also potent ($b = .800$). If endogamy is accepted as a de-

TABLE 2

RELATIONSHIP OF GROUP FORMATION VARIABLES ($N = 17$)

	ZERO-ORDER CORRELATIONS		
	1	2	3
1 Mean occupational prestige	1 000		
2 Occupational specialization ...	549	1 000	
3. Index of endogamy	− 534	266	1.000

TABLE 3

DETERMINANTS OF ENDOGAMY: EFFECT OF OCCUPATIONAL STRUCTURE VARIABLES

Variable	*b*	*B**	*F*	Significance
Mean occupational prestige	−.973	− 054	34.678	000
Occupational specialization	800	2.786	23.421	000

NOTE.—$R^2 = .733$, $F = 19.191$, significance = .000, $N = 17$.
* Unstandardized regression coefficient.

married couples in the 1% sample (thus, including males without any occupation). Since differences in endogamy are based upon a population of married couples, the effects of differential age-sex composition among the groups should be small. Hence potential error creeps into the analysis from at least two separate directions. First, there are differences in the populations used to create the independent variables and the dependent variable. Second, the sample may include recent immigrants to the United States, but there is no way to determine whether any given marriage occurred outside the country—for example, in the country of emigration. It should therefore be expected that the most recent groups to immigrate in large numbers will have inflated rates of endogamy. For all of these reasons, even in the unlikely event that this were a perfectly specified and complete model, it could not be expected wholly to predict the variation in endogamy among these different groups.

cent indicator of the salience of group boundaries, any explanation of ethnic group solidarity must include *both* of these parameters. Each is important quite apart from the effects of the other. Attention has already been drawn to the salience of boundaries between ethnic groups having similar positions within the stratification system. An important reason for the tenuousness of "rainbow coalitions" among racial minorities in the United States is elucidated by this analysis: blacks and Hispanics are specialized in very different occupations.

It must be stressed that this simple structural explanation of ethnic solidarity was made without reference to a host of factors often held responsible for its development—territoriality, institutional completeness, and the cultural legacies of particular groups, among others. There is little doubt that these factors play an important role in the determination of ethnic solidarity; it will be seen that territoriality even has causal significance. Most, however, are probably dependent on a group's position in the occupational structure.

The model is more successful in accounting for solidarity in some of these groups than others. Analysis of the residual values from the regression equation reveals that the endogamy rate among Asians, Yiddish speakers, blacks, and Italians is significantly underestimated by this equation, indicating that solidarity among these groups is not simply a function of their position in the occupational structure: territorial concentration has an independent effect. The greater the territorial concentration of an ethnic group, the more likely its intragroup interaction and therefore its solidarity. When a measure of the territorial concentration of ethnic groups in counties is added to the stepwise regression equation,[14] 89% of the variance in endogamy rates is explained—an increase of 16% (table 4; see also table 1). The residual values for Asians, Yiddish speakers, and Italians are considerably reduced by the addition of the territorial variable. The residual for blacks, however, remains high since they are so dispersed geographically. Despite their lack of territorial concentration (when counties are taken as the unit of analysis) blacks are extremely segregated residentially—a dimension this crude ecological variable cannot tap. National estimates of the respective residential

[14] This measure was developed by Thomas Hall from a combination of two geographical units: county groups ($N = 149$) and individual counties. A cross-tabulation of ethnic groups by county groups was produced from the census tape. Each county group containing more than 2% of all ethnic groups was broken down into its constituent counties. This combination of county groups and counties yielded 244 distinct areal units. The measure of territorial concentration is based only on those cells (in the cross-tabulation of ethnic groups by areal units) containing 2% or more of each ethnic group. The cumulative percentage of each group contained in these cells was then divided by the total number of cells meeting the 2% criterion. The resulting measure is the mean percentage per cell for cells containing 2% or more of an ethnic group.

TABLE 4

DETERMINANTS OF ENDOGAMY: EFFECT OF OCCUPATIONAL STRUCTURE VARIABLES AND TERRITORIAL CONCENTRATION

Variable	b	B*	F	Significance
Mean occupational prestige	−1 046	−.054	90.026	0
Occupational specialization	.656	2.287	33.320	.000
Territorial concentration. . . .	.465	040	19 209	.001

NOTE.—R^2 = .892, F = 35.838, significance = .000, N = 17.
* Unstandardized regression coefficient.

segregation of ethnic groups would go a long way toward a full explanation of variation in rates of endogamy.

The territorial variable is by far the weakest of these three determinants of endogamy. This suggests that the territoriality of a group may be in part a function of its position in the occupational structure (for some evidence supporting this position, see Guest and Weed [1976]). Thus, to a remarkable degree, the predicted relationships are borne out in this test.

ON THE RELATIONSHIP BETWEEN ETHNIC AND CLASS IDENTITY

The previous section presented a simple structural account of group formation in a context where only one type of group membership, ethnicity, was considered. It argued that the solidarity of a group is increased to the extent that both intergroup stratification and intragroup interaction are maximized. But this is an unduly simplified picture. Group formation in industrial societies is complicated by the fact that individuals must choose between several identities, and these identities may be more congruent or less so. Just as every individual may be assigned to a particular ethnic group on the basis of such markers as language, religion, or skin color, so he may be assigned to many other kinds of groups on the basis of different criteria. Most important, each member of an ethnic group is simultaneously a member of a particular class.

Classes are comprehensive groups made up of individuals having a common relationship to the means of production. They are quite limited in number and are hierarchically ordered. By definition classes have antagonistic material interests and, presumably, a sharp social boundary separates them (Ossowski 1963, pp. 135–36). Like any other groups, classes may be more or less self-conscious. The social structural (as against ideological or political) conditions leading to the development of class consciousness should be similar to those promoting any other type of group consciousness. Thus it should be expected that the greater the material and social distance between proletariat and bourgeoisie, the greater the probability that the proletariat as a whole will be class conscious. This is one of the central arguments in

The Communist Manifesto. And the greater the intensity of interaction within the proletariat, the greater its resultant solidarity. Hence Marx placed great emphasis on the ecological obstacles to class consciousness among French peasants in *The 18th Brumaire of Louis Bonaparte:* these are seen to impose barriers to communication among individuals otherwise having the potential to form a single corporate group.

If any individual identifies primarily with others having a similar relation to the means of production he may be said to be class conscious. If, on the other hand, he identifies primarily with others bearing similar cultural markers, irrespective of their relationship to the means of production, he may be said to be ethnically conscious. The question therefore arises: what determines which of these potential bases of association is relatively stronger?

Patterns of both stratification and interaction play a role here. To the extent that group formation occurs through interaction processes, class formation will predominate if most interaction occurs within classes and between ethnic groups. (For the purposes of argument, only two types of group formation are considered here. The situation is complicated by the addition of more types, but it is not fundamentally changed.) On the other hand, ethnicity will be favored if most interaction occurs within ethnic groups and between classes. It follows that neither will be favored if interaction occurs within classes and ethnic groups simultaneously.

Similarly, if an individual perceives his class origin to be more important for the determination of his life chances than his ethnicity, he is more likely to be class than ethnically conscious. Correlatively, if his ethnicity appears to be more important than his class origins in this respect, his ethnic identity will be more salient. Of course the respective weight of these independent factors in the establishment of ethnic and class identities remains to be determined empirically.

In the discussion which follows an attempt will be made to discover the relative importance of class as against ethnic identity among the 17 American ethnic groups. Owing to the absence of any attitudinal measures the analysis will perforce be based on patterns of class and ethnic endogamy. There is little doubt that this provides rather indirect evidence of class and ethnic identity. Yet there are also grounds to believe that endogamy is a decent, if imperfect, indicator of both ethnic and class boundaries. Endogamy has been used previously to indicate the strength of ethnic boundaries, but it can serve the same purpose in the case of class boundaries. Thus it has been found that British workers with wives of white-collar origins have lower rates of participation in working-class voluntary associations, and less class awareness, than their comrades who married within the working class (Goldthorpe et al. 1969, pp. 112–13, 159–60).

The data on ethnic endogamy presented above shed no light on the relationship between ethnic and class identity within these groups. One way to learn about this is to ask whether each group of males chooses its wives from similar class or ethnic backgrounds. Since marriage represents more than a casual alliance between families, it opens new avenues for primary interaction, avenues that are to some degree obligatory (especially among the working class). Each group's willingness to cross ethnic or class lines in its marital behavior should reveal something about the salience of these respective categories as social boundaries.

Measurement problems continue to complicate the picture. The Public Use Samples do not provide information on fathers' occupations for either the male or female respondents. Hence estimates of class origin must be made. Since father's occupation is rather strongly correlated with both son's (Blau and Duncan 1967, p. 169) and daughter's (Treiman and Terrell 1975, pp. 179–80) educational attainment, the latter variable can reasonably stand as a proxy. Table 5 compares the probabilities of homogamous marriages on both class and ethnic dimensions. Despite the indirectness of the class measure the results are strikingly unambiguous. Whereas rates of ethnic endogamy vary among the groups all the way from .99 to .16, about

TABLE 5

EFFECTS OF CLASS AND ETHNIC ORIGIN ON MARITAL CHOICE

	Probabilities of					
Ethnic Group	Ethnic Endogamy (1)	Class Endogamy* (2)	Ethnic and Class Endogamy (3)	Ethnic and Class Exogamy (4)	Ethnic Exogamy and Class Endogamy (5)	Ethnic Endogamy and Class Exogamy (6)
Black	985	675	665	005	010	320
Asian	877	692	605	036	087	272
Hispanic	777	661	538	.093	130	239
Yiddish speaking	601	.661	396	134	265	205
Greek	.519	.636	333	217	264	.186
Italian	.478	.676	344	190	332	.134
French-Canadian	.411	.621	257	226	363	.154
Dutch	.405	.612	.298	.281	314	107
Polish	.395	.654	267	.218	387	128
German	350	.617	.232	.264	386	118
Hungarian	336	685	217	.195	.469	119
Czech	.313	.626	.207	268	419	106
Slovak	310	.649	222	.263	427	088
Scandinavian	.270	.608	174	164	.434	.096
Irish	.215	.607	092	.248	.463	123
Other English speaking	.207	.631	137	.287	.494	070
British	.155	.628	101	.372	.527	054
Unidentifiable	857	660	571	.054	.089	.286

* Indicated by level of educational attainment of the respective couples. The categories are based on highest grade achieved—16 and above, 12–15, 11 and under.

two-thirds of the males in *each* group are likely to choose a spouse from the same class background. Yet this does not indicate the relative importance of either factor as a social boundary because the effects of class and ethnicity are confounded. Columns 3–6 disentangle these effects by portraying patterns of marital choice in each group as a series of four separate probabilities. These are the probability that a marriage occurs within the same class and ethnic group (col. 3); the probability that it occurs across both class and ethnic lines (col. 4); the probability that it crosses ethnic lines alone (col. 5); and the probability that it crosses class lines alone (col. 6).

Insofar as marital choice affects an individual's class or ethnic identity, class endogamy should facilitate class identity, whereas ethnic endogamy should facilitate ethnic identity. Hence columns 3 and 4 are of no use in determining which of these factors is stronger among the various groups. Columns 5 and 6, however, do provide some evidence of the relative permeability of class as against ethnic boundaries. The probability that cross-ethnic marriage occurs among individuals of the same class rises dramatically from the blacks (.010) to the British (.527). On the other hand, the probability that cross-class marriage occurs among individuals within the same ethnic group falls in a continuum from the blacks (.320) to the British (.054). Thus there is a strong positive relationship between a group's overall level of ethnic endogamy and its preference for marriages across class as against ethnic lines.[15] To illustrate: blacks are 30 times more likely to marry across class than ethnic lines whereas the British group is nearly 10 times more likely to engage in cross-ethnic than cross-class marriages.

Altogether these findings indicate that individuals in ethnically exogamous groups are more likely to interact along class than along ethnic lines. It may reasonably be argued that on this account they are more likely to develop class identities and thus will be more open to the ideology of class consciousness than are individuals from ethnically endogamous groups. The relationship between class and ethnic identity among these groups is therefore to some degree a competitive one.

CLASS AND ETHNIC CLEAVAGES AT THE SOCIETAL LEVEL: THE ROLE OF THE CULTURAL DIVISION OF LABOR

Heretofore the ethnic group has been taken as the unit of analysis in order to explore the determinants of group solidarity. Now the focus shifts to a higher level of analysis—that of the social formation as a whole. The task

[15] It is worth emphasizing that differences in the overall rates of ethnic endogamy cannot wholly be accounted for either by the group's position in the stratification system (see table 4), or—at the individual level—by holding educational attainment constant. The probabilities of ethnic endogamy among males with 16 or more years of education vary all the way from .14 to .98.

is to discover the consequences of different patterns of ethnic stratification on the salience of class and ethnic cleavages in the polity.

The widespread belief of the classical social theorists that class would supersede ethnicity and other status-based identities rested in part upon the assumption that the spatial and cultural barriers to intraclass communication would be eroded in the course of industrialization. A long list of structural changes—including the separation of workplace and residence; the increased scale of social production; urbanization, leading to that famous cauldron, the melting pot; the breakup of extended families; the greater efficiency of labor and commodity markets; and the establishment of universal education in the national language—all seemed to insure that interaction would occur increasingly on an intraclass rather than an intraethnic basis. Moreover, many of these theorists anticipated that under capitalism individuals would be assigned to jobs on universalistic grounds, relating to skill and efficiency, which in principle would be orthogonal to membership in ethnic groups. For this reason there was every expectation that ethnic groups—and all other kinds of status groups—would in time have equivalent positions in the social structure. If ethnicity had little effect on an individual's life chances its force would surely diminish—and there would be one fewer obstacle to the formation of self-conscious classes.

But this simple expectation overlooked several things. First was the importance of international migration. Every developing industrial economy was faced from time to time with shortages of unskilled labor that were frequently met by labor recruitment from less developed sectors of the world economy. The resulting waves of immigration promoted ethnic stratification instead of reducing it (Hechter 1976*a*; Castells 1975). This view also underestimated the role of informal interaction networks for getting jobs and choosing residential location. These and other factors are responsible for the reality that at least some ethnic groups continue to hold particular niches in the social structure of the United States and in many other industrial countries. The fact that they do so reinforces, or institutes anew, their ethnic identity somewhat to the detriment of loyalty to that wider and more culturally heterogeneous group, their class.

If class cleavages should prove to be muted in those societies where ethnicity has great salience, what are the societal conditions promoting ethnic solidarity in general? From the earlier analysis it has been seen that ethnic solidarity will be strengthened to the extent that interethnic stratification and intraethnic interaction are both maximized. Since a society with these characteristics should have strong ethnic cleavages it should have weak class cleavages. On the other hand, to the degree that interethnic stratification decreases and interethnic interaction increases, the prospects for class formation will be enhanced.

These general issues may be approached by using the concept of the cul-

tural division of labor. Whenever individuals having different cultural markers (in this case, ethnicity) are distributed through an occupational structure a cultural division of labor is thereby formed. However, since the pattern of the distribution of culturally marked groups in the occupational structure is variable, the cultural division of labor may take on different configurations. Just as a group's position in the occupational structure determines much about its level of solidarity, so the configuration of the cultural division of labor will affect the relative salience of ethnic as against class cleavages in the society as a whole.

The two defining parameters of the configuration of a cultural division of labor are its degrees of hierarchy and of segmentation. A cultural division of labor is *hierarchical* to the extent that the groups within it (ethnic groups in this case) are differentially stratified. A cultural division of labor is *segmental* to the extent that the ethnic groups within it are occupationally specialized to a high degree. Although these two factors may be empirically related they are analytically independent of one another. Both hierarchy and segmentation contribute separately to the strength of ethnic cleavages and corresponding weakness of class ones in the polity as a whole. To the degree that the cultural division of labor is hierarchical, stratification between ethnic groups will be maximized; to the degree that it is segmental, interaction within ethnic groups will be maximized. If these two basic requirements of group formation are met by ethnic groups, but not by classes, ethnic cleavages will be correspondingly strong. For precisely the opposite reasons cultural divisions of labor having low degrees of hierarchy and segmentation will encourage (but not necessarily *cause*) class formation and thus lead to stronger class cleavages in the polity as a whole.

The best example of a cultural division of labor that is simultaneously hierarchical and segmental is, of course, caste society (Weber 1946; Bouglé 1971; Leach 1962; Barth 1962; Berreman 1972). While there has been much debate about the precise definition of caste—as well as its utility in comparative analysis (see de Reuck and Knight 1967)—nevertheless it is usually held that caste implies three things: hereditary occupational specialization, a hierarchical ranking of groups, and great social distance between groups leading to extreme rates of endogamy. Interaction between castes is governed by rules of purity and impurity. If an untouchable so much as gazes at the dinner of a Brahman, the latter's dinner will be considered impure. Exogamy is prohibited, and imbalanced sex ratios among higher castes lead to hypergamy as well as female infanticide. The extreme sense of mutual repulsion among castes inhibits the development of alliances among groups having similar relations to the means of production.

There is a powerful structural tendency in most industrial societies that limits these dimensions of the cultural division of labor, namely, the pres-

sure toward social mobility (Bendix and Lipset 1959). In a highly differentiated division of labor all individuals are not capable of performing each occupational role. Not everyone can become a physics professor or a soccer star. Further, there is no a priori reason to suspect that some ethnic groups are better endowed than others to produce physicists or soccer stars. Channels of social mobility, education chief among them, have the usual function of insuring an ample supply of qualified individuals to perform specialized tasks. The idealized image of capitalist industrialism held by the 19th-century theorists, and by their contemporary followers, is thus neither hierarchical nor segmental. Rather, it is a kind of meritocracy where "race, religion, and national origin" are irrelevant to an individual's life chances. Under these conditions, it is easy to see that ethnic groups would not be differentially stratified and that interaction would generally cut across ethnic boundaries, thus favoring class formation. These, then, are the polar types of configurations of the cultural division of labor. Any actual social formation will tend to fall somewhere between these extremes of hierarchy and segmentation.

To explain differences in the political salience of class by variations in the configurations of the cultural division of labor would seem a relatively simple task. Yet adequate data are once again in short supply. Although the concept of class has long concerned political sociologists, there have been relatively few attempts to measure class cleavages comparatively. Following the conception that elections are reflective of the class structure (Lipset 1963) the most effective measures have been based on surveys of voter preferences, or the analysis of aggregate election returns. The most straightforward of these measures is an index of class voting developed by Alford (1963).[16] Using this index Alford compared patterns of class voting in the United Kingdom, Australia, the United States, and Canada. Large differences in class voting emerged among the four societies, but these differences were not explained systematically. Alford ascribed some of the variation to contextual differences among the four countries: he held that the level of class voting was affected by the society's type of party system, the extent of its regional economic inequality, and the degree to which national minorities are geographically concentrated. All things equal, states with trade-union-based parties (the United Kingdom and Australia) should have higher levels of class voting than those lacking such parties (the United States and Canada). Similarly, regional economic inequality should lower class voting.

[16] The index of class voting is computed by subtracting the percentage of persons in nonmanual occupations voting for Left parties from the percentage of persons in manual occupations voting for Left parties. The index assumes that the key dimension of class voting is the gap between the voting patterns of manual and nonmanual occupations, not the overall level of Right or Left voting.

In order to evaluate the effects of variation in cultural divisions of labor, all independent effects on class voting should be controlled. In the face of severe evidentiary constraints the best strategy is to perform a regional analysis in a single country where contextual differences between the units to be compared are negligible. Since continental Australia is considerably more regionally homogeneous—both economically (Williamson 1965) and culturally (Alford 1963, p. 168)—than the United Kingdom, the United States, or Canada, it is best suited for this test.

The degree of hierarchy in the cultural division of labor may be estimated by the variance in mean occupational prestige among all ethnic groups. The higher this variance, the more the interethnic stratification; consequently, class voting should decline. The degree of segmentation is indicated by the mean occupational specialization for all ethnic groups. The higher this value, the greater the overall tendency toward ethnic occupational specialization; consequently, the lower the class voting.

Aggregate data from the Australian census of 1961 provide a rough occupational breakdown ($K = 10$) for immigrants from 28 countries as well as the native born. Alford (1967, p. 86) reports the index of class voting for Australian states in 1964. Table 6 presents this for five Australian states, together with the estimates of hierarchy and segmentation (Tasmania is excluded from the analysis because its politics is quite different from that of the mainland states).[17]

Class voting is strongly associated in the predicted direction with the

TABLE 6

CHARACTERISTICS OF FIVE AUSTRALIAN STATES, 1961

	Index of Class Voting*	Variance in Mean Ethnic Occupational Prestige†	Mean Ethnic Occupational Specialization
Queensland	37	6 38	180
Western Australia	27	8.37	.186
New South Wales	26	7.04	.233
Victoria	19	11.64	247
South Australia	17	9 72	268

SOURCES.—*Census of the Commonwealth of Australia*, 1961 (Canberra: Commonwealth Bureau of Census and Statistics, 1963), Alford 1967, p. 86.
* 1964 data.
† Estimates of occupational prestige are derived from Treiman's (1975) Standard International Occupational Prestige Scale Data are presented for the 28 largest migrant groups as well as the native born.

[17] The regional distinctiveness of Tasmania derives from its insular geography. The islanders are careful to distinguish between "Australians" and "Taswegians." Tasmania's political distinctiveness is reflected in institutional terms: alone among Australian states it has an electoral system based on proportional representation (Townsley 1976, pp 20–38).

measures of both hierarchy and segmentation. The zero-order correlation between the index of class voting and the variance in ethnic occupational prestige is —.837 (significant at the .08 level), while its correlation with mean ethnic occupational specialization is —.893 (significant at the .04 level). Whereas ethnic heterogeneity per se has often been thought to inhibit class formation (Rosenblum 1973), this analysis suggests that the absolute percentage of ethnic minorities in a social formation has no necessary consequence for variations in class formation. The configurational hypotheses, instead, predict that the interrelationship of these groups within the occupational structure is critical for class formation. To the degree these relationships are both hierarchical and segmental, the class principle will be attenuated.

While these results seem to support the configurational hypotheses, they can be regarded as merely suggestive. The small number of cases coupled with the absence of adequate controls does not allow the role of the configurations of the cultural division of labor to be gauged with much confidence. Nevertheless, these are the only data to be analyzed that bear on the hypothesis. As such, they should not be ignored: rather, they call for further research.

CONCLUSION

This paper has discussed the process of group formation and the relation between class and status group formation in complex societies. The concept of the cultural division of labor was introduced in order to analyze the latter problem. The cultural division of labor approach provides a simple and internally consistent set of propositions concerning the relationship between class and status group formation. When these propositions are stated in an operational form the problem becomes subject to straightforward empirical analysis. The importance of this cannot be overemphasized, for this is an issue that tends to be discussed in polemical or impressionistic ways—when it is raised at all. But the question is too consequential to be treated casually and with narrow political animus.

Data from two limited tests illustrated the utility of this approach by demonstrating that the solidarity of groups is in large part a function of overall patterns of intergroup stratification and interaction. Finally, the degrees of hierarchy and segmentation of the cultural division of labor were seen to influence the probability of class as against status group based political behavior for social formations as a whole.

There are several important things this kind of structural analysis does not pretend to accomplish. It cannot explain the cultural character (see Sahlins 1976), the intensity of the sentiments, or the organizational capacity

of particular groups. Nor can it predict the outbreak of specific intergroup conflicts, such as race riots, in a particular place and time. The thrust of the analysis is quite different; it seeks to explicate the structural conditions that must lie at the base of different types of group formation.

Clearly factors lying beyond the realm of the cultural division of labor can intervene to blunt the potential of ethnic cleavages and strengthen that of class cleavages, or vice versa. If the state permits organization on an ethnic basis, but prohibits it on a class basis, this will decisively strengthen ethnic cleavages and weaken those of class quite independently of a given cultural division of labor. Similarly, if a depression sharply raises rates of unemployment among all ethnic groups in a society (as happened in many parts of the world during the 1930s) the ideology of class struggle will become much more appealing, even if there is no change in the cultural division of labor. This approach thus offers no substitute to detailed historical analysis of particular cases. However, in the absence of significant exogenous effects such as these, the kind of structural analysis illustrated here should be promising.

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Schooling and Socioeconomic Attainments: High School and College Influences[1]

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The socioeconomic consequences of qualitative variations in educational experiences are evaluated for a sample of young adult males who were first surveyed in 1955 as high school sophomores and followed up in 1970. Models of institutional influence and of within-school processes are developed for both secondary and postsecondary education to integrate and refine the literatures on school effects and returns to schooling. Rather impressive occupational status and earnings differentials are associated with gross school-to-school differences and with qualitative differences in educational experiences within institutions. Secondary school characteristics and experiences weigh particularly upon the market outcomes of youth who terminated formal schooling at high school graduation. We suggest that the traditional use of quantitative indices of schooling (years of school completed or certification levels) in assessing the market consequences of investments in education needs to be supplemented by information on qualitative variations in educational experiences. Additionally, the likelihood that school experiences may have quite different implications for selected target populations deserves further consideration. The simplistic assumption implicit in much of the school-effects literature that institutional effects are homogeneous may actually mask quite important consequences for certain students.

The diverse literatures on education and social stratification reveal a distressing disjuncture between the expectations based on theory and the evidence provided by research. Virtually every commentary on the functions of schooling in industrial society (sometimes limited to advanced capitalist societies) posits (or, more often, assumes) a preeminent responsibility of schools in preparing students for and allocating them to their adult work roles. While there is often sharp disagreement on precisely how schools promote such career preparation, on what dimensions of competency training are thought most significant, and on the valuation of this state of affairs, authors of such different persuasions as the functionalists Parsons (1959)

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and Moore (1969) and the radical economists Bowles and Gintis (1973, 1976; Bowles 1977; Gintis 1971) nevertheless find themselves on common ground in perceiving intimate linkages between society's systems of educational and social stratification. Schools are thought not only to channel and certify students but also to change them, either cognitively or noncognitively (see, especially, Dreeban 1967), in ways dictated by the requirements of the prevailing economic order.

The specific worker qualities that are thought instilled through schooling are themselves wide ranging, running the gamut from cognitive facility to deferential docility (cf., e.g., Mincer 1974 with Edwards 1976). Regardless of which of these are the theorists' preferred constructs, there appears to be quite general agreement that schooling and school organization are effective mechanisms for promoting them and, hence, for preparing students for their eventual careers. Thus it is held that the organization of schooling does not merely reflect the prevailing economic and social orders but, more significantly, is an active agent in their reproduction (Bowles 1977).

These are impressive and recurrent claims for the efficacy of schools as social constructions, yet the assertions of these largely theoretical expositions find little support in empirical assessments of the actual impact of school organization upon students (Jencks et al. 1972). How, then, might the bold and persistent claims from theory and/or informed judgment be reconciled with the failure of educational research to document the seemingly self-evident? While we are open to the possibility that this more theoretical literature may well exaggerate the extent to which schools and educational experiences do in fact transform students, we nevertheless think it premature to draw such a conclusion from the generally nonsupportive research evidence currently available. Four limitations of this and related literatures strike us as especially troublesome.

First, research on the contribution of schooling to the maintenance or alleviation of inequality, though voluminous and quite illuminating, nevertheless suffers from a certain narrowness of focus. The most common concern in both the sociological and economics literatures is to estimate the socioeconomic benefits accruing to marginal increments in educational attainment (Sewell and Hauser 1975; Griffin 1976; Mincer 1974; Bowles 1972). Such assessments, relying as they do on simple quantitative indices of years of schooling completed, or certification levels, ignore the labor market consequences of qualitative variations in education deriving both from differences between schools and from diverse experiences within them. An adequate investigation of the role of formal education in reproducing socioeconomic inequality from generation to generation must necessarily attend to these qualitative dimensions of educational inequality and to the way in which they, in turn, affect the positioning of individuals in the stratification system (cf. Bowles and Gintis 1973, 1976).

Second, it is now generally appreciated that between-school differences are at best modestly consequential for cognitive growth, educational performance, and educational attainment (e.g., Coleman et al. 1966; Mosteller and Moynihan 1972; Jencks et al. 1972; Hauser 1971; Hauser, Sewell, and Alwin 1976; Klitgaard and Hall 1975). While these, unquestionably, are important products of schooling, they can hardly be thought to exhaust the competencies and qualities that might be assets in the workplace (cf. Berg 1970). The personal qualities that actually make for a successful worker are not well understood, but suggestions abound in the literature, and the responsiveness of such traits, aside from the few mentioned above, to variations in school quality and to school experiences is virtually unknown. Inkeles's (1966) catalog of the dimensions of competency might be a useful starting point for such an exercise, but even his seemingly comprehensive scheme falls considerably short of exhausting the reasonable possibilities. Various radical critiques of schooling emphasize its role in producing workers intellectually and dispositionally suited to the requirements of the hierarchical social organization of work characteristic of monopoly capitalist production (Bowles and Gintis 1976). Qualities such as docility, reliability, punctuality, and deference to authority are assumed by Marxist scholars to be the important products of school socialization (Edwards 1976; Bowles and Gintis 1976). That such facets of disposition and deportment have been almost completely neglected in research on school effects underscores its narrow scope.

Actually there have been surprisingly few studies of school effects, either secondary or postsecondary, on adult socioeconomic achievement; therefore, we cannot conclude securely that school differences have little relevance for career attainments. In fact, the little literature that is available, though hardly conclusive, suggests otherwise. Jencks and Brown (1975), for example, found secondary school differences to account for an additional 9.5% (or 3.6% after corrections for degrees of freedom) of the variance in the occupational status of young men over that attributable to race, status origins, academic achievement, *and* educational attainment. Although comparable analyses have yet to be reported for earnings, the importance of per pupil educational expenditures for economic attainments has been the subject of some study (Wachtel 1975, 1976; Johnson and Stafford 1973; Link and Ratledge 1975; Morgenstern 1973). While the results of this research are often contradictory (cf., e.g., the positive conclusions of Wachtel [1975] and Link and Ratledge [1975] with the negative assessment by Morgenstern [1973]), Wachtel (1975), in what is perhaps the most adequate study of this sort, concluded that secondary school "quality" remained a significant determinant of earnings even net of a variety of individual-level controls. Thus, there is at least the suggestion that the influence of secondary schools on socioeconomic achievements may be appre-

ciable, despite the generally negative conclusions stemming from research on cognitive outcomes. Possible postsecondary consequences for socioeconomic attainment, also central to the present project, will be discussed below.

Third, the traditional search for school effects, in focusing on differences *between* educational institutions, neglects potentially quite important qualitative variations in the educational experiences of students within a given school setting. Thus, the differential access to and utilization of resources *within* schools may be of far greater consequence for student socialization and instructional outcomes than any disparities across schools in their personnel, facilities, educational philosophies, curriculum organization, and the like. Research on the relevance of the quality and character of peer associates, on the intensity and frequency of contact with counselors and teachers, and on secondary school track placement for a broad range of cognitive and noncognitive products of the educational process clearly suggests this possibility (Kerckhoff 1976; Hauser et al. 1976; Heyns 1974; Alexander, Cook, and McDill 1978).

We assume that such school resources also may have relevance both for adult achievements and for the perpetuation of inequality across generations. Enrollment in a college preparatory track, or the completion of particular courses in high school, may contribute to occupational and economic success by equipping students with valued skills, knowledge, or attitudes. Enrollment in vocational or business curricula may also furnish students—especially those lacking college certification—with specific job-related skills. These, in turn, may be rewarded in the workplace in the form of increased status or earnings. Even though evidence on the economic returns to vocational education in secondary schools is weak and inconsistent (cf. Hu, Lee, and Stromsdorfer 1971 and Baker and Levenson 1976), vocational education is often assumed to have considerable market utility, especially for the occupational careers of disadvantaged youth (see *Journal of Human Resources* 1968). Favorable interaction with counselors and teachers could result in more effective career counseling as well as more extensive (and more accurate) information on labor market conditions and employment opportunities, either or both of which may enhance career success (cf. Parnes and Kohen 1975). Of course, to the extent that these variables are directly affected by status origins (see Alexander, Cook, and McDill 1978; Heyns 1974) they may also identify schooling mechanisms responsible for intergenerational continuities in economic position, a point of some importance in the radical critique of American schooling (e.g., Bowles and Gintis 1976). Since the importance of such factors as sources of school influence has only recently been appreciated and since their consequences for stu-

dents' eventual labor market prospects have received scant consideration,[2] we think this a particularly fertile line of inquiry for furthering our understanding of the linkages between schooling and socioeconomic well-being.

Finally, virtually all studies of school impact on labor market outcomes have assumed that the determinants of occupational and economic success are similarly efficacious across all levels of educational attainment. Should this not, in fact, be the case—and there is good reason for skepticism (e.g., Alexander and Eckland 1975)—estimates of the importance of school experiences for adult success may be quite misleading, essentially confounding the importance of certain influences for some students with their triviality for other students.

Drawing again upon the suggestions from labor economics, in this instance the implications of both neoclassical theories of human capital and various perspectives on labor market segmentation (e.g., Gordon 1972), it might be assumed that students at different levels of schooling typically are being prepared for quite distinctive labor market careers with distinctive requirements for effective worker performance. If indeed the criteria for "appropriate" career preparation differ across levels of schooling, and schools are organized to promote these ends, then it seems plausible to anticipate that variations in school "quality" at any level of schooling will be most consequential for the market prospects of students who terminate their formal education at that level. Thus, the impact of differences in secondary school organization and experiences should be at a maximum for terminal high school graduates and quite attenuated for youth who continue their education into college (Wachtel 1976). For the latter, college experiences and characteristics should exceed those of their secondary schools in market consequences. The rationale for this expectation is advanced persuasively by Bowles and Gintis: ". . . the work-related personality traits required of employees differ according to the work role in question, those at the base of the hierarchy requiring a heavy emphasis on obedience and rules and those at the top, where the discretionary scope is considerable, requiring a greater ability to make decisions on the basis of well-internalized norms. This pattern is closely replicated in the social relations of schooling. Note the wide range of choice over curriculum, life style, and

[2] Although several studies have estimated the importance of school-based social supports and interpersonal resources for socioeconomic achievements (Sewell and Hauser 1975; Sewell, Haller, and Portes 1969; Alexander, Eckland, and Griffin 1975; Alwin 1974), none of them has controlled for secondary school differences. If schools do, in fact, affect labor market behavior, school differences must be taken into account; otherwise inferences about within-school (or individual) processes may be misleading, the counterpart of the school-effects "fallacy" (Alwin 1976*a*). Additionally, none of these studies considers whether the labor market consequences of school experiences vary for students with different levels of educational certification. We discuss the potential importance of these interactions as our final point in this section.

allocation of time afforded to college students, compared with the obedience and respect for authority expected in high school" (1973, p. 87). Thus, for terminal high school graduates the job-related skills acquired and/or refined in high school should be more relevant for economic success not only because they exhaust the stock of so-called human capital acquired by such students through formal schooling but also because they should be skills better suited to the kinds of work they are most likely to pursue. Additionally, in the absence of other screening devices for these youth, such as college diploma and evidence of college performance, employers are more likely to draw upon information pertaining to the quality of their secondary schools and to their educational experiences there in making hiring and/or promotion decisions.

We therefore anticipate that the labor market consequences of variations in educational experiences and school quality will differ substantially across student groups depending on the level at which their formal schooling is concluded. If such differences do obtain and are pronounced, the traditional procedures for assessing the impact of schooling may well underestimate that impact by mistakenly assuming it to be uniform for all students. The analysis that follows considers this possibility through parallel analysis of both the between-school and the within-school determinants of labor market success for terminal high school graduates and college goers.

We examine college goers separately, however, for yet another reason. Given the presumed need for highly skilled, technologically sophisticated labor power in advanced capitalist societies, postsecondary education is of increasing importance for the maintenance and growth of a knowledge-based economy (Clark 1962; Bell 1973). Indeed, societal pressure for a professionalized labor force is dramatically reflected in college-enrollment statistics; in the early 1970s, for example, over 50% of American secondary school graduates enrolled in some college program after high school graduation (Freeman 1976). We are observing, therefore, the "democratization" of higher education in an era of nearly universal access to secondary education (cf. Jencks et al. 1972). As institutions of higher learning have adapted to the changing economic requirements of late capitalism, they have not only grown in size and in number but also developed new programs and restructured existing ones (Freeman 1976). The distinctions both among and within postsecondary educational institutions are presumed to have a substantial impact on the growth of the "knowledge elite" (Galbraith 1967; Bell 1973) and on the subsequent socioeconomic careers of those who generate, disseminate, and manage technical expertise (Freeman 1976; Reed and Miller 1970). Empirical explorations linking schooling and social inequality, thus, should attend not only to secondary school "effects" but also to the manner in which diverse

college experiences are implicated in the intergenerational transmission of socioeconomic success or failure.

A modest body of literature is, in fact, devoted to the role of postsecondary education in affecting career attainments (e.g., Alwin, Hauser, and Sewell 1975; Solmon 1975; Reed and Miller 1970; Wachtel 1975, 1976). However, how it is that attendance at one or another postsecondary institution, or one's educational experiences within such institutions, condition career attainments remains poorly understood. On the one hand, it is known that students are not randomly allocated to colleges (Alwin 1974). Secondary school differences, status backgrounds, and academic ability influence the probability of college enrollment and, moreover, affect selection into certain types of colleges (Alexander and Eckland 1977). Thus, a college education may simply certify the marketability or trainability of prospective workers without actually changing them to a marked degree (on the "screening" hypothesis, see Berg [1970] and Layard and Psacharopoulos [1974]). On the other hand, colleges actually may equip their students with skills and/or attitudes which prove valuable in work organizations (e.g., Solmon 1975; Freeman 1976). The latter belief, certainly, provided the rationale for massive inflows of public money into higher education during the late 1950s and 1960s.

Early, and to some extent more recent, research on the socioeconomic careers of college men often did not (or could not) take into account many of these selection or "input" variables, thus possibly resulting in questionable inferences concerning the efficacy of college "quality" and/or experiences within postsecondary institutions (e.g., Freeman 1976; Spaeth 1970; Weisbrod and Karpoff 1968; Solmon 1975; Wales 1973). The results of more adequately specified achievement models now available (e.g., Alwin 1974, 1976*b*; Alwin et al. 1975; Wachtel 1975) indicate that institutional differences and/or academic performance and course of study in college influence some labor market attainments (earnings, probability of promotion) but not others (occupational status; e.g., Alwin 1974).

THE PRESENT ANALYSIS

The present project addresses many of the limitations described above and integrates these various perspectives on schooling and inequality by assessing the occupational status and earnings differentials associated, first, with attending different secondary and postsecondary educational institutions and, second, with academic experiences and allocative mechanisms situated within schools and colleges. For the reasons noted above, we examine these issues separately for terminal high school graduates and college goers. We hope to determine whether, how, and for whom between-

school differences and within-school resources and educational experiences constrain subsequent labor market achievement and thereby reproduce or modify socioeconomic inequality.

The Model

The model to be evaluated in this paper is presented schematically in figure 1. In addition to including variables examined in other research on general socioeconomic achievement processes (e.g., Sewell and Hauser 1975; Alexander et al. 1975), it has certain distinctive features. Because so few data sets contain information on parental income (the exceptions are Sewell and Hauser [1975] and Griffin [1976]) and religious background (Featherman 1971), for example, our understanding of their relevance for the quality of education received and, subsequently, for adult achievement is quite limited (for conjecture on such effects, see Bowles [1972]). Our present work also complements and expands existing research on the labor market achievements of college goers. Our college models include one important postsecondary institutional characteristic, college "selectivity" (Astin 1965), and several indicators of experiences and attainments within colleges (e.g., performance, receipt of honors; major; academic self-image). Few studies have considered the importance of both college attributes and experiences for socioeconomic attainment. Following Alwin (1974, 1976*b;* Alwin et al. 1975), we also specify the impact of socioeconomic background, ability, and a variety of high school socialization and allocation mechanisms, thereby controlling for the influence of these variables on college selection and performance and assessing the degree to which their effects are mediated by subsequent college experiences. Finally, in the college analyses we also hold constant secondary school differences (on the necessity of this control, see Alexander and Eckland [1977] and Wachtel [1975, 1976]), thereby taking

MOMED							
FAED	PEER	EDEXP	COLLCUR	COLIMAGE*			
FAOCC	PAR	OCCASP	COURSE	COLGPA*	EDUC*		
PARINC	TEACH		STAND	BUSINESS*			EARN
ACQUIS			IMAGE	ENGINEER*		OCC	
CATHOL			VOCCURR	HONORS*			
JEWISH*				SELECT*			
ABL							
SCHOOL							
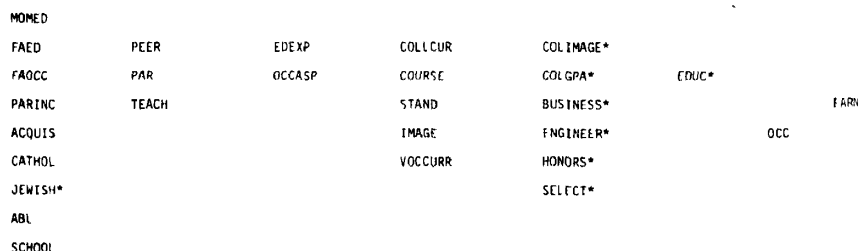
FIG 1.—High school and college model of socioeconomic achievement. See text and table 1 for explanation of variable abbreviations. Variables are ordered according to assumed causal sequencing. The model is fully recursive; to avoid unnecessary confusion we do not present arrows. Variables marked by an asterisk are employed only in the analysis of college goers.

into account patterns of institutional stratification across levels of schooling in assessing college effects. This model is one of the few which simultaneously includes indicators of high school *and* college quality and experiences.

The causal ordering of variables in this model was dictated, where possible, by the time frame in which they were measured or, if retrospective, to which they refer. Thus, for example, variables measured when the respondents were high school sophomores (e.g., significant others' influence, goal orientations) are considered antecedent to senior year outcomes (curricula, coursework, self-image, etc.), and all high school variables precede college experiences. Decisions regarding the ordering of variables measured at the same point in time were guided by the implications of specific item wording and judgments based on their theoretical relationships (e.g., Sewell et al. 1969). Our specification leaves unanalyzed the exact causal relationships between background status and school membership. As Hauser et al. note, however, net effects are the major concern of most school-effects research, and the estimation of these is unaffected by the causality between background and school membership (1976, p. 316).

PROCEDURES

An accurate assessment of the impact of school differences requires proper controls for individual-level attributes which may affect economic success (Hauser 1971; Alwin 1976*a*). Unless school-level determinants of socioeconomic achievement are unrelated to important individual traits that may have been omitted from the inquiry, inferences concerning the efficacy of school "quality" are quite likely to be erroneous. Jencks and Brown acknowledge this problem in noting that their estimate of school effects on occupational status attainment may reflect unmeasured student differences (1975, p. 306), and the various studies by economists are subject to the same criticism. The analytic procedures employed by most economists are deficient in yet another respect. Economists typically relate only *one* school resource—for example, per pupil expenditures—to individual earnings, and often that resource is measured at the district or state level. As a consequence, the estimated returns to educational expenditures are probably biased upward owing to the omission of other school-level attributes correlated with both expenditures and earnings. On the other hand, if this effect is interpreted as reflecting the consequences of attending different high schools, or of "school quality," as is typical, it is probably biased downward, since the one school-level characteristic studied is unlikely to capture all relevant school-to-school differences. Upper-bound or gross estimates of the socioeconomic consequences of attending different high schools may be obtained by analysis of covariance (see Hauser 1971; Alwin 1976*a*),

ʏhich is the analytic strategy employed in this paper.[8] Since this procedure ›artitions the variation in variables into their within- and between-school ːomponents, we can also assess the impact on socioeconomic inequality of ducational resources located *within* schools without fear that such influ- nces are contaminated by school-to-school differences (see n. 2).

ʼhe Sample

ʼhe data for this analysis are based on a national sample of youth, first tudied in 1955 as high school sophomores and followed up in 1970. The riginal 1955 survey of almost 35,000 students, conducted by the Educa- ional Testing Service (ETS), included all sophomores in 97 schools. It onsisted of two instruments: a 20-item test of academic aptitude which ıeasured both verbal and mathematical ability and a questionnaire.

The sample for the 1970 follow-up consisted of 4,151 of the same respon- ents from 42 schools. Usable follow-up data were obtained for 2,077 of ıese former students—1,130 females and 947 males. Our analysis is limited › the male subsample. There were 525 men in our sample who reported aving pursued an academic course of study in either a two-year community ›llege or a four-year college or university. We identify them as "college ›ers." There were 338 men who terminated their academic schooling at ›r, for a very small number of students, before) high school graduation. hey constitute the subsample of "high school graduates" and include re- ›ondents whose postsecondary schooling was restricted to either technical/ ›cational training or nonacademic coursework. Men with unknown educa- onal attainments were excluded from this analysis.

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[ost of the variables we used pertain to both terminal high school grad- ıtes and college goers. Those numbered 12 through 17 pertain only to ›llege goers.

1. Five background status variables are employed in this analysis. Except r some missing data estimates, all were obtained from the 1970 schedule:) mother's education and (*b*) father's education, scaled to years of school- g completed; (*c*) father's occupation while the respondent was in high hool, coded in the Duncan SEI scale; (*d*) parental income while the re-

[8] The technical details of the analysis of covariance and of its application to the study school effects are developed thoroughly in the references cited. The procedure pre- mes homogeneity of regression within schools, a condition which we have not formal- evaluated in the data used in the present project. We feel justified in assuming it, wever, in that no project which has tested for school interactions of this sort has tained substantively interpretable differences (Alexander and McDill 1976; Alexander al. 1978; Hauser 1971; Hauser et al. 1976; Heyns 1974).

spondent was in high school, originally coded in 24 income categories and rescaled into dollars by assigning the midpoints of the income categories; and (*e*) a 13-item factor-weighted "acquisition" index of possessions in the respondent's household.

2. Religious background was measured by an item in the 1970 instrument. After extensive experimentation, we decided to use two dummy variables representing a Catholic background and a Jewish background. Because so few respondents from Jewish backgrounds did not enter college, this variable is not employed in the analysis of high school graduates.

3. Academic aptitude was measured with the 20-item test mentioned above, which was administered by the ETS during the 1955 survey.

4. An index of perceived peer college orientations was constructed by summing responses to two items from the 1955 questionnaire, one tapping the college plans of the friend the respondent most liked and the other reporting the proportion of peer associates attending or planning to attend college.

5. Two indicators of the influence of adult significant others were obtained from the 1955 schedule. One item pertained to school personnel and asked, "To what extent have you discussed going to college with the teachers or guidance counselors (advisers) in your school?" Another item, comparably worded, pertained to parents. The response categories for both items were "not at all," "some," and "quite a lot."

6. Educational expectations were ascertained from the 1955 schedule and responses were trichotomized into "college goers," "possible college goers," and "non–college goers."

7. Different indicators of occupational aspiration are employed for the college goers and for the high school graduates, though both are based on the same item from the 1955 survey. For college goers, occupational aspiration is assigned SEI scores. For high school graduates, we employ a dummy variable representing aspiration to a professional or managerial occupation. Preliminary work revealed that for high school graduates the simple distinction between high status aspirations and lesser goals captures the relevant motivational content of such expressed ambitions.

8. Academic self-image is a factor-weighted scale constructed from nine self-evaluation items in the 1970 schedule dealing with high school performance and problems, such as "I usually did a bit more than the teacher required" and "I wasn't able to concentrate on what I read."

9. Senior-year curriculum enrollment, obtained from the 1970 schedule, is measured by two dummy variables, "college curriculum" and "vocational-commercial curriculum."

10. Mathematics and science coursework was ascertained by summing responses to two items contained in the 1970 questionnaire which asked how many semesters of mathematics and science were taken while in high school.

11. Senior class standing is based on a quintile ranking obtained from ırincipals in 1959.

12. Educational attainment was originally reported as certification level highest degree obtained) and subsequently recoded into years of schooling ompleted. There is no variance in the educational attainment for high :hool graduates; hence, it is not employed in the analyses for those men.

13. Academic self-concept in college is a three-item factor-weighted scale. 'he items pertained to perceived difficulty in "learning how to study" and ı "keeping up with my grades" and to "lack of adequate preparation (in igh school)."

14. Grade point average (GPA) in college was obtained from the 1970 :hedule. A 10-category precoded scale was provided with categories rangıg from "A or A+" to "D or less" in half-grade intervals. A substantial umber of college dropouts failed to provide cumulative GPAs, probably ıinking that this item did not pertain to them, but did provide "terminal ɜar" GPAs which also were requested. When nonrespondents reported only ıree semesters or less of college enrollment, the average of freshman-year ıd terminal-year GPAs was used as an approximation for cumulative GPA.

15. An index of the respondent's academic honors in college was calcuted by summing responses to a question in the 1970 instrument asking, Did you ever receive any of the following academic awards or honors as ı undergraduate? [Circle as many as apply]." Nine types of honors or vards were listed, ranging from "Dean's List" and "Phi Beta Kappa" to ;raduation with honors (cum, magna, summa)."

16. College major was taken from the 1970 questionnaire. Individuals ho did not report their major as a "senior," or during their "last year," ɜre assigned their intended freshman-year major. After extensive experientation with a variety of coding schemes, we decided to employ the llowing dummy variables: engineering major and business major. Roughly)% of our college goers reported their major as one of these.

17. College selectivity scores, indexing the quality of the colleges atnded by these respondents, were assigned to the last undergraduate institution attended by the respondent, using Astin's Estimated Selectivity Level dex.[4] In the event that information on terminal-year institution was un'ailable, we used the selectivity of the respondent's first undergrad.te institution. Selectivity is defined as "the total number of highly

:vidence bearing on the assumption that "selectivity" indexes college "quality" may found in Alwin (1974) and Solmon (1975). Based on data presented by Solmon 975), we calculate that the average correlation between selectivity and eight other lege attributes, all of which are presumed to tap institutional quality, is .693. The ht are average SAT verbal scores; average SAT mathematics scores; average facy salary; per student departmental expenditures; basic income per student; basic ›enditures per student; overall Gourman rating; and Gourman academic rating ›lmon 1975, p. 543).

able students who want to enroll at the college divided by the number of freshmen admitted. Thus, the greater the number of these bright students who apply, relative to the number admitted, the more 'selective' the institution can be" (Astin 1965, p. 55). A sizable number of respondents attended either junior colleges or four-year institutions which were not included in Astin's ranking; consequently, selectivity scores could not be directly assigned in these cases. Average aptitude scores within college selectivity categories in our sample were used to determine the placement of these noncoded respondents on the Astin scale. See Alexander and Eckland (1977) for more detail on this estimation procedure.

18. Occupational attainment in 1970 was indexed by the Duncan SEI scale.

19. Annual earnings (as of 1969) were ascertained from the 1970 questionnaire in a manner identical with that discussed above for parental income.

Occasionally we employ other variables for control purposes or to elaborate on a particular discussion. These include regional location and the degree of urbanization of the respondent's 1955 and 1970 residences, status and aptitude compositions of high schools, and several postschooling training activities and work experiences, including annual hours worked. In order to keep a rather complex design and an elaborate model from becoming even more unwieldy, these results are discussed when necessary, but not presented.

RESULTS

School-to-School Differences

Table 1 presents means, standard deviations, and percentages of variance lying between high schools for the variables employed in this analysis. Generally, the amount of between-school variance in these measures is quite comparable to other estimates (e.g., Heyns 1974; Hauser 1971; Hauser et al. 1976; Alexander and McDill 1976).

In table 2, we present estimates of the contributions of schools (included in the regression equations as a set of 41 dummy variables) to occupational status and earnings *for high school graduates*, before and after controlling for the remaining variables in figure 1 (see above). In these analyses, as in all that follow, bivariate data-present subsamples are employed. The results for listwise deleted samples were quite comparable throughout unless otherwise noted.

Roughly 15% of the variance in occupational status is situated between schools (row 1).[5] Controlling for status and religious origins and academic

[5] Unless otherwise noted, figures have not been corrected for loss of degrees of freedom due to the number of school dummy variables used in the analysis (41). Since our

TABLE 1

Means and Standard Deviations of Variables and Proportion of Variance between Schools, by Educational Attainment

Variable	High School Graduates $\bar{X}$	High School Graduates SD	High School Graduates %	College Goers $\bar{X}$	College Goers SD	College Goers %
Mother's education (MOMED)	9 96	2 87	19 1	11.59	2.81	11.5
Father's education (FAED)	8 77	3 12	18 0	11 16	3 80	13.3
Father's occupation (FAOCC)	30 58	18 06	28 4	45 84	22.23	20.5
Parental income (PARINC)	6,681	3,503	26 8	9,428	6,445	13 0
Material possess. (ACQUIS)	– 413	805	39.1	306	.841	21.5
Catholic (CATHOL)	201	.402	32 5	130	337	11.3
Jewish (JEWISH)				.081	.272	37 5
Ability (ABL)	5 70	3 03	14 3	8 86	4 03	11.3
Peers' college plans (PEER)	4 22	1 50	21 2	5 51	1.44	20.5
Parental encouragement for college (PAR)	1 85	656	14 3	2 39	.633	10.8
Contact with teachers (TEACH)	1 44	565	18 9	1.74	.616	14.2
Educational plans (EDEXP)	1 47	71	12 2	2 25	762	12.7
Occupational aspirations (OCCASP)	.453	499	18 7	65 02	23 93	19.5
Academic self-image (IMAGE)	.621	771	14.6	954	785	10.0
Vocational curriculum (VOCCUR)	172	378	11 0	055	.227	8.8
College curriculum (COLLCUR)	.112	315	22 3	607	.489	20.4
Math/science courses (COURSE)	5 45	3 53	19 9	8 97	3 90	20 2
Senior rank (STAND)	1.22	1 12	12 1	2 33	1 32	5.7
Occupational status (OCC)	35 18	19 81	15 0	62 88	20 67	9 3
Earnings (EARN)	9,971	4,550	19 0	11,977	5,255	17.4
Education (EDUC)	.			16.21	1.97	11.7
College self-image (COLIMAGE)			.	249	584	14.0
College GPA (COLGPA)		.		4 75	1 69	12.8
Honors (HONORS)			.	.970	1.462	9.7
Business major (BUSINESS)			.	233	423	7.7
Engineering major (ENGINEER)		.	.	141	.348	14.4
Selectivity (SELECT)				49 76	9.39	17.9

Note —Abbreviations for variables the same in all tables

TABLE 2

GROSS AND NET CONTRIBUTIONS OF SCHOOLS TO VARIANCE IN OCCUPATIONAL STATUS AND EARNINGS: HIGH SCHOOL GRADUATES WITH PAIRWISE DATA PRESENT (N = 338)

	DEPENDENT VARIABLES	
PREDETERMINED VARIABLES	Occupational Status R^2	Earnings R^2
1. None	.150	190
2. MOMED, FAED, FAOCC, PARINC, ACQUIS, CATHOL, ABL	.119	.172
3. MOMED, FAED, FAOCC, PARINC, ACQUIS, CATHOL, ABL, PEER, PAR, TEACH	107	.172
4. MOMED, FAED, FAOCC, PARINC, ACQUIS, CATHOL, ABL, PEER, PAR, TEACH, EDEXP, OCCASP	.105	.169
5. MOMED, FAED, FAOCC, PARINC, ACQUIS, CATHOL, ABL, PEER, PAR, TEACH, EDEXP, OCCASP, COLLCUR, COURSE, STAND, IMAGE, VOCCURR	110	.174
6. 5+OCC	...	.155

aptitude reduces this effect by one-fifth, to 11.9% (row 2); this level is hardly affected by the additional controls introduced in later equations (see rows 3 through 5). Net of a variety of individual level variables, then, including social background, ability, and academic experiences and performances, secondary school differences alone account for an additional 11% of the variance in occupational status attainment for high school graduates. School-to-school differences thus appear quite influential in situating high school graduates in the occupational-status hierarchy.

Even more impressive statistics are observed for annual earnings differentials. Nineteen percent of the variance in this dimension of labor market success lies between schools (row 1). Controlling for the remaining variables has little effect on this estimate of "school effect." Fully 15.5% (or 4.7% after correcting for degrees of freedom) of the variance in the earnings of terminal high school graduates is accounted for by secondary school differences even after controlling for *all* other variables in our model, including occupational achievement. This represents 82% of the entire variance situated between schools. In fact, secondary school differences *alone* account for almost two-thirds of the variance in economic achievement which is explained by *all* of the variables (both within and between school) included in our high school model ($R^2 = .248$).

The data presented in table 2 suggest that even after a period of 15 years high schools have a pronounced impact on the occupational and,

primary purpose is to identify potentially important sources of school influences, adjusted R^2s would be extremely conservative. Nevertheless, these are mentioned occasionally for the benefit of interested readers. The adjusted R^2 here would be 3.2%.

especially, economic successes of those students who did not go on to college. These school effects, moreover, rival in importance achievement-related personal characteristics. It is possible, of course, that our estimates are inflated owing to the neglect of relevant individual-level attributes or community or regional characteristics which are correlated with both secondary school quality and career attainments, but our controls on background characteristics and school experiences are quite extensive. Moreover, additional controls (not reported in tables) for a number of post-school labor market activities—including self-reported sources of job training, annual hours worked and work experience—and for regional differences (i.e., place of residence as measured by dummy variables distinguishing the Northeast, North Central, and Western regions and large towns and metropolitan areas for both 1955 and 1970) reduce the unique contribution of high school differences to occupational status attainments by only about 2% and barely affect the estimates for earnings. Thus, the influence of secondary schools on socioeconomic achievements is not appreciably affected by (1) extraschooling "investments in human capital," (2) the respondent's place of residence in 1970, or (3) either the size of the community or the region of the country in which the respondent's secondary school was located.[6]

In table 3, we present the gross and net contributions of schools to the variance in these labor market outcomes for college goers. Consider first occupational status. Less variance in this dimension of career achievement is situated between schools for college goers than for high school graduates (about 9% and 15%, respectively).[7] Additionally, the *net* between-school variance, as suggested by our high school model, is markedly lower for those students who obtain at least some college education. For example, controls for background characteristics and school-process variables reduce the effect of schools to 4.7% (row 5, table 3). This compares with 11% for high school graduates (row 5, table 2). Controls for the educational attainments of these workers (row 6, table 3), reduce this figure still

[6] Unfortunately, almost no information is available on the actual characteristics of these schools. Hence, we cannot adequately pursue what specific attributes might account for their socioeconomic consequences. We can, however, report that two commonly studied contextual variables, student-body status and ability compositions, account for very little of this between-school attainment variance (about 8% and 16% for earnings and occupational status, respectively). This applies to our results for college goers as well.

[7] One reason for the lesser importance of secondary school differences for the career attainments of college men is due to the fact that high schools contribute modestly to the probability that a student will enter college. We estimate that for the total sample of Explorations in Equality of Opportunity men roughly 13.5% of the variance in college entry is situated between secondary schools. After controlling for the background, ability, and school-process variables presented in fig. 1, however, secondary school differences explain only 2.2% (unadjusted) of the variance in college entry.

TABLE 3

GROSS AND NET CONTRIBUTIONS OF HIGH SCHOOLS TO VARIANCE IN OCCUPATIONAL STATUS AND EARNINGS: COLLEGE GOERS WITH PAIRWISE DATA PRESENT ($N = 525$)

	DEPENDENT VARIABLES	
PREDETERMINED VARIABLES	Occupational Status R^2	Earnings R^2
1. None	.093	.174
2. SES+CATHOL+JEWISH+ABL	.060	.103
3. 2+PEER, PAR, TEACH	054	099
4. 3+EDEXP, OCCASP	.051	.098
5. 4+senior year variables	.047	092
6. 5+EDUC	.033	.092
7. 6+OCC	.	.093
8. 5+college variables	.046	.089
9. 8+EDUC	.031	089
10. 9+OCC		.089

further, to 3.3% (or 0% after adjustments for loss of degrees of freedom). Thus, educational attainment, which itself is moderately affected by school differences (see n. 7), is one of the mechanisms by which schools affect occupational status. Further controls for postsecondary educational experiences, including college selectivity, do not reduce this "net" estimate appreciably (row 9, table 3). High schools, then, do not appear to be markedly influential in the occupational-status achievements of college goers, regardless of whether postsecondary educational experiences are controlled.

Annual earnings, on the other hand, are much more sensitive to school differences, even for these relatively highly educated workers. About 17.4% (10.4% adjusted for degrees of freedom) of the variance in earnings of college goers is situated between high schools (row 1, table 3), but this is reduced to slightly over 10% after controlling only for status and religious background and academic aptitude. Additional controls for the school process variables (rows 3–5, table 3), years of schooling completed (row 6, table 3), and occupational status (row 7, table 3) further reduce the net between-school variance only slightly, to 9.3% (or 2.8% after adjustments for loss of degrees of freedom). Secondary school differences, then, do appear to be modestly important in creating inequalities in the economic attainments of workers with at least some college education, even after controlling for various college characteristics, experiences, and attainments (row 10, table 3). Finally, controlling for the respondent's 1955 and 1970 place of residence and postschooling job experiences discussed above scarcely affects (by less than 0.5%) this estimate of the net effect of secondary schools, a finding already observed for high school graduates.

We hypothesized above that socioeconomic consequences of secondary

school differences should be more pronounced for workers who terminate their formal schooling with high school graduation. The data presented in tables 2 and 3 support this proposition. Moreover, since college experiences are largely independent of high school differences, these differential secondary school effects do not reflect merely the differences among secondary schools in the likelihood of their graduates' attending more or less selective colleges or of their having qualitatively different experiences and attainments in college.

Within-School Effects

Just as with the between-school analysis, the within-school results, though somewhat mixed, are generally consistent with our expectations. Tables 4, 5, and 6 present the within-school regression slopes for the two student groups.[8] Only aspirations for a high-status white-collar job and scientific and mathematics coursework affect positively the positioning of high school graduates in the occupational status hierarchy (table 4, top).[9]

The positive consequences of career ambition are not particularly surprising (e.g., Sewell and Hauser 1975). That courses in mathematics and science enhance occupational achievement may, however, shed some light on how schooling actually affects adult labor market success. Employers are unlikely to know about such coursework—except perhaps indirectly through educational certification, which is constant for these men, or secondary school differences, which are statistically controlled—and, therefore, are unlikely to reward job incumbents simply for having taken such courses. It does appear, then, that at least these school experiences enhance job-related skills *or* attitudes, a finding more consistent with the "productivity" (e.g., Mincer 1974) or "attitudinal" (e.g., Bowles and Gintis 1976; Gintis 1971) hypotheses than with the "screening" interpretation of school and schooling effects on market success (e.g., Berg 1970). We also note that neither occupational aspirations nor coursework affects the occupa-

[8] We considered the possibility that, for college goers, these variables interacted with college dropout versus college graduate status. Neither the overall increment to R^2 nor any single interaction term was significant at the .05 level. This was observed whether or not we controlled for secondary school differences. For this test, we analyzed those respondents for whom we had complete data on all variables included in the analysis.

[9] The only respect in which the results from listwise-present and pairwise-present subsamples are not comparable involves the effect of vocational curriculum on annual earnings of high school graduates. As table 4 shows, the analysis based on pairwise-present data suggests that this tracking variable exerts no direct influence on occupational status or earnings. Analysis with the listwise-present subsample ($N = 131$) indicates, however, that vocational curriculum enrollment does enhance earnings (over $1,000 annually). Thus, for a very select group of terminal high school graduates, this dimension of academic differentiation appears relevant for career achievements. The ambiguity in our results, therefore, precludes our drawing any conclusions regarding the socioeconomic consequences of vocational tracking.

TABLE 4

WITHIN-HIGH SCHOOL REGRESSION MODEL OF OCCUPATIONAL STATUS AND EARNINGS: HIGH SCHOOL GRADUATES WITH PAIRWISE DATA PRESENT (N = 338)

EQUATION	PREDETERMINED VARIABLES																		
	MOMED	FAED	FAOCC	PARINC	ACQUIS	CATHOL	ABL	PEER	PAR	TEACH	EDEXP	OCCASP	COLLCUR	COURSE	STAND	IMAGE	VOCCUR	OCC	R^2
	Occupational Status																		
(1)....	151	394	.093	− 006	−1 29	3 17	578									...	...	...	.034
(2)....	072	377	080	− 006	−1 70	4 06	426	1 23	2 18	1 26					...	...	...	...	060
(3)....	− 021	.571	032	− 006	−1 78	3 92	396	994	1 21	1 42	− 127	7 88*			...	...	...	...	.101
(4)....	− 041	539	028	− 004	−2 00	4 51	025	736	567	1 56	− 276	6 44*	−3 13	811*	622	817	−3.74	...	.125
	Earnings																		
(1)....	26	22	−14	030	−82	80	−8								...	...	...	...	.004
(2)....	20	18	−15	020	−137	222	−23	52	202	749					...	...	...	...	.020
(3)....	15	27	−17	032	−155	215	−14	74	256	799*	−354	515			...	...	...	...	.025
(4)....	27	17	−13	022	−58	288	−54	26	153	849*	−271	468	−2,199*	82	380	152	−158	...	.052
(5)....	28	−2	−17	042	24	201	−63	−15	107	827*	−233	224	−2,165*	52	387	126	−72	32*	.072

* Coefficient at least twice its standard error.

tional success of college-going men (see table 5), thus supporting, in part, our thesis that high school–based factors are more important for high school graduates.

Turning next to the earnings of high school graduates (table 4, bottom), the only significant determinants of economic attainments are teacher contact, high school academic track,[10] and occupational status (class rank, registering a standardized effect of 0.099, falls just short of conventional levels of statistical significance). While the item tapping teacher and counselor influence asked specifically about "going to college," it might possibly index more general teacher and counselor encouragement and influences as

TABLE 5

WITHIN-HIGH SCHOOL REGRESSION MODEL OF OCCUPATIONAL STATUS: COLLEGE GOERS WITH PAIRWISE DATA PRESENT (N = 525)

PREDETERMINED VARIABLES	EQUATION						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MOMED	−.742	− 805	− 808	− 789	− 741	− 710	− 678
FAED	933*	878*	.837*	.823*	.792*	.642	.701*
FAOCC	.061	051	049	060	.039	061	041
PARINC	−.000	− 000	− 000	− 000	− 000	− 000	− 000
ACQUIS	.327	− 193	−.283	− 526	− 345	009	−.082
CATHOL	−2.42	−1.89	−1 83	− 501	−1.72	− 441	−1 67
JEWISH	5.09	3 76	3 83	3 91	1 17	3 75	1 44
ABL	898*	855*	816*	112	−.163	− 091	− 212
PEER		1 22	.961	.831	469	442	284
PAR		1.59	1.18	.390	547	882	885
TEACH		.532	341	.183	−.003	586	.369
EDEXP			1 45	1 01	622	1 19	806
OCCASP			017	− 018	− 022	− 010	− 014
COURSE				.567	552	.523	512
STAND				2.917*	1 85	2 13*	1 51
VOCCUR				−.835	− 114	−2.04	− 781
IMAGE				− 303	− 717	− 285	− 382
COLCURR				2 75	1 80	2 46	1 73
EDUC					3 04*		2 88*
COLIMAGE						−2 09	−2 15
COLGPA						2 48*	1 91*
BUSINESS						−.845	1 75
ENGINEER						651	2 84
HONORS						− 207	− 924
SELECT						219	143
R^2	.094	.106	109	.164	.228	201	246

* Coefficient at least twice its standard error.

[10] We have no satisfactory explanation for the significant negative earnings returns to college tracking for our high school graduates, and we know of no other study which has examined the effect of this variable for youth who did not subsequently enter college. Taken at face value, the coefficient suggests that, for youth who do not go on to college, the skills or attitudes learned in a college curriculum are actually negatively evaluated by employers, a finding not inconsistent with the thesis proffered by Bowles and Gintis (1976) concerning the attitudinal prerequisites necessary for work in lower-status occupations.

well. Interestingly, it is not teacher influences but parental support which affects the subsequent earnings achievements of college goers (see table 6). For high school graduates, on the other hand, parental influences are trivial (table 4). Again, we must note that while the question measuring parental encouragement referred to educational outcomes, its importance here may reflect other kinds of influences or motivations not measured directly.

We think it important that for college goers parents are important sources of interpersonal influence, while for high school graduates it is school personnel who predominate. This, of course, is consistent with our assumption that secondary school influences are more consequential in the socioeconomic careers of high school graduates than in the careers of college goers. We note also the large differences, in the expected direction, in the economic benefits of high school academic performance (STAND) for the two groups. Other notable differences between the two groups are the smaller economic benefits accruing to occupational rank and the more important influences of status origin (especially parental income) and religious background on annual earnings among college goers. Only for college goers, then, does there appear to be any appreciable "inheritance" of earnings and other status advantages and liabilities.

Such intergenerational transmission of parental income or wealth was observed earlier in these (Alexander et al. 1975; Griffin 1976) and the Wisconsin data (Sewell and Hauser 1975; Alwin 1976*b*) and has been the subject of much conjecture (Bowles 1972), but the finding has yet to be explained in a statistical sense. The data presented in table 6 offer at least a partial explanation. Controls for postsecondary educational experiences reduce the parental income coefficient by about a fourth, from .098 to .077 (cf. col. 9 with cols. 5 and 7). Youth from high-income families tend to enroll in more select colleges and are somewhat more likely to pursue a business-related major (perhaps a proxy for income aspirations—see Freeman [1976]) while in college (not shown). College selectivity and college major are thus the only appreciable mediators of parental income effects identified in our model. Alwin (1976*b*; Alwin et al. 1975) also found college differences to account for a small amount (6%) of the impact of parental income in the Wisconsin data. Our analysis reproduces this result and suggests as well that the acquisition of job-related skills in college, through one's college major, may also serve to maintain socioeconomic inequalities across generations, albeit only to a modest degree (while the structural estimate of parental income is not significantly different from zero, this, of course, is partly a function of our relatively small sample size).

We consider, finally, the impact of level of education and college experiences on socioeconomic achievement among college goers. Each year of education yields a status payoff of about 2.9 points (table 5, col. 7) and is worth $111 annually (table 6, col. 6), of which about $91 (or 82%) is

TABLE 6

WITHIN-HIGH SCHOOL REGRESSION MODEL OF EARNINGS: COLLEGE GOERS WITH PAIRWISE DATA PRESENT ($N = 525$)

VARIABLES	EQUATION (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
MOMED	64	58	57	66	96	97	101	67	76
FAED	75	57	67	60	54	56	51	60	47
FAOCC	−14	−17	−16	−14	−16	−18	−18	−14	−15
PARINC	.001	088	090	094*	075	075	077	.094*	.098*
ACQUIS	366	225	245	245	278	274	275	245	249
CATHOL	765	916	893	1,085	1,040	997	1,009	1,082	1,110
JEWISH	2,738*	2,692*	2,680*	2,661*	2,916*	2,816*	2,796*	2,651*	2,612*
ABL	34	20	30	−7	−22	−26	−25	−8	−4
PEER	...	14	76	63	8	− 368	−4	61	50
PAR	...	1,141*	1,239*	1,253*	1,309*	1,310*	1,304*	1,253*	1,245*
TEACH	...	−169	−124	−108	49	47	51	−109	−96
EDEXP	...	...	−325	−306	−338	−353	−357	−307	−315
OCCASP	...	...	−5	−9	−7	−7	−7	−9	−9
COURSE	...	...	...	99	75	74	70	99	89
STAND	...	...	...	181	66	44	37	178	157
VOCCUR	...	...	...	211	30	82	91	214	227
IMAGE	...	...	...	−128	−158	−161	−159	−129	−119
COLCURR	...	...	...	−380	−259	−289	−301	−384	−410
COLIMAGE	...	...	...	...	−16	−22	−11	...	...
COLGPA	...	...	...	...	321	299	288	...	...
BUSINESS	...	...	...	...	2,168*	2,262*	2,243*	...	...
ENGINEER	...	...	...	...	2,548*	2,629*	2,606*	...	...
HONORS	...	...	...	...	−38	−65	−58	...	...
SELECT	...	...	...	...	42	38	38	...	...
EDUC	...	...	...	...	...	111	91	9	−38
OCC	...	...	...	...	...	...	6	...	14
R^2	059	.079	081	089	150	151	.152	.089	.091

* Coefficient at least twice its standard error.

the direct effect of educational attainment within levels of occupational status (col. 7).[11] This statistic increases to over $200 annually after we control for a variety of labor market experiences and training activities (not shown), most of which are negatively correlated with educational attainment (the longer one stays in school the less opportunity he has to acquire market experience [Griffin 1978]). College selectivity exerts only minor influences on occupational status (table 5, col. 6) and earnings (table 6, col. 7), even in the absence of controls for secondary school differences (not shown). Data limitations preclude our evaluating the effects of any other college characteristics, and, because of this, we are probably underestimating the importance of all college differences (e.g., Alwin 1976*b*; Solmon and Wachtel 1975).

Although college selectivity has little impact on the labor market outcomes for these men, some college experiences and outcomes do facilitate occupational and economic attainments. Academic performance in college, for example, significantly affects occupational status (table 5, col. 7), while majoring in business or engineering yields substantial earnings returns, net of occupational position. Independent of a multiplicity of other factors, including secondary school differences and college "quality" (as measured by selectivity), these postsecondary schooling mechanisms thus affect the placement of men in high-status jobs and their subsequent economic attainments.

SUMMARY AND CONCLUSIONS

Advanced capitalist societies, characterized as they are by a meritocratic state ideology, a knowledge-intensive market economy, the ethos of productivity and efficiency, and structured social inequality, place extensive and possibly contradictory demands upon their formal educational institutions. Schools are chartered not only to socialize youth into general adult roles but also to identify and subsequently train adolescents for what are thought to be appropriate careers. Additionally, schools are one—and possibly the most important—arena in which economically handicapped youth are thought to acquire the cognitive and noncognitive skills necessary for them to escape their disadvantaged status origins (Coleman et al. 1966). Ideally, then, the social organization of schooling is so structured as to facilitate individual achievement and simultaneously ensure that late capitalism's social, political, and economic requirements are suitably met. Hence the institutional missions of formal education are wide ranging and perhaps mutually antagonistic.

[11] We evaluated the possibility that educational attainment influences occupational status and earnings nonlinearly by employing a series of categorical variables representing levels of post–high school educational certification. We found little evidence of such nonlinear effects.

Thus, to the extent that any one objective of schooling assumes paramount importance, other institutional goals may be slighted or actively subverted. Marxists believe that schools are not neutral agencies in the clashes among different status and economic groups but are, in fact, vehicles to be used by the dominant classes to reproduce the prevailing capitalist order (Bowles 1972, 1977; Bowles and Gintis 1973, 1976). Late capitalism, it is argued, requires a well-disciplined, motivated, and productive labor force, and the social relations of education are assumed to develop precisely these qualities in students. Hence, schools do not "liberate" their clients from their pasts (the goal of social mobility) but prepare them to assume their "proper" place in society's stratification system. Surely, though, not all strategies of school organization are equally efficacious for discharging these prescribed missions; yet, as we noted early in this paper, almost two decades of cross-disciplinary research have failed to produce compelling evidence that schools actually perform the ends attributed to them by either Marxists or functionalists.

We have argued in this paper, however, that limitations of the accumulated literature on schooling and social stratification are such that it would be premature to draw firm conclusions on these matters. Four major problem areas were identified in the existing literature: (1) overreliance on purely quantitative indices of exposure to schooling (i.e., years of schooling completed); (2) the student outcomes typically evaluated (e.g., test performance); (3) the nearly exclusive concern with school-to-school differences and the attendant paucity of research on the impact of within-school resources; and (4) the use of undifferentiated student populations, a practice which may obscure the importance of schooling for adult socioeconomic success and in transmitting inequality from generation to generation.

We have attempted to redress some of these deficiencies in assessing school effects on socioeconomic inequality. We conclude that qualitative variations in the educational process, deriving both from differences between schools and from allocative mechanisms and educational experiences within them, do, in fact, have important socioeconomic consequences for men early in their labor force careers and that such school effects may be either more pronounced or less so for selected groups of students. The finding that the socioeconomic success of terminal high school graduates is considerably more responsive to characteristics of their secondary schools and to experiences therein than is that of college goers may have especially important implications both for our appreciation of how school organization affects stratification outcomes and, more generally, for the conduct of future school effects research. Our having drawn successfully upon the implications of widely accepted theories from labor economics to identify outcomes and youth particularly responsive to variations in school organization and experiences suggests that the search for "usually effective schools" (Klitgaard

and Hall 1975) might profitably be complemented by theoretically informed searches for especially sensitive student populations. The customary conduct of school-effects research, in implicitly assuming uniformity of impact, may actually mask quite substantial consequences of variations in school organization and quality for selected students. Thus, even the generally negative conclusions reviewed earlier regarding school impact on subjective outcomes, academic performance, and educational attainment may merit reconsideration.

We see at least four further implications of this study. First, arguments which purport to explain that portion of the inequality in earnings not accounted for by traditional "status attainment" variables (e.g., social origins, ability, educational attainment, occupational status) by the use of such nebulous and unsatisfactory concepts as "luck" (e.g., Jencks et al. 1972) are, at least in part, incorrect. For both groups of men studied here, high school differences are quite important in explaining their differential economic achievements.[12] This is not to imply that random disturbances are of no consequence; indeed, they are, especially for men early in their socioeconomic careers (e.g., Mincer 1974). Nor are we suggesting that career attainments, especially earnings, are *predominantly* determined by educational experiences and certification, for clearly most of the variance in earnings is orthogonal to the secondary and postsecondary schooling processes measured here. Hence, our results suggest that both Marxists and functionalists may overstate the linkage between educational and social stratification. We *are* arguing, however, that researchers should assess the impact on career attainments of a variety of qualitative variations in the educational experience (not to mention labor market conditions and experiences) before resorting to the use of such catchalls as "luck" or "personality."

Second, Duncan's argument in behalf of simple quantitative measures of educational attainment ("Inferior [school] quality at any one level of the school system is likely to result in impaired chances of proceeding to the next level. . . . Hence, school years completed has partly built into it a correlation with quality" [1969, p. 104]), while plausible and undoubtedly correct, cannot justify the exclusion of direct measures of school "quality" in earnings functions. Although the quantity and quality of schooling are correlated, they are conceptually and empirically distinct. The consequences of this observation are twofold. On the one hand, to exclude school-to-school differences from achievement models results in underestimating the influence of schooling in its most general sense. For example, we found for college

[12] We must note that our estimates of school effects are probably biased upward due to the differential reliability of the variables included in this analysis. "Schools" are almost perfectly measured, while our other variables are undoubtedly subject to some degree of error (Bielby, Hauser, and Featherman 1976), thus inflating estimates for "schools" vis-à-vis the remaining variables. Unfortunately, for most variables we lack the reliability and validity information necessary to correct for attenuation.

goers that, net of years of schooling completed, high school differences registered an important impact on earnings. On the other hand, to assume that educational attainment is a proxy for school quality, and thus to exclude school differences from earnings functions for this reason, may lead to severe biases in our estimates of the efficacy of individual-level resources. Our data suggest, for example, that secondary school differences are of crucial importance for correctly estimating the effects of social or religious backgrounds on adult attainment, since these variables are more strongly correlated with such differences than are within-school resources.[13]

Third, insofar as schools and schooling mechanisms are implicated in the reproduction of socioeconomic inequality from generation to generation (e.g., Bowles 1972, 1977; Bowles and Gintis 1976; Gintis 1971), attention should be devoted to postsecondary educational institutions and to the differential access to and utilization of resources located within them. Our results suggest that these mechanisms both enhance career achievements and transmit some of the benefits associated with advantaged social origins.

Fourth, our finding that educational certification (i.e., level of schooling) interacts with several other variables in our analysis suggests that socioeconomic achievement processes may not be uniform even for an age-homogeneous sample of white men (racial and gender differences in these processes, of course, are well documented [e.g., Treiman and Terrell 1975]). We have discussed the implications of only one such possible complication, but achievement processes may differ also by class position (Wright and Perrone 1977), labor market sector (Gordon 1972), occupation (Stolzenberg 1975), and other social aggregates. Such differences must be more thoroughly understood before we can generalize securely about the processes governing socioeconomic success.

Finally, in assessing our own effort, we believe this inquiry important not only for the conclusions we have offered but also, and perhaps more significantly, in its framing an agenda for exploring (1) the impact of school differences and within-school processes on the labor market careers of men (and, it is to be hoped, women too in future research) differing in levels of educational certification and (2) the function of such diverse educational experiences in reproducing social inequalities over time. Certainly our analysis is not unflawed. The small sizes of our samples, their known biases, the relatively few schools represented in our data, the crudity of many of our measures and the unavailability of others that might have been desired, all dictate that our results be used cautiously and be considered

[13] We estimate, for example, that for high school graduates the omission of high schools from earning functions is responsible for the following upward biases: mother's education (67%), parental income (60%), and material possessions (95%). For college goers we observe the following biases: material possessions (45%), Catholic background (47%), Jewish background (29%), coursework (49%), and educational attainment (21%).

tentative. We therefore urge other researchers to collect and/or analyze better data to either support or disprove our conclusions. Our intention in this paper has been not to provide definitive answers but to provide enough empirical evidence to stimulate the interest and thought of others and to avoid premature closure on relationships among social origins, "schools," and socioeconomic inequality in American society.

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Artwork: Collection and Contemporary Culture[1]

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The definitional transformation of film from industrial commodity to art form that took place in the United States during the 1950s represents a more general change in the treatment of industrial commodities, as more outmoded goods have become "collectibles." This pattern has consequences which should be important to sociologists of culture since collection keeps pieces of material culture in the environment longer, perhaps to be used as design precedents for future objects.

During the 1950s critics in the United States began to write about film as an art form. Their ideas were not particularly new since intellectuals in Eastern Europe, France, Italy, and Japan had written in a similar vein around the turn of the century (Liehm and Liehm 1977; Robinson 1973; Leprohon [1966] 1972; Richie 1971) and such notions had been widely accepted throughout Western Europe since the 1920s (Robinson 1973; Lawder 1975). What is interesting is that Americans had been particularly resistant to this kind of thinking until the post–World War II era (Hampton [1931] 1970; Crowther 1957; Jowett 1976); yet by the 1960s a majority of U.S. cognoscenti found themselves embarrassed by those who continued to think of film as an exclusively popular entertainment.

One could argue that American anti-intellectualism (Hofstader 1963) had once more desensitized Americans to the obvious artistic value of a medium and that the redefinition of film as an art form was rectification of an error. But that would be misleading. The literature on film as art that grew out of the 1950s certainly made this kind of argument, but it was a literature of aesthetic advocacy, not social science. In many countries, including India and Egypt as well as the United States, people simply did not consider film to be an art form (Barnouw and Krishnaswamy 1963; Tun stall 1977; Sadoul 1966). Early elite audiences regarded film as a threat to traditional art and so dismissed it as commercial culture created by upwardly mobile and ambitious people with no training or interest in art traditions. Films were seen (as they sometimes are today) as pap fed to the masses, industrial commodities produced to make industrialists rich by exploiting people's desires for both entertainment and escape from the

[1] The author would like to thank Bennett Berger, Cesar Grana, Joseph Gusfield, and David Phillips for their criticism of and support for this work.

burden of their daily lives (Indian Cinematograph Committee 1928; Schiller 1976; Jowett 1976).

The definitional transformation of film from industrial commodity to art form should be of particular interest to sociologists, who rarely associate art with products of industrial manufacture. According to stereotype and social theory, industrial goods are considered important for shaping the contemporary economy and social relations of production (see, for instance, Baran and Sweezy 1966; Blauner 1964; Feuer 1959; Smelser 1959; Rostow 1963; Heilbroner 1962; Thompson 1963). Few sociologists attribute any significance to the cultural meaning or design of industrial commodities, two notable exceptions being Veblen ([1899] 1953) and Braverman (1974). Art, on the other hand, is significant to scholars because it embodies cultural symbolism and allows those with imaginative gifts to find lasting meaning in transient events (see, for example, Dewey 1934; D. Bell 1976; Rosenberg and Fliegal 1965; Lukács 1964; Solomon 1974). This view of art discourages sociologists from discussing "commercial" culture (popular culture, mass culture) in the same context as artworks (for exceptions, see Gans 1975; Becker 1978). Most consider the aesthetic differences between artworks and other objects more important than any similarities there might be between them (see, for example, Grana [1962] 1964; D. Bell 1976).

In this paper I hope to demonstrate some virtues in studying the continuities between fine art and commercial culture. Right now our understanding of the arts tends to be limited to what can be learned by comparing one medium to another (for instance, how films are like novels or how paintings are like prints). The result is that we know relatively little about how the products of the film industry, for example, are like the products of other industries. The tendency to conceive of plastic combs in people's pockets and paintings in museums as completely unrelated kinds of objects obscures an important connection between the two: both are designed to have cultural meanings and social uses. By recognizing and clarifying this and other shared characteristics of artworks, craft goods, and industrial commodities, sociologists can begin to use concepts and models from the sociology of art to analyze broader patterns of material culture, and vice versa.

The redefinition of U.S. films provides an opportunity to take a fresh look at this continuity between industrial commodities and art. The case of film appears to be one of many where an industrial commodity has become "worth saving" just as it was threatened with extinction. In the 1950s, when the definition of film as art gained currency, U.S. filmmakers faced enormous economic difficulties, and it seemed the industry itself might collapse.

Since that time diverse industrial commodities have increasingly been collected by museums as art (for example, posters, furniture, and other objects whose aesthetic qualities are particularly apparent) and by the general public as collectibles or "nostalgia pieces" (mostly everyday practical goods like Depression glassware and old Coke bottles, which have both sentimental and documentary value).[2] If, as seems likely, this revaluation of old industrial goods constitutes a pattern that may be generalized to describe other commodities, then it seems important to understand this process. It may be that people are finding lasting value in the transient processes and commonplace objects that characterized the early industrial era.

To explore this possibility a few important topics have to be discussed. First, it is necessary to understand something about the design of commodities, including how they are designed for market. Second, it is important to study patterns of everyday collection, what makes objects worth preserving, and how people go about saving them from neglect. One can then examine art to see how it acts as the major and prototypical collection system for preserving cultural objects. With this background both the problems faced by film-as-art advocates and the bases for their success appear part of a larger pattern of revaluating industrial goods.

DESIGN IN PRODUCTION

Workers who make objects of any sort do not randomly design and produce works; they carry with them cultural assumptions about how chairs, cars, paintings, or TV ads should look and function (Becker 1974, 1978). Although it may seem odd to think of plumbers and electricians as culture producers, they act like artists when they organize their work around conventional conceptions of how plumbing and wiring should be done (based in part on building code regulations but also on occupational norms). Landscape architects planting trees around a new house, people producing wallpaper and fabric designs, and storeowners displaying their wares in shop windows all share one common design problem: how to work from precedent to design an object that is similar enough to objects that are already legitimated as gardens, wallpaper, or window displays to suggest what kind of object it is supposed to be.

The human habit of transforming raw materials into objects is possible because people can imagine a product from their labors that is "worth making" and strive to achieve that goal. The cognitive skill behind production has been called many names. Braverman (1974) talks about the

[2] The growing public interest in collectibles is reflected in a *New Yorker* (July 4, 1977) piece that describes a meeting of advisors to the *American Heritage Dictionary* where the proper spelling of collectibles/collectables was debated.

"power of conceptual thought"; others speak of the human capacity to design (Papanek 1972) or previsualization (Mukerji 1977; White 1968). The terms are less important than the capacity they describe; people can design objects they want to make because they can preconceive them.

Most of the time people use this skill not to introduce radical kinds of goods to the world but to make new versions of familiar objects. To do this they base some of their designs on precedents or design features of previously produced work essential to their identification and use.

Design precedents may last so long that even those who produce goods can be quite unaware of their designs' historical roots. Objects such as chairs have had consistent design features over such a long period of history that many modern U.S. chairs resemble European medieval ones. Some designs even remain relatively stable through changes in modes of production. People in the United States going to discount stores can buy mass-produced reproductions of Victorian chairs which were originally designed to be handmade. Artifacts of the cultural past are brought into the present through the use of design precedents, not necessarily because designers revere the past in some abstract way, but because they find some historical design features useful for their own contemporary purposes. Moreover, this is as true of those who design industrial commodities as of those who make craft goods or who attempt to produce art.

COMMODITY DESIGN FOR MARKET

Part of any design conception includes anticipating the work's economic value. Just as people use precedents to determine what features make objects recognizable, they consider how to design objects to give them economic value. They do this by examining how similar objects have done on the market before. Goods made by both artisans and industry are produced to please large numbers of people.[3] People producing these commodities compare the costs of making objects to their anticipated returns from sales.

This economic logic is both a rough description of a more complex process and an incomplete picture of commodity design. To make a salable coffee cup, potters or industrial designers must make one that feels good in the hand, holds a hot liquid without disintegrating, does not fall over when set on a table, and looks nice to people. The economic value of goods depends on their design, and there is some evidence of this in the early industrial revolution. "The great revolution of capital importance was that of printed calico. The combined effort of science and art was needed to

[3] Artisans such as carpenters, masons, and plumbers, who contract jobs, apparently only have to please individual clients. More realistically, each job is a public showpiece because it may be used as evidence of the artisan's work, either attracting or repelling customers.

force a difficult and unpromising material, cotton, to bear every day so many brilliant transformations and then, when it was thus transformed, to distribute it everywhere and to put it within range of the poor" (Cipolla 1973, pp. 63–64). Veblen presents a less glowing report of the tastes of these times and how objects had to be designed to be salable: "It is notorious that in their selection of serviceable goods in the retail market, purchasers are guided more by the finish and workmanship of the goods than by any marks of substantial serviceability. Goods, in order to sell, must have some appreciable amount of labor spent in giving them the marks of decent expensiveness, in addition to what goes to give them efficiency for the material use which they are to serve" (Veblen [1899] 1953, p. 113). It may be of crucial importance to commodity designers to consider how goods made from their preconceptions will be produced and marketed, but it is also important to them to design objects consumers will find both recognizable and appealing.

COLLECTION

People not only make things, they look at them, talk about them, develop opinions about them, buy them, sell them, use them, throw them away, break them, or put them away in the attic, museum, garage, or basement. In other words, people live with what they (or others) produce.

There are probably a limited number of things that people do with objects. Among them, collecting goods may have the greatest consequences for material culture. Not only are "saved" works kept in the cultural environment for a longer period of time than those that are broken and/or thrown away, but they are also made available for future generations to use as design precedents. Grandma's hat in the attic may not seem like much until a similar hat appears in the fashion section of a newspaper as the latest design from Paris being picked up by U.S. designers. This is not to say that all the things people save will become important to future designs, only that objects not collected will not be available for future use.[4]

Commodity design (as described in the previous section) does not concern itself with collection. The greatest economic value of commodities is supposed to be their initial sale, not their future resale value. Most commodities are expected to depreciate in value over time.[5] That is why it may be surprising to find so many Americans now collecting nostalgia pieces and other industrially produced collectibles. These objects, which once would

[4] For a discussion of this problem in research on fashion, see Q. Bell (1976).

[5] It was suggested to me that houses were an exception to this rule. I doubt that is the case. When houses appreciate in value, it is usually the result of rising property values On the other hand, there are some commonly collected items which do appreciate in value over time—stamps and coins, for example.

have been thrown away, have developed new value (and markets) as collectors' items because so many of their counterparts have disappeared, making them relatively scare. Collectibles seem to be commodities on the brink of extinction, saved by people who saw in them some lasting importance.

LASTING DESIGN VALUES

The process of collecting goods is filled with value judgments. It is quite obvious that people decide whether or not they like an object before deciding to keep it. What is less obvious is the complexity of this process. What makes an object worth making is not necessarily what makes it worth saving; as a result, collecting often involves a reassessment of the object to identify those features that might make it worth having in the future.

There are many examples of collectors' items that have been given new significance in being redefined for collection. For instance, lots of people save old tire innertubes to use as water toys, investing these objects with a practical value that was not envisioned by their designers. Similarly, parents often save their children's drawings for sentimental reasons that were not of concern to the children at the time the drawings were made. Anthropologists collect potsherds as evidence for their research, giving these scraps of clay academic significance. Finally, collectors of primitive art make gallery shows out of works produced for practical or sacred purposes. I use all these examples to suggest that the redefinition of objects that accompanies their collection is a widespread pattern that may depend on practical, sentimental, and documentary values as well as aesthetic ones.

Not only do people quite literally save objects in collections, they also save some design features of old versions of goods by adapting them in their designs for new ones. Based on notions about the practical value of traditional tool design, contemporary mass-produced hammers, saws, and screwdrivers look very much like their handmade turn-of-the-century ancestors. Based on the notion that family relationships can give objects lasting sentimental value, the people who make Hallmark cards produce "keepsakes" for relatives to give one another for important family occasions such as Christmas, birthdays, graduation, anniversaries, and weddings. Finally, people use ideas about aesthetics and the history of art to produce contemporary works with identifiable relationships to meritorious precedents.

Whether people save whole objects or only aspects of their designs, and whether they save them for sentimental, documentary, practical, or aesthetic reasons, they render these design features available to future designers.

Where objects or design features are valued for reasons not anticipated by their producers, two sets of precedents may be drawn from the same work. Some features of pot fragments in museums may interest potters, and others may interest anthropologists. The way in which an object is redefined when it is collected, then, may influence the ways in which future generations will attend to its design. All collected designs may go through a special evaluation procedure before collection, and the "lasting value" of a given design may vary according to the nature of this evaluation.

ARTWORK

The work of art seems to be a prototypical collectors' item[6] in that artwork, as a class of objects, developed with the emergence of collectors in Renaissance Italy.

> The collector and the artist working independently of the customer are historically correlative; in the course of the Renaissance they appear simultaneously and side by side The change does not, however, occur all at once; it represents a long process The art of the early Renaissance still bears an, on the whole, workmanlike character, varying according to the nature of the commission, so that the starting-point of production is to be found mostly not in the creative urge, the subjective self-expression and spontaneous inspiration of the artist, but in the task set by the customer The market is, therefore, not yet determined by the supply but by the demand . . . [Artists] tried to emancipate themselves from these restraints upon their freedom as soon as conditions in the art market allowed. This occurred when the place of the mere consumer of art was taken by the amateur, the connoisseur, and the collector—that is, by that modern type of consumer who no longer orders what he needs but buys what is offered [Hauser 1951, pp. 40–41]

If we believe Hauser's (1951) argument,[7] a fruitful way to understand art would be to examine art collection.

Contemporary art markets still depend on collectors. But there are two major changes in collection patterns from the ones Hauser (1951) describes. Today, institutions (primarily museums, but also some foundations and businesses) have become more important to the art market than in-

[6] This notion of art as collector's item may appear to be undermined by conceptual art, but it also characterizes this objectless art form when documentation of the conceptual piece becomes a collector's item.

[7] Gombrich (1963) has written a strong piece of criticism on Hauser's work that questions, among other things, Hauser's accuracy in describing the financial transactions of artists in the Renaissance. While he identifies inaccuracies, he does not refute the argument that Hauser advances. He appears simply offended by the assumptions behind social histories of art, feeling that institutional patterns are not as uniform as Hauser might suggest and that these patterns could never account for what is interesting about art—aesthetics.

dividual collectors, and most collectors invest in art when they collect it.

In the present system, artists seem to have lost many of the advantages they gained in the Renaissance by developing a distinctive role; most of them cannot make a living from the sale of their work (see Christopherson 1977; Adler 1975; Becker 1974, 1976), and many have lost much of their autonomy by taking jobs in universities or other bureaucracies (see Adler 1975; Rosenblum 1973; Mukerji 1977). Again, in the present system, artworks tend to have relatively little market value when artists first sell them, but they can gain tremendous value through collection and resale.

The work of art gains much of its prestige and economic meaning because elites invest in it when they collect it and have the power to manipulate the value of their collections by making the identification of art problematic and limiting membership in that category of objects (see Zolberg 1974; Christopherson 1977; Sears 1941).

It is in the interest of those who profit from art investments to encourage the use of esoteric aesthetic systems in evaluating art. Such criteria help to legitimate a scarcity of "great art," while technical evaluations help to protect the autonomy of the art world. If people with fewer stakes in maintaining the scarcity of good art could make judgments about the quality of art, the economy of the art world could be upset. For example, a group of museum goers might consider a modern, abstract painting to be a piece of garbage, but the economic value of the work would not be affected since these judgments would not have the weight of those made with technical language by "insiders."

The autonomy of the art world in some ways parallels the autonomy of the professions (see Freidson 1974). Just as professionals gain much of their prestige because they are able to judge their professional performance and control recruitment of new members to the professions, major art collectors gain prestige for their collections by choosing the reigning aesthetic theories and helping to control the kinds of work that gain institutional legitimacy.

Art maintains its distinctiveness through the efforts of collectors to protect their investments. They support a distinctive market for artworks by singling out some goods as collectors' investments based on intense scrutiny of their aesthetic merit.

COLLECTING COMMODITIES AS ART

Given the previous descriptions of the art world and the character of commodity design, it seems quite unreasonable to imagine commodities not designed for collection, ever acceding to the status of art. Artworks are valued for their scarcity, and commodities (particularly industrial com-

modities) are valued for their mass reproducibility. Walter Benjamin explores this problem in the following way:

> That which withers in the age of mechanical reproduction is the aura of the work of art. This is a symptomatic process whose significance points beyond the realm of art. One might generalize by saying: the technique of reproduction detaches the reproduced object from the domain of tradition. By making many reproductions it substitutes a plurality of copies for a unique existence. . . . The uniqueness of a work of art is inseparable from its being imbedded in the fabric of tradition. This tradition itself is thoroughly alive and extremely changeable. An ancient statue of Venus, for example, stood in a different tradition context with the Greeks, who made it an object of veneration, than with the clerics of the Middle Ages, who viewed it as an ominous idol. Both of them, however, were equally confronted with its uniqueness, that is, its aura. . . . The work of art reproduced becomes the work of art designed for reproducibility. From a photographic negative, for example, one can make any number of prints; to ask for the "authentic" print makes no sense. But the instant the criterion of authenticity ceases to be applicable to artistic production, the total function of art is reversed. [Benjamin 1969, pp. 555–56]

The traditional ways of locating artworks, based on their uniqueness and authenticity, cannot be used to locate mass-produced works of art. Industrial commodities, designed for mass production and sale, simply do not pretend to be unique and are not valued for their "authenticity" (in the literal sense of having a known origin and/or author). Since access to the marketplace and the success of goods on the mass market are major systems for evaluating industrial designs, and since market success is in part measured by the quantity of objects mass produced and marketed from each design prototype, industrial production tends to discourage the creation of unique objects with independently valuable designs.

In contrast, craft commodities may have an "author" and be unique, but they are not usually made to develop lasting value, independent of the value for which they were produced. Today plumbing or other craft traditions are protected and innovations in plumbing techniques limited by building codes which are meant to distinguish between good and bad work but also to allow for a large amount of good work to be both created and legitimated. Codes are strict enough to discourage massive amounts of amateur work, making the work of professional plumbers more valuable, but they are not detailed enough to distinguish outstanding from adequate work. In this system almost everyone can have good plumbing (or plumbing that is recognizable within the traditional value systems), but probably little rare and independently valuable plumbing is identifiable.

Particularly industrial commodities but also craft goods are designed to

reach a mass audience. Good plumbers and talented industrial designers are recognized for their ability to make work at relatively low cost and high mass appeal for quick sale rather than lasting significance.

The differences among art, craft, and industrial goods have been enhanced by the social norms that make artists critical of "commercial" work, craftspeople critical of the poor quality of industrial goods, and businesspeople critical of artists and craftspeople because they are not "progressive" and cling to "old ways" in the face of change. But what allows people to identify lasting value in commodities, belying Benjamin's (1969) argument, is that art, craft, and commodities can be and frequently are carriers of the same design traditions. In spite of the structured hostilities among artists, craftspeople, and industrial designers, all have been known to look to the others' work for precedents (or ideas) for their own work (see Becker 1978; Bluestone 1957; Papanek 1972; Newhall 1964). As a result, it is possible to find in pottery, as well as film, the use of aesthetics that would appear sophisticated even to some of the more conservative members of the art world.

THE U.S. FILM BUSINESS

The founders of America's film studios were not in film to explore aesthetic values but to make money. They were not from elite backgrounds, had no training in high culture, and apparently were not self-trained in it, either. They were close to the stereotype of ambitious and upwardly mobile people that elites disliked and imagined were using film to make a sleazy business rather than a fine art (see Jowett 1976; Sklar 1975; Hampton [1931] 1970).

For their part, the businessmen who ran the studies were convinced that thinking about film as an art form was a business mistake, one the Europeans had made which destroyed their supremacy in international filmmaking (Hampton [1931] 1970). They were not against improving film quality, but, according to Hampton ([1931] 1970), they wanted improvements to appeal to their audiences, not to improve their reputations as culture producers.

> Something unforeseen was happening to the mind of the mob. In some mysterious manner, millions of human beings were refuting the dogma that mass mentality could not, or would not, move ahead. . . . There was merely a ceaseless, irresistible drift of millions of plain people to little ticket-windows to buy amusement—cheap, flimsy amusement that aroused the disgust and scorn of the intelligentsia. But all the time the demand was for better pictures, and they had to be devised and manufactured, not by intellectuals, but by mere business men whose only purpose was to make

> *money by selling better shows to their customers.* [Hampton (1931) 1970, pp. 121–22][8]

The "mere business men" that Hampton describes did not make all the improvements in film quality on their own. They hired people to work for them and frequently they recruited people who had fine arts backgrounds, the most notable of them being D. W. Griffith.

Griffith came from a "genteel" family, wanted to be a writer, tried to become a stage actor and failed, and turned to the film business for work (see Sklar 1975). One does not need to elaborate in this paper the scope of his contribution to U.S. filmmaking. What is important here is that he was one of a long line of people with an elite background and/or training in fine art who brought elements of high culture into this entertainment business.

By the 1920s large numbers of people with literary and theatrical backgrounds entered Hollywood. This huge influx was stimulated by the growing legitimacy of the film business and the need for writers and trained actors and actresses to handle the dialogue on sound films (see Carey 1975; Robinson 1973; Crowther 1957).

This pattern of recruitment created a longstanding conflict between "money people" and "talent," where the money people generally won (Powdermaker 1950). The studio moguls might need "talent" to make their business run, but they jealously guarded their control over film production. Films were the commodity they traded and around which they had built huge business organizations. They were the ones with the connections to banks and other sources of capital; they had created the vertical monopolies that made the studio system so strong; they had the international distribution circuits; and they had the "talent" on contract (see Jowett 1976; Sklar 1975; Balio 1976*a*; Crowther 1957).[9]

This model of the U.S. studio system reached its apex in the 1940s (Tunstall 1977), when it also began to receive the major blows which were to hurt it even more noticeably in the 1950s. The House Un-American Activities Committee was beginning its investigations (Jowett 1976; Hellman 1976); television was becoming a true mass medium (Jowett 1976; Tunstall 1977); and the studios were forced to divest themselves of their theaters in the antitrust decision known as the Paramount Decision (Jowett 1976; Tunstall 1977). The loss of control over distribution at home was matched by growing distribution problems in Europe, where countries put

[8] The populist rhetoric Hampton uses contains a strong antielite, antiintellectual component that Gans (1975) finds characteristic of the low-taste group.

[9] The longstanding conflict between stars and studio heads was in large part responsible for the growth of United Artists (see Balio 1976*a*).

up trade restrictions to reduce U.S. dominance in their home markets (Jowett 1976; Robinson 1973; Tunstall 1977).

The wisdom of hindsight suggests that the response of U.S. filmmakers to these problems was sometimes as destructive to the business as the problems themselves. The HUAC hearings stimulated mass blackballing which depleted Hollywood of some of its great talents; the film studios used their contracts and longstanding association with the remaining talent to keep them from working in television; and U.S. companies began to make films in cooperation with European film companies to get around their distribution problems, sometimes further weakening European competition and sometimes simply making film production more difficult (see Tunstall 1977; Jowett 1976; Guback 1969; Balio 1976*b*; Hellman 1976; Sklar 1975).

By the mid-1950s the studio system, which had made filmmaking in the United States such good business, appeared to be in serious financial difficulty. The businessmen who had held such a tight rein over the studios seemed to be losing control over their organizations, their audiences, and their ability to define filmmaking.

U.S. FILM AS ART

While the U.S. studio system was faltering, a new type of movie theater was developing: the "art house."

> One post-war phenomenon which did much to influence both the production and distribution of American films was the growth and success of specialized movie theaters which exhibited "art" films. It was especially heartening to see this development at precisely the time that television was wreaking havoc in Hollywood, for it indicated the existence of a hard core of serious motion picture patrons who were prepared to pay to see films of good quality, or with provocative themes. By 1956, there were over 220 such specialized movie theaters in the United States, devoting their exhibition time to films from other countries, reissues of old-time Hollywood "classics," documentaries and independently made American films with offbeat themes. . . . There were another 400 movie houses which exhibited specialty films on a part-time basis. Almost all of these art houses were located in the large metropolitan areas, and in college and university communities. [Jowett 1976, pp. 377–78]

These art houses were not big business[10] since they appealed to such a

[10] These art houses provided film studios with some relief for their distribution problems. By showing European films in theaters that had previously block-booked films from only one of the major studios, they killed two birds with one stone: they developed reciprocal trade agreements with European countries, and they could claim to have divested themselves of control over theaters without allowing theaters to book films from a competing U.S. studio.

small segment of the U.S. movie-going population, but they did provide a new market for films with aesthetic merit, providing a new legitimacy for films that was welcomed by filmmakers during this period of financial crisis.

U.S. filmmakers could have gained a sense of aesthetic legitimacy for their work much earlier than they were willing to do so. As early as 1915 some American writers had described the "art" in film (Lindsay [1915] 1970; Ramsaye [1926] 1964), but their ideas seem to have had little impact on either audiences or U.S. filmmakers. In the 1930s the Museum of Modern Art opened its film library and asked for cooperation from the studios in developing their collection; the studios were reluctant to help (Jowett 1976). Cine clubs had developed in the 1930s to show "specialty" films, particularly documentaries, but these groups were resisted rather than encouraged by Hollywood (Jowett 1976). It seems to have taken the crises of the 1940s–1950s to have brought the film-as-art notion into fashion.

The economic problems U.S. studios faced at home during the late forties and early fifties encouraged them to increase their international distribution (Guback 1969). To do this U.S. companies increased their European coproductions (Tunstall 1977) and began importing more European art films (Jowett 1976). Through this expanded trade, art house audiences in the United States may have acquired a new perspective on their domestic product, coming to see U.S. films as art.

In 1957 Arthur Knight published a book that was to become the prototypical American film-as-art history.[11] His work is an international film history that attempts to describe the aesthetic development of the medium. He does not try to account for the growth of filmmaking in all the countries of the world, but rather focuses on periods of filmmaking in various (primarily European) countries when a large number of films produced were acclaimed on aesthetic grounds. "In the arts, one discovers in various countries and at different times that happy combination of social, cultural and economic forces which provides creative artists with just the right stimulus to produce their best work. . . . The same process can also be discerned in the motion picture, with the sole difference that the creative history of this art has been crammed into little more than fifty years" (Knight 1957, p. 6).

Knight's (1957) analysis takes stock of the film industry's economic problem: he suggests that the threat posed to the film industry by television is at least in part created by filmmakers' misunderstanding of the aesthetic potential of their medium. He is less harsh than compassionate with the

[11] The significance of Knight's book as precedent for future film books is described in Thompson's (1975) review of film textbooks.

U.S. film studios, showing concern for their future and the fate of U.S. films.

In this work Knight (1957) assumes a role likely to appeal to elites. He acts as a critic-advocate (Wolfe 1975) trying to bring to the attention of art connoisseurs works whose aesthetic value is deserving of their attention. Many critics of traditional art media were adopting a similar stance to make their reputations as champions of new schools of artists, so Knight's tone in this work sounded a familiar note in the art world (Wolfe 1975).

By 1957 there was probably a large audience in the United States ready to hear what Knight had to say. Many people had been going to art houses, seeing European films, hearing that Europeans thought of film as art, and learning that many U.S. films were revered in Europe. These audiences were probably quite prepared to accept Knight's ideas about the aesthetic importance of the medium and eager to understand the artistic value in film.

THE ARTISTIC VALUE OF FILM

Anyone who has faced Benjamin's (1969) work or considered independently the logical problems involved in viewing mass-produced films as art may be surprised by the lack of concern for this issue in film-as-art books. Most authors of film books use a stroke of definitional magic to avoid this problem; they write as though films were handcrafted objects lovingly created by the artist-director. Their rhetoric rarely suggests that businessmen have kept a close eye on film production and used films for mass distribution to gain tremendous profits. By discreetly shunning film's business significance, writers can provide films with the bases for the "aura" that Benjamin (1969) expects in art; they treat films as though they had authenticity (or auteurs) and uniqueness (handcrafted production).

This style of analysis stands out in studies of film because it is so obviously a restricted and one-sided way of regarding film production. Films are necessarily made by groups of people. There is even some evidence that decisions about film content are often made by many different members of film crews (see Mukerji 1976). It is probably also erroneous to assume that film directors always take charge of the same set of decisions for any film. To think that one person, the director, is selected as the "creator" of a film is either a self-serving move for people who want to claim film as an art form or another expression of the Western capitalist cult of individuality (Lomax 1968).

The extensive film-as-art literature that has developed in the United States explores three major themes to show the reader the artistic value in film: the aesthetic heritage (particularly literature and drama) that has helped shape the use of the film medium, the aesthetic possibilities designed into the film medium, and the genius of great directors in exploring the expressive possibilities of film.

These books are written in a variety of forms, but they all "make" film an art by analyzing films and patterns of filmmaking that fit the rhetoric of art history/ criticism. Some histories use Knight's (1957) style of analysis to study international filmmaking or filmmaking in a given country (see, for instance, Robinson 1973; Richie 1971; Leprohon [1966] 1972). Others present directors' lives and their ideas about film (cf. Seton, 1960; Sarris 1967; Renan 1967), and still others analyze conventions of film production (see Stephenson and Debrix 1965; Bluestone 1957). Taken as a body of literature these books do not so much argue that film is an art form as assume that its status as art is unquestionable.

Benesch ([1945] 1965) describes the purpose of art history as the identification of aesthetic problems and their resolution by artists. Gans (1975) describes the central characteristic of the high-taste group as a "creator-orientation." By both these measures, the film books written in the United States since the fifties use the style of art history/criticism to analyze films.

Authors of this film literature present the public with methods for making aesthetic evaluations of films, methods for understanding the lasting value of "classics" and recognizing what distinguishes them from lesser films. Readers can learn from them, as they learn from most art history/ criticism literature, what great works have been made, who made them, and what technical examinations of these works reveal. Defining films as an art is one way to make films meaningful after their initial market value no longer applies.

While histories of film based on the definition of film as business are likely to discuss films such as *Ben Hur* and *Cleopatra,* which were either big money-makers or terribly expensive to produce, film-as-art histories tend to include films by people like Melies whose business sense was not great but whose films were technically innovative. Depending on how the object category, film, is defined, what is deemed important in films can vary tremendously.

FROM COMMODITY TO COLLECTIBLE

Just as art is the prototypical collection system, the redefinition of film from commodity to art may be prototypical of the growing redefinition of U.S. industrial commodities as collectibles. In the film case, this redefinition was accomplished through exposure of Europeans to American films and exposure of Americans to both European art films and European definitions of U.S. films as art just at the time when the U.S. film business was having trouble making films valuable commodities.

European ideas about the aesthetic value of American films had little effect in the United States until the film business faced possible financial

failure. Until that time American film moguls had marketed U.S. films in part on the "glamour" of the film business made possible by the financial success of film studios. European film critics might never have shown great interest in the aesthetics of U.S. films if this U.S. definition of films as commodities had not been so dominant in the United States. If American critics had developed elaborate aesthetic critiques of U.S. films, European critics might have decried the lack of aesthetic quality in U.S. films just as they had previously derided the low quality of U.S. paintings and writing. In lauding U.S. films European critics could give Americans the kind of backhanded compliment they have often used in describing American culture; they could laud U.S. films while muttering about the insensitivity of Americans which blinded them to aesthetic merits of their own art form.[12] But the economic crises of the studios in the fifties encouraged Americans to embrace the notion of film as art and use it to attract new audiences to movie theaters.

This pattern for the redefinition of U.S. films suggests some general dimensions which could explain other forms of contemporary collection. The most important factor seems to be potential scarcity. Once it appears that a given kind of object is on the verge of extinction (in the sense that it may not be manufactured for much longer), then loyal consumers may try to develop reasons for sustaining its production by searching for and identifying design features which make that kind of object particularly valuable. In modern industrial societies manufacturers routinely phase out old products that are not selling well and introduce new ones in the hopes of improving their sales. Through a combination of planned and unplanned obsolescence, many mass-produced objects become scarce just a few years after their original manufacture. When consumers find themselves continuously asked to change their buying habits to adjust to manufacturers' production decisions, some conflicts of interest are likely to arise where consumers find worth keeping objects that producers no longer find worth making. This situation may stimulate collection.

As I mentioned earlier, people can collect objects for sentimental, documentary, and practical reasons as well as for aesthetic ones. When objects are on the verge of extinction people may search for some combination of these reasons to maintain the manufacture of or at least the secondhand trade in those objects. The film case suggests some sources for redefining these goods as "worth keeping." Where products have been distributed to a range of different social groups, consumers may already have a rich mine of different reasons for valuing these goods. Where unprofitable goods are distributed to new social groups in an effort to extend their market value

[12] The contrariness of European film critics vis-à-vis American critics has been borne out by recent events, where Europeans are praising American action films just as U.S. critics are decrying film violence (see Drew 1978).

(the case with films), the new consumers may add even more ideas about the value of these goods which can be used to point out the significance of their designs. These definitions can be used to give objects new market value, either as commodities with new uses and potential for initial sales or as collectibles with lasting or resale value.

Artwork seems to be the prototypical collectible because its major value lies in its resale trade rather than in its initial sales. The way goods become defined as art, including film's redefinition as art, represents one of many ways in which people define pieces of material culture as "worth keeping" or collectible, making them available as design precedents for future generations to use in shaping their contemporary culture.

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Max Weber's Elective Affinities: Sociology within the Bounds of Pure Reason[1]

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Several scholars have called attention to the importance of Weber's use of the term "elective affinity," yet nowhere has the term received a treatment both systematic and historically founded. The present paper attempts to fill that gap. Each instance of Weber's usage is cited and discussed. Next, the place of elective affinity in his order of discourse is determined. Then, the lineage of the term in the histories of literature, chemistry, and philosophy is examined with special reference to Weber's knowledge of those histories. Two related terms, "affinity" and "inner affinity," are examined and brought into relationship with Weber's use of elective affinity. These materials suggest that elective affinity, conceived as an "idea" in the Kantian sense, would have served to answer the question, How is social science possible? which was implicit in the neo-Kantian framework of Weber's order of discourse.

In the lexicon of Weber's thought, "elective affinity" (*Wahlverwandtschaft*) has often been sensed as a crucial term, most notably by Gerth and Mills (1946), Stark (1958), and Mayer (1975). And rightly so. For understanding Weber's idea of history and thus the logic of his social science no term is more crucial than "elective affinity." For Gerth and Mills, elective affinity is "the decisive conception by which Weber relates ideas and interests" (1946, p. 62); for Stark, elective affinity is Weber's theoretical alternative to "mechanistic causalism and quasi-organological functionalism" (1958, p. 256); for Mayer, Weber's thought moves generally "in concepts of elective affinities . . . between existence and consciousness" (1975, p. 707). Yet, despite this sense of its importance, Weber's use of elective affinity has never received a treatment both systematic and historically founded. Certainly the glosses of these scholars do not begin to exhaust its potential, for it would yield as idea the possibility of social science altogether within the bounds of Weber's order of discourse, which is to say, within the bounds of Kant's pure reason. To be sure, few if any of Weber's terms pose more resistance to explanation. The Kantian tradition to which Weber was heir has largely been lost on succeeding generations of social scientists, particularly in America. Instances of elective affinity in Weber's

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work are altogether rare (1904*a*, pp. 29, 34 [1949, pp. 56, 61]; 1905*a*, p. 54 [1958, pp. 91–92]; 1906*a*, p. 148 [1949, p. 118]; 1906*b*, p. 347; 1910, pp. 581, 596; 1916, pp. 19, 20 [1946, pp. 284, 285]; 1918, p. 76 [1968, p. 1429]; 1922, pp. 183, 270, 780, 795, 796, 815 [1968, pp. 341, 472, 1160, 1180, 1208]). His usage is diverse and moreover quite informal. Nowhere does he enter a definition of elective affinity into his categorical casuistic. Some instances of his usage are striking enough in context to draw a second look, but even there the term defines its contexts so much more strikingly than those contexts condition the term that little can be gleaned from those contexts alone. Thus, his use of the term has continued to fascinate while remaining an enigma, and efforts to understand it have become symptomatic of the helplessness with which mere exegesis confronts the order of a discourse other than its own. It is one thing to sense the term's importance, another to establish it, and yet another to find its locus in the order of his thought. No reading of his work alone, be it ever so close, could ever accomplish all this, and least of all would pseudoetymology (see Gerth and Mills 1946, p. 63) or the commonplace—however true—that "like attracts like" suffice (see Stark 1958, p. 257). The problem is one for history and not for exegesis (see Skinner 1969; Jones 1977). To discover what elective affinity meant to Weber is to discover its place in the order of his discourse. The term "elective affinity," as it was known to Weber, has its source in the chemistry of the 18th century. From there it entered into literature, above all through Goethe, and into an order of intellectual discourse, where it was joined with the related expression "inner affinity" (*innere Verwandtschaft*) and with Kant's idea of reason which he had termed "affinity" (*Affinität*). In Weber's unique order of discourse, his use of elective affinity stands at the intersection of two sets of historical coordinates, the one defined by his order of discourse, the other by that rare combination of erudition and insight which was Max Weber himself. Then as now, the potential of that intersection has remained more virtual than actual. With what alchemy the transmutations of its elements into the idea of Weber's social science as one within the bounds of Kant's pure reason was accomplished is the object of the present inquiry. Such an inquiry has no fear of the counterfactual, of inference in the subjunctive mood. For only thus can elective affinity be brought into its full relief. In the light of the virtual—the order of the possible—elective affinity stands out as a source through which the order of Weber's discourse becomes just visible within his own work as the latent structure of his thought.

INSTANCES

The instances of elective affinity in Weber's work show a usage as diverse as it is informal. The first appears in his 1904 essay on "objectivity" where,

discussing the sources and the inevitability of conflict in the arena of social policymaking, he states that ". . . there is conflict not only . . . between 'class interests' but also between Weltanschauungen—whereby of course the truth remains wholly intact that as to which Weltanschauung the individual represents . . . the degree of elective affinity which joins it to his 'class interest' . . . is usually decisive . . ." (1904*a*, p. 29 [1949, p. 56]).[2] A few pages later, this time by way of explaining the equal inevitability of his *Archiv*'s manifesting a tendency with respect to those conflicts, despite its "value-free" stance, he states that "the elective affinity of similar ideals will . . . allow the circle of [its] contributors to hold together and to recruit new members, and this will stamp the journal . . . with a definite 'character' . . ." (1904*a*, p. 34 [1949, p. 61]). These instances are typical of the informality with which Weber uses elective affinity. The term appears as a factor in causal propositions, but no definition is given, and neither syntax nor context lends it any special weight.

But a year later, closing the first installment of the "Protestant Ethic" with a caution to his readers, he states that, ". . . in view of the immense confusion of reciprocal influences between the material bases, the forms of social and political organization, and the intellectual and spiritual contents of the cultural epochs of the Reformation, one can proceed only by first of all inquiring as to whether and in what points definite elective affinities between certain forms of its religious faith and its work ethic are discernible. Thereby and at the same time, the manner and the general direction in which in consequence of such elective affinities the religious movement affected the development of the material culture will be clarified as much as possible" (1905*a*, p. 54 [1958, pp. 91–92]). While his usage here is no less informal, its methodological and explanatory import lend it a special significance. Within the chaos that the social scientist confronts, there is an order; this order exists not only for himself but also for the actors in history and largely affects history's course. The logic of history would be the logic of the elective affinities.

Weber used elective affinity twice more in connection with the "Protestant Ethic," in a reply to his critic Rachfahl. In the first of these, he speaks of "the elective affinity of Calvinism . . . for capitalism" (1910, p. 581), while in the second he speaks of the "elective affinity of the bourgeoisie for certain life-styles," among them those of ascetic Protestantism (1910, p. 596). In a similar vein, he speaks in "The Economic Ethic of the World Religions" of the elective affinities between the various social strata and their characteristic forms of religiosity and of the greater prominence of such elective affinities among the burgher strata (1916, p. 19 [1946, p.

[2] All translations are my own, with the single exception of the quotation from Bergman. References to the available translations of Weber's works have been provided in brackets following the German references.

284]); a page later he speaks of the elective affinity of missionary prophecy for the conception of a personal god (1916, p. 20 [1946, p. 285]), while in *Economy and Society* he speaks of the lack of any such elective affinity of the warriors for the conception of an ethically transcendental god (1922, p. 270 [1968, p. 472]); of the "elective affinity of the religiously demanded life-style with the socially conditioned life-style" (1922, p. 796 [1968, p. 1180]); of the "universal elective affinity between burgher and religious powers" (1922, p. 780 [1968, p. 1160]); of the same exemplified in antiquity (1922, p. 795 [1968, p. 1180]); and of the "inner elective affinity" of the structural principles of the early Protestant sects with the structure of democracy (1922, p. 815 [1968, p. 1208]).

But Weber's use of elective affinity was not restricted to his studies of religion, as the instances of 1904 already show. In a similarly secular vein, he stated in his article "On the Situation of Bourgeois Democracy in Russia" that it was "utterly ridiculous to attribute elective affinity with 'democracy' or even 'freedom' . . . to today's advanced capitalism" (1906*b*, p. 347). Much later, in *Parliament and Administration in Reconstructed Germany,* he spoke of the "elective affinity of [the Center party's] authoritarian mentality with the authoritarian state" (1918, p. 76 [1968, p. 1429]). His usage could also be purely conceptual, as evidenced by his satirical remark in his review of "Critical Studies in the Field of Cultural-Scientific Logic" that E. M. Meyer seemed to imagine "an especially close elective affinity between 'chance' and 'freedom of the will' " (1906*a*, p. 148 [1949, p. 118]).

The informality and diversity of Weber's use of elective affinity obscure what he may have meant by the term, despite the significance given it in the "Protestant Ethic." But once, in *Economy and Society,* he ventures a few remarks of explanation. After rehearsing the difficulties inherent in making either causal or functionalist propositions concerning the interaction of economy and society, particularly significant universal ones, he states that ". . . something universal can, however, be asserted concerning the degree of elective affinity of concrete structural forms of social action with concrete economic forms, that is, concerning whether and how strongly they mutually favor one another's continuance or, conversely, hinder or exclude one another—are 'adequate' or 'inadequate' to one another" (1922, p. 183 [1968, p. 341]). These brief remarks can only suggest that for Weber the sole significant universal propositions that social science could make would be propositions of elective affinity, which would be to say that the very possibility of social science would rest on the logic of elective affinity. But why this would be so and what that logic would be for materials as diverse as those encompassed by his own usage remains unstated, and nowhere else did he elaborate upon the term. But then, he had no need to. His order of discourse took care of all that.

ORDER OF DISCOURSE

An order of discourse is, first of all, an order of words. Their structural relationships within a given order are codified in its dictionaries, the referential networks of meanings which are their histories and thus their definitions. Elective affinity belongs to the order of Weber's discourse and thus to the larger order of the German language as well. The most authoritative codex of that order, the Grimms' *Deutsches Wörterbuch*, has the term (see vol. 13, cols. 597–99) entering German in 1779 as the translator Weigel's neologism for the Swedish chemist Torborn Bergman's term *"attractio electiva,"* referring to the laws of association and dissociation among the elements. From there it gained some currency as a figure of speech among artists and intellectuals, securing a permanent niche in the realm of letters through Goethe, who appropriated the chemical meaning of the term as the root metaphor for a novel, *The Elective Affinities* ([1809] 1951), which Weber's contemporaries found to be a rich expression of the conflict between the natural and the moral and social orders.

But the place of elective affinity in the general order of the German language was never so secure, as may be discerned from its fate in the keepers of that order, the great encyclopedic dictionaries. Twenty years after Weigel, it was not to be found in Adelung's (1801), but Campe's listed it a decade later, most likely as a result of the notoriety lent it by Goethe, whose recent novel the entry mentions (1811, p. 542). It is not in Sanders's (1865) or in Weigand's (1876) or in Sanders's later supplement (1885), although it is in Heyne's (1895, p. 1770), and it is picked up later by Sanders's eighth edition (1910, p. 643) and by Weigand's fifth (1910, p. 803). Paul's lists it (1908, p. 1199), while Kluge's does not (1915). These entries ranged from mere acknowledgments of the word's existence to brief descriptions of its source in chemistry and in the novel by Goethe. More thorough treatment had to wait until the vast enterprise of the brothers Grimm, begun in 1854, reached the letter "W" in 1922, just two years after Weber's death.

Thus, to the larger order of discourse defined by the German language, elective affinity was a marginal word, though its position had improved somewhat by the decade before the First World War. Acquaintance with it could be presupposed only for those familiar with the history of chemistry or for the somewhat larger circle of those who read the German classics. But as a segment of the entire German language community, that circle was small indeed. To have known the meaning of elective affinity was to have been part of a very special order of discourse: the discourse of the humanistically educated elite (see Wehler 1973, pp. 124–29; Ringer 1969). To the rest, the word could well have seemed a contradiction in terms. *Verwandtschaft* ("affinity") denoted blood relationship, while *Wahl* ("elec-

tive") suggested a choice in those matters—an obvious absurdity (see Vetter 1931, pp. 99–100).

Significantly, elective affinity turns up in the philosophical dictionaries, although again marginally. It is not, for example, in the first edition of Krug's (1829), but it does appear in the supplement to the second edition (1838, p. 446). It is not in Kirchner's (1896) or in Eisler's (1899), but a decade later it appears in Schmidt's little paperback (1912*b*, p. 98). That may have been due to Schmidt's other interests: that same year he published a Goethe lexicon in which, of course, the term is entered (1912*a*, p. 248). Schmidt's entry for elective affinity in his philosophical dictionary refers the reader back to the entry for affinity (*Affinität*), which states that ". . . the Law of A[ffinity] (principle of the continuity of the forms) bids a continuous transition from every single species [*Gattung*] to every other via the stepwise increase in multiplicity (Kant)" (1912*b*, p. 9). From there the reader is referred back to the entry for elective affinity, with the usual mention of chemistry and Goethe, thus closing a circle and opening a network in the order of philosophical discourse.

Elective affinity was unknown to the handbooks and lexica of the social sciences, yet it was known to the social scientists themselves—after all, they belonged to the humanistically educated elite. Riehl, for example, made use of the term, writing that ". . . one of the most remarkable monuments to the elective affinity of the North German coastal states with the South German highland is the gothic Church of Our Lady in Munich" (Grimm and Grimm, vol. 13, col. 599). And Treitschke too had used the term, writing that ". . . the Romans were truly intimate only with the old Roman world; the Germans were drawn by a feeling of elective affinity toward the Hellenic genius" (Grimm and Grimm, vol. 13, col. 599). Treitschke had been Weber senior's household intimate in Berlin, where Max junior heard his lectures at the university (Marianne Weber 1926, pp. 42, 102), while later on the Webers, Max and Marianne, were frequent guests in Riehl's home in Freiburg (Marianne Weber 1926, p. 216). In company such as this, the company of men of letters and learning, a familiarity with the meaning of elective affinity could be taken for granted, and Weber did just that when he wrote. The term was a touchstone, even of itself, of the elective affinities that joined such company in an order of discourse which set it apart.

GOETHE

If elective affinity had entered the order of the German language through chemistry and Weigel/Bergman, it entered the order of Weber's discourse through literature and Goethe. Like few others—perhaps like Schiller, like

Kant most certainly—Goethe personified culture itself and, as the pages of the *Goethe-Jahrbuch* (1880–) amply demonstrate, he provided an inexhaustible fund of raw materials for philological industry as well. That industry was no respecter of the disciplines. Kuno Fischer, the leading figure of the Kant revival of the 1860s, produced a number of shorter studies and then a nine-volume series (1890–94); Simmel likewise wrote shorter studies and then two books (1906, 1913); Gundolf, Weber's connection to the charismatic circle around the poet Stefan George, wrote what became a classic study of the poet (1916); even Weber's colleague Rickert wrote a book on Goethe (1932).

Though he eventually came to an appreciation of Goethe, as a young man Weber set himself apart from his contemporaries by judging Schiller the better poet, noting in a letter to his cousin Emmy that "the exaggerated, exclusive Goethe worship" of his friends had spoiled their taste in literature and made them unjust to other poets (Marianne Weber 1926, p. 164). In fact, he continued, Goethe's obsession with happiness resulted in a badly one-sided view of life; Goethe was ". . . sensitive to the debased as such only when it was at the same time the hateful and trivial; he had, on the other hand, no clear sensitivity to it when he encountered it in the form of certain beautiful feelings—cf. *The Elective Affinities*" (Marianne Weber 1926, p. 164). That judgment was made when Weber was 23. He had first encountered the elective affinities nine years before while reading through, "hidden under his desk during class, all forty volumes of the Cotta edition of Goethe" (Marianne Weber 1926, p. 50).

The Elective Affinities is a tale of adultery or, rather, of desired but unconsummated adultery. The plot revolves around the fortunes of four characters: Eduard, a landed gentryman; Charlotte, his recent bride (it is the second marriage for both); the captain, an architect friend of Eduard's; and Ottilie, Charlotte's young and beautiful niece. As the novel opens, Eduard and Charlotte are busy with the renovation of Eduard's country estate. Eduard proposes that the captain join them to help with the planning and building. Charlotte is uneasy about the company of a third but wishes one herself: her niece Ottilie. The third arrives, and the fourth too; and, as might be expected in any novel with such an opening situation, Eduard falls in love with his wife's niece, Charlotte with her husband's friend. The novel develops the unhappy consequences of these elective affinities.

Goethe was not only a poet, he was also a scientist with an interest in, among other things, the progress of chemistry (see Geitel 1911). So it is not surprising that in the advertisement to *The Elective Affinities* he writes: "It appears that the author's continued studies in the physical sciences have occasioned this strange title. He would like to remark that in the study of nature one very often makes use of moral imagery in order to bring closer

something far removed from the circle of human knowledge, and so he wished in a moral case to trace a chemical metaphor back to its spiritual origins" (von Wiese 1951, p. 621). A year earlier, during the composition of the novel, he had told his friend Riemer that "his idea for the new novel . . . was to portray social relationships and their conflicts symbolically" (von Wiese 1951, p. 620).

The novel awoke immediate controversy; the number of negative judgments was "astonishing, for a work by Goethe" (Kolbe 1968, p. 30, n. 55). Jacobi called it "an ascension of evil lust" (von Wiese 1951, p. 645). By the end of the century, such judgments had been tempered, if not forgotten. Cotta's Goethe editor, Karl Goedeke, whose prefaces appeared in all the later editions, took note of them, writing that "the name of the novel derives from the chemical designation of the process wherein different substances united with one another break out of their union in favor of another. This designation, merely borrowed from science, has been so construed by the poet's opponents (whom he was lacking at no time in his life) as if he had attempted to deny the law of free will and to justify a waywardness caught in conflict with civil morality as a law of nature. He did just the opposite" (Goedeke 1885, p. 4). However they judged its morality, the degree of elective affinity between the novel and the problematics of Weber's contemporaries was strong. Whether the analogy between natural and moral and social processes was to suggest that nature itself was somehow "ensouled" (Zeitler 1918, p. 511) or rather that the soul of man stood under the sway of nature (Gundolf 1916, p. 553), the problem it posed was the central one of the relationship between the order of nature and the moral and social orders. And so, however tangentially, the novel touched upon the question of how a social science would be possible if society were a realm of freedom.

CHEMISTRY

Goethe took the metaphor of the elective affinities from chemistry, indirectly from Torborn Bergman by way of J. S. T. Gehlen's *Dictionary of Physics* (Zeitler 1918, p. 513). In conversations with Riemer, he spoke of "the moral symbols in the natural sciences, for example, that of 'elective affinity,' discovered and employed by the great Bergman" (von Wiese 1951, p. 621). But here Goethe errs, for while Bergman may well have been the most famous exponent of the theory of elective affinity in the chemistry of the 18th century, he was not its originator. The history of elective affinity in chemistry and of affinity (*Verwandtschaft*) before that predates his work by more than a century.

Max Weber may have known that history, as evidenced in the opening paragraph of his review of Wilhelm Ostwald's "energy theory of culture,"

where he speaks of "... the most complete layman ... [who] has read the expositions in the general sections—usually so lean—of the older compendia on chemistry, say on atomic weights and equivalences and all that goes along with that; on the concept of 'solutions' as opposed to the 'combinations'; on the electrochemical problems; on isomerism, etc. ..." (1909, p. 575). This most complete layman who had read compendia on chemistry would be Max Weber himself. He gives no references. But Kopp's standard German history of chemistry (1843–) would be representative of whatever he may in fact have read. And he may also have read in Ostwald's own survey of the field (1902), with its lengthy extracts from the sources.

Whatever he read, he would have known affinity (*Verwandtschaft*) as the older part of the expression. It appeared in the 13th century in the work of Albertus Magnus, which was printed in the 16th century. By the 17th century the term was used with some frequency (Kopp 1843–, 2: 288). In 1648 Glauber discussed the fact that "a body did not have the same inclination to combine with every other" (Kopp 1843–, 2: 293), and this led to various attempts to order the elements according to their differing inclinations to combine with one another. In 1718 these efforts bore their first decisive fruit. In an essay entitled, "Concerning the Different Affinities Observed in Chemistry between Different Substances," the French chemist Etienne-Francoise Geoffroy proposed as law the following statement: "Whenever two substances which have some inclination to combine with one another are combined with one another and a third which has more affinity for one of the two is added, then it will combine with that one and exclude the other" (Ostwald 1902, p. 21). This is elective affinity in everything but the name. Geoffroy backed his proposed law with a table in which the differing affinities of some 24 substances were displayed in order. The remainder of the century saw the production of many revisions and extensions of Geoffroy's first "table of affinities" (Kopp 1843–, 2:279).

The climax of the theory of elective affinity came with Torborn Bergman's *De attractionibus electivus* of 1775. Bergman published complete tables of affinity for all 51 elements known then. His work was soon widely known. It reached Germany in 1783, where its translator, Hein Tabor, followed the convention established by Weigel four years previously by writing *Wahlverwandtschaft* ("elective affinity") for Bergman's *attractio electiva* (Grimm and Grimm, vol. 13, col. 597). In his book, Bergman defines elective affinity at the outset:

> Suppose A to be a substance for which other heterogeneous substances, a, b, c, &c., have an attraction; suppose further A combined with c to saturation (this union I shall call Ac), should, upon the addition of b, tend to unite with it to the exclusion of c, A is then said to attract b more strongly than c, or to have a stronger elective attraction for it; lastly, let the union of Ab, upon the addition of a, be broken, let b be rejected, and

> a chosen in its place, it will follow that a exceeds b in attractive power, and we shall have a series a, b, c. in respect of efficacy. What I here call attraction, others denominate affinity. . . . [trans. in Bergman (1775) 1970, pp. 6–7]

This became the classic formulation of elective affinity, the one that appeared in paraphrase in all the handbooks and dictionaries, including the one that Goethe used to weave the definition into the after-dinner conversation of the characters of his novel, where Weber and his contemporaries, even those who had never read compendia on chemistry could find it spelled out even to the letters (Goethe [1809] 1951, p. 276).

THE ART OF DIVORCE

Even in its late 18th-century heyday, the theory of elective affinity had its critics. Guyton de Morveau, leading exponent of the theory in France, complained in 1776 that "some chemists . . . have been unable to restrain themselves from making sour jokes about the so-called table-turners." A year earlier, the critic Buffon had complained of the chemists that they "assume just as many little lawlets of affinity as there are special cases of association and dissociation" (both in Ostwald 1902, pp. 29–30). Over and above the strivings of these early chemists, there loomed the for them as yet unattainable ideal of Newton's mechanics, whose mathematical necessity knew no need for "little lawlets." So it is not surprising that Immanuel Kant, who had heard of Bergman (see Kant 1902–, 9:198; 10:219, 234) but adored Newton, should have held chemistry in comparatively low esteem.

In his *Metaphysical Bases of Natural Science,* Kant stated that "in every theory of nature only as much real science is to be met as there is knowledge a priori to be found therein" (1902–, 4:470), and on this basis he pronounced his judgment that ". . . chemistry can never become anything more than a systematic art or theory of experiment, never a real science, because its principles are merely empirical and allow of no portrayal a priori in envisagement [*Anschauung*]; consequently, they cannot make the axioms of chemical phenomena the least bit comprehensible vis-à-vis their possibility . . ." (1902–, 4:471). Accordingly, chemistry must content itself with a mixed position between mechanics, the pure science of outward envisagement which is space, and psychology, the pure description of inward envisagement which is time (1902–, 4:471).

And yet this mixed position which Kant assigned to chemistry came later to his benefit as an analogy in the final paragraph of his *Critique of Practical Reason:* "To follow that same path [of analysis] in the treatment of the moral capacities of our nature, that example [mechanics] can counsel us and give us hope for a similarly good success. We do have the examples

of morally judging reason at hand. Now to analyze these examples into their elementary concepts . . . a process analogous to chemistry, of divorcing the empirical from the rational which may be found within them, can, through repeated trials . . . make both known to us, purely . . . and with certainty" (1902–, 5:163). "The Art of Divorce" (*Die Scheidekunst*) had been the common language name for chemistry then, a play on words which Goethe later also made ([1809] 1951, p. 273), and only through an equally artful divorce had Kant been able to argue the possibility of man as a moral agent, for only after such a divorce of the empirical from the rational could reason rejoin and order anew what its critique had put asunder.

AFFINITIES

In the *Critique of Pure Reason,* affinity (*Affinität*) denotes a principle or maxim of reason, which is to say, an idea. Kant introduced the term first in connection with his "axiom of complete determination," which states that for any single thing to be possible, its predicates must include, positively or negatively, all possible predicates of things in general (see 1902–, 3:385). From this it follows that ". . . every thing is referred to a common correlatum, namely, the totality of all possibility, which, were it . . . to be met in the idea of one single thing, would prove an affinity of everything possible through the identity of the ground of its complete determination" (1902–, 3:356n.). Once this has been established, the idea of affinity is introduced as a principle or maxim of reason: "Reason . . . prepares the field of intellect: (1) through a principle of the equivalence of the multiplicitous under higher species; (2) through an axiom of the variety of the equivalent under lower species; and, in order to complete its systematic unity, it adds, further, (3) a law of the affinity of all concepts, which bids a continuous transition from every single species to every other via the stepwise increase in multiplicity" (1902–, 3:485). Kant called such principles "regulative," for the ideas underlying his reason form no part of his objects of knowledge, but rather serve to guide cognition by providing its ideals (see 1902–, 3:426–42).

Through Kant's usage, affinity survived in the order of philosophical discourse in a manner akin to that of elective affinity. The connection between the philosophical and the chemical affinities was not to be missed. The first volume of G. S. A. Mellin's great *Encyclopaedia of the Critical Philosophy* began an 18-page entry of affinity with the analogy from chemistry: ". . . (logical or analytical) affinity . . . thus is called that property of the concepts, that they have certain features in common with other concepts . . . that certain features of the pure concept are one and the same with certain features of the other concepts. . . . The concept of the alkaline salts

is, for example, that they are salts which have a sharp, burning, uremic, but not sour taste. . . . The alkaline salts and calcareous earths have . . . in their concepts a common feature through which they are affine to one another . . . namely, that they both absorb or can unite with acids" (1797, pp. 89–90). Mellin went on at once to discuss "the logical law of the affinity of all concepts" and its treatment by Kant, and then, true to the order of late 18th-century thought, he put forth a table of affinities with combinations of Greek and Roman letters standing not for chemicals but for concepts, letters in common representing shared features (1797, p. 92). The closing pages of the entry, however, provide a key to those letters with concepts drawn from chemistry (1797, pp. 104–6).

Over the course of the next 100-odd years, the philosophical idea of affinity fared no better but perhaps no worse than its chemical counterpart elective affinity. The entry in Krug's dictionary is broadly conceived, mentioning beside the philosophical and chemical affinities the ones of aesthetics and ethics (1838, pp. 406–7). Kirchner's ignores affinity altogether, just as it ignores elective affinity (1896); the entry in Eisler's, on the other hand, is replete with citations from Kant (1899, p. 20). Fritz Mauthner's "new contributions to a critique of language" allows affinity the space of two columns, only to denounce its various uses as metaphysical abuses of language (1910, pp. 14–15). Then there is Schmidt's, already cited, with its cross-referencing of affinity with elective affinity (1912*b*, pp. 9, 98).

Thus, affinity too in its technical sense was a marginal word, a marker of an order of discourse: the discourse of those familiar with Kant. Weber would have known that sense of the word through Kuno Fischer, whose lectures on the history of philosophy he heard bleary eyed in the early mornings of 1882 (Marianne Weber 1926, p. 70), if not directly from reading Kant, which he had done from boyhood on (Marianne Weber 1926, p. 63). Among his Heidelberg colleagues, neither Windelband nor Rickert nor Lask dealt with the term as their teacher and grand-teacher Kuno Fischer had. As Weber had read in Fischer's *History of Modern Philosophy*, which may also have served as text for the lectures he heard (see Windelband 1897, p. 6), the place of affinity as an idea of reason in Kant's critique was for Fischer a crucial one. Through Kant the Platonic doctrine of the ideas had suffered a radical change. No longer were the ideas to relate to the things as their concepts and their archetypes. Now the ideas were to serve knowledge, not its object. As an idea of reason, affinity stood as an ideal for concept formation, a maxim for the conduct of intellect (see Fischer 1869, pp. 592–97). A similar conception would hold for his pupil Weber also. In a Kantian lexicon of Weber's thought, elective affinity would be an idea.

INNER AFFINITY

In his much neglected review of "The Conflict in the German Literature of the Past Decade concerning the Character of the Social Constitution of the Ancient Germans," Weber writes: "When we construct a 'stage of culture,' then this thought construct, analyzed into judgments, means only that the individual appearances that we thereby assemble conceptually are 'adequate' to one another, possess a certain measure of 'inner affinity' [*innere Verwandtschaft*] . . . with one another, but never that they follow from one another with any kind of lawfulness. In other words, they are a conceptual means of portrayal, but not foundations for a deduction in accord with the infamous schema 'All men are mortal; Caius is a man; therefore, he is mortal' " (1904*b*, pp. 440–41). While the term here is "inner affinity" rather than "elective affinity," the reference to "adequacy" may serve to connect this passage with the later one in *Economy and Society*, where Weber explained his usage of elective affinity in that instance in terms of the "adequacy" or "inadequacy" of forms of action and economic forms to one another (1922, p. 183 [1968, p. 341]). But equally, if not more, important here for the logic of elective affinity in Weber's usage are his reference to the analysis of constructs into judgments and his denial of necessity in the relationships between those judgments. The former would yield the framework for the idea of elective affinity; the latter, its locus in the order of his thought.

As with so many points of epistemology and logic, it was his friend and colleague Heinrich Rickert who provided a starting point for Weber's thought (see Burger 1976). In his dissertation on the theory of definition, Rickert writes that ". . . the definition, which enumerates a number of features, [is] a complex of judgments, for the declaration of each feature is always a judgment, and to be sure a complex of 'analytical judgments' that explicitly display everything that was already conceived within the concept. Accordingly, analytical definition translates the concept into a judgment or into a series of judgments each of whose subjects is the concept to be analyzed and whose predicates form the features . . ." ([1888] 1915, pp. 57–58). But this translation of an individual concept into judgments goes much further in its consequences, as Rickert continues:

> . . . we know that the logical ideal of our knowledge consists of a complete system of judgments, whose subjects and predicates are constant, therefore defined concepts Let us imagine this systematization of our knowledge accomplished in that direction. We could then compare the content of our knowledge with a net of threads in which the fixed knots represent the concepts; the threads, on the other hand, which go from one knot to the other are to delineate the connections between the concepts, that is, the judgments. . . . Human thought . . . could never "envisage" this net in its totality, but could only traverse it in that it now forms concepts from out

> of the elements connected to one another, that is, the judgments, now analyzes these concepts back into judgments. . . . just as the knots in the net consist only of threads, concepts are nothing else but the transition points of intersecting judgments. [(1888) 1915, pp. 58–59]

That then is the logic of any typology, Weber's included. Where the judgments constituting two or more of its terms intersect, a transition point between the terms is defined. Where two or more of those terms share a predicate in common, they are in that respect "adequate" to one another, or, in the sense of Mellin's table of conceptual affinities and Kant's idea of reason before it, they possess inner affinity.

But these inner affinities hold not only for Rickert's "net" of constant, defined scientific concepts, but, as any dictionary demonstrates, they also hold for the words themselves in the referential networks of their ordinary usage. Encountering this insight, however obscurely and incompletely thought out in Gottl's *Dominance of the Word* (1901), Weber made note that "our experience is absolutely no longer to be separated from . . . the words" (Brunn 1972, p. 99n.).

The natural affinities of ordinary language are not unrelated to those of the scientific typologies. As Weber points out, "The constructive concepts of sociology . . . are ideal-typical not only outwardly but also inwardly. In the great majority of cases, real action proceeds in muffled half-consciousness of its 'intended meaning.' The actor vaguely 'feels' it rather than knowing it. . . . Actually effective, that is, fully conscious and clear meaningful action is in reality always only a limiting case. . . . But that is not to hinder sociology in forming its concepts through classification of the possible 'intended meaning,' which is to say as if the action did in fact proceed in conscious orientation to meaning" (1922, p. 10 [1968, p. 21]). If the concepts of sociology are the limit cases of possible intended meanings of the words, and if the words are "inner affine" to one another, then the inner affinities of the words will be reflected at their limit in those concepts. Whether inner or elective, in the meanings of the words the affinities of the concepts will have their source.

MUNDUS INTELLIGIBILIS

"Action," said Weber in the most basic of his definitions, "is . . . to mean a human behavior . . . if and insofar as the actor or actors attach a subjective meaning to it" (1922, p. 1 [1968, p. 4]). From this definition it follows at once that every meaning in the lexicon of the actors represents a possible action and that, conversely, every action portrays the meaning in that lexicon to which it is referred. Thus, the world of actions, both actual and possible, is a *mundus intelligibilis*. But the elements of this

intelligible world are not to be considered in their relationship to truth: "From Simmel's method . . . I depart by distinguishing as much as possible intended from objectively valid 'meaning,' which Simmel not only does not distinguish but often intentionally conflates" (Weber 1922, p. 1 [1968, p. 4]). Nor, paradoxically, are these subjectively intended meanings to be considered psychologically: ". . . how mistaken it is to regard any sort of 'psychology' as the ultimate 'foundation' of . . . sociology. . . . The mistake lies in the concept of the 'psychological': that whatever is not 'physical' is 'psychological.' But the meaning of a calculation that someone intends is not at all 'psychological' " (Weber 1922, p. 9 [1968, p. 19]). In a chapter on "Historical Understanding and the Unreal Formations of Meaning," written for the fourth edition of his *Limits of Natural Scientific Concept Formation,* published the year after Weber's death but no doubt composed and discussed while he was still alive, Rickert states that ". . . 'incorporeal' need not necessarily be real-psychological. Rather, there are formations that belong neither to the corporeal nor to the events of the soul . . . nevertheless are directly known to everyone and therefore may also not be situated in a metaphysical beyond. To this in every respect unreal realm we must count, for example, the 'meaning' of a word or the theoretical 'content' of a judgment. Everyone who 'understands' such a formation at all understands it as the same thing" (1921, p. 405). That solves the epistemological problems of *verstehen* by fiat. But with the unanimity which for Rickert here defined both understanding and the elements of his *mundus intelligibilis,* there comes the moment of coercion inherent in a language community as a consensual community (Weber 1913, pp. 276–87). To be sure, that community is not fully an "institution" with a rational constitution, despite all rules of grammar and even with some apparatus of coercion (see 1913, p. 287). But to be understood or to act appropriately and without censure, the actors' understanding of the lexicon must be largely congruent with that of their language community overall. Insofar as this situation is the case, the affinities of the words would have objective force, if not "validity."

The lexicon of a language community defines the universe of possible actions of its members. But just as that lexicon is structured by the inner affinities of its elements, by the intersecting meanings of the words, so too then does each such point of intersection, each such inner affinity, present to the actor a transition point of choice of possible actions in his traversals of his universe of meanings. For the actors of Weber's sociology have that choice. Very late in its history and much circumscribed in scope, Kant's doctrine of the empirical and the intelligible characters (1902–, 3:368–77) makes itself felt in Weber's definitions of action as behavior oriented to meaning and of sociology as "a science that seeks to understand social action explicatively and in so doing to explain it causally" (1922, p. 1 [1968, p. 4]). But for Weber, as for Kant, "that intervention of the intelligible char-

acter into the chain of empirical causality by means of ethically normative actions" (1905*b*, p. 108) remained a limit concept in two related senses. First, and most familiarly, in the sense of the value neutrality of his social science: "An empirical science can teach no one what it is he ought to do but rather only what it is he can do and—in certain circumstances—what it is he wants to do" (1904*a*, p. 27 [1949, p. 54]). But then conversely, too, sociology as an empirical science cannot determine which choice of possible actions the actor will make in his traversals of his universe of meanings. To be sure, the actor's choice of possible actions is circumscribed by the inner affinities of the elements of his lexicon. Just as these structure its elements into networks of meanings, so do they structure his possible actions into constellations. And the more and with the greater constancy his actions are oriented to his ultimate values, the more they fall under the schema of purposive-rational as teleological-rational action (see Weber 1906*c*, p. 108 [1975, p. 118]). Nevertheless, the actor's choice of ultimate values is beyond the ken of social science. Therefore, Weber denies necessity in the relationships constituting the conceptual constructs of his science, despite all adequacy or inner affinity. But with that denial comes a second one: without that necessity there could be no a priori foundation for the possibility of a social science in the manner in which Kant conceived such a foundation for the natural sciences. Accordingly, as in Kant's view of the chemistry of the 18th century, social science could never be anything more than a systematic art based on a divorce of the empirical from the rational. That yields the locus of elective affinity in the order of Max Weber's thought.

ELECTIVE AFFINITY

It is the freedom of the actors with respect to their choices of ultimate values that renders Weber's social science problematical within the Kantian bounds of his order of discourse, even as this freedom derives from that Kantian frame. The logic of elective affinity would provide a solution. Here a metaphor may be of use. Values are related to meanings as are the constellations to the stars. They are not their source and in no way could knowledge of meanings determine choice of values. For the elements of the ***mundus intelligibilis*** are multivalent. Their affinities are manifold and may be of three kinds. They may join the elements of the ***mundus intelligibilis*** to one another through the intersections of their meanings. They may exclude those elements from one another through a like intersection with change of sign. There may be no intersection, but through the former all meanings are joined, however indirectly. A meaning in total isolation from the rest is no meaning at all. The greater the number of positive inner affinities between two elements vis-à-vis the total possible number, the more strongly

are the elements joined. That is their "degree" of elective affinity. The higher the meaning, the greater would be its elective affinity for all the rest. The highest are values. But the lexicon of possible actions, of meanings, is finite and the harmony of the values by no means assured. Impossible it may be to behold the bear and the dipper at once upon the northern sky; but very possible to serve at once the Reformation's work ethic as well as its faith.

Only as the logic of the interrelationships of networks of meanings, of possible actions, could elective affinity extend across the diversity of Weber's usage, from the elective affinity of concrete forms of social action with concrete economic forms to the elective affinity of similar ideals. Here Weber's erudition and insight would meet with his order of discourse. From chemistry and Bergman would come the basic paradigm of elective affinity; from literature and Goethe, its application to the portrayal of social relationships; from philosophy and Kant, the art of divorce of the empirical from the rational and the affinity of all things in their possibility. As a maxim for the conduct of scholarship, elective affinity would suggest the construction of ideal-types derived from the universe of ordinary language and the analysis of those types according to their judgments in order to trace in their elective affinities the actors' choices of possible actions. That yields a logic for a sociology—*die verstehende*—which would "in the consciousness of the narrow limits into which it is banned" (Weber 1922, p. 8 [1968, p. 17]) vouchsafe its possibility. It is elective affinity conceived as idea.

POSTSCRIPT

Weber never worked out the logical consequences implicit in his own usage of elective affinity. But despite the informality of his usage, these consequences can be inferred from its diversity through recourse to the order of discourse which formed its substrate. For Weber, in the great tradition of German philological scholarship, the order of a language was the virtual order of a society. The elements of that order were the meanings of the words in their ordinary usage by the actors in history. Viewed from within the Kantian bounds of his order of discourse, those actors are free in their choice of actual actions. Thus, history would be a logical chaos were it not for an order in the universe of the meanings to which those actors orient their actions. That order is to be found in the elective affinities of words, the greater or lesser extents to which they possess inner affinity through the intersections of their meanings. It is this order in the universe of possible actions which makes his social science possible. The actors' choices of possible actions are given by the elective affinities of their universe of meanings. The order of the actual, the course of history and the structure of society,

is to be read from this order of the possible. The task of Weber's science is to portray its changing constellations.

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Supervision and Conformity: A Cross-cultural Analysis of Parental Socialization Values[1]

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This paper integrates the existing theories on the antecedents of parental values in socialization, drawing heavily on the work of Kohn; Barry, Child, and Bacon; Stephens; and Olsen. Their theories, although varying in specific hypotheses, may be partially subsumed under the general proposition that supervision of the parent in salient behavioral domains is positively related to a parental value on conformity and negatively related to a value on self-reliance. Hypotheses derived from this theory are tested on a sample of 122 cultures drawn from the Human Relations Area Files and the Standard Cross-cultural Sample and are generally supported. Implications for further theoretical development are discussed.

SOCIAL CLASS AND PARENTAL VALUES

Sociologists have long been interested in the association between socioeconomic status and parental values in the socialization process. Kohn's work (1959*a*, 1959*b*, 1963, 1969; Kohn and Schooler 1969, 1973; Pearlin and Kohn 1966), pointing to a negative relationship between socioeconomic status and a parental value on conformity in children, has been of particular interest (see also Gecas and Nye 1974; Wright and Wright 1976; Gecas 1978).

The interpretation of this relationship is often couched in terms of class differences between blue- and white-collar workers. Briefly, a part of the argument is that members of these strata operate under differing reward

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structures in the pursuit of occupational (and thus economic) success.[2] Blue-collar workers are typically subjected to a high degree of supervision on the job. Their tasks are likely to be routinized, adherence to established procedures is required, and there is little need for innovation. Satisfactory performance is largely defined by conformity to external authority. Blue-collar parents, so the interpretation goes, generalize this value to other arenas of behavior and thus value the trait of conformity in their children.[3] White-collar workers, on the other hand, achieve success largely by virtue of individual initiative. They are less closely supervised and must often exercise independent judgment. The reward structure in white-collar occupations is likely to place a premium on autonomy and self-reliance. Since these traits are perceived as conducive to success, white-collar parents value them in their children.

In support of this theory, Kohn and others have consistently found that American blue-collar parents value conformity in children over self-reliance,[4] while the opposite is true of white-collar parents. In addition, these findings have been supported in several studies outside the United States (see Kohn [1977, introduction] for a summary of this literature). Pearlin (1971, pp. 58–70) demonstrates, with data from both Italy and the United States, that class differences in parental values are due largely to class-related differences in the nature of occupational experiences. There are three relevant types of differences: differences in the closeness of supervision, in the major component of work (things, people, or ideas), and in the requirement of self-reliance in work. White-collar occupations are characterized by low supervision, an emphasis on people or ideas rather than objects, and a relatively high degree of self-reliance. Each of these factors is negatively related to the value placed on conformity in children and thus contributes to the explanation of class differences in socialization values.

[2] For more detailed summaries of this theory, see Kohn 1969; Pearlin 1971; Lee 1977; and Gecas 1978.

[3] This is not intended to imply that blue-collar children are actually more conforming than others; in fact, the opposite may well be true (see Devereaux, Bronfenbrenner, and Rodgers 1969; Devereaux 1970, 1972; Lee 1977). The relevant point here is that blue-collar parents appear to value conformity in children more than white-collar parents do.

[4] A great number of terms have been employed by Kohn and other researchers working with similar ideas (see below) to refer to the general trait which we label here as self-reliance. The terms have included self-direction, autonomy, independence, and even assertiveness. We do not wish to confuse conceptual clarity with labeling decisions; consequently we employ the term "self-reliance" throughout this paper. Our intention is to refer to the essential common properties of the terms noted above as the concepts have been employed by previous researchers. It is not our intention to replicate precisely the conceptualization of any previous researcher, singly or collectively Our operational definition of self-reliance is given in the Methods section below.

EXTENSIONS OF THE THEORY

Kohn's theory has most frequently been interpreted as a theory of the effects of social class on socialization values, with dimensions of occupational experience serving as intervening variables. Olsen (1974), however, has demonstrated that the implications of the theory may be considerably broader than this. She generalizes the theory by concentrating on the supervision dimension and noting that closeness of supervision is a general concept which pertains to many behavioral domains other than occupation.[5] One of these domains is the family. Olsen reasons that mothers in patrilocal extended families are closely supervised in the performance of family roles by the husband's mother. She hypothesizes that ". . . mothers in three-generational households should resemble working-class men in the high value they place upon conformity and obedience in children, while mothers in nuclear families should place more emphasis on autonomy and self-reliance" (1974, p. 1396). This hypothesis was supported on a Taiwanese sample. Olsen concludes that the effects of closeness of supervision in extended families may be interpreted according to the logic developed by Kohn for the explanation of occupational stratum differences in parental values. Closeness of supervision of the parent, whether in the occupational or familial role, causes the parent to value the traits of conformity and obedience in children.

Anthropologists have also investigated antecedents of socialization values via cross-cultural research and have employed similar conceptualizations of the dependent variables. Barry, Child, and Bacon (1959) distinguish between "compliance" and "assertion" as socialization values.[6] Their dependent variable is actually a composite of four separate measures, including emphasis on responsibility and obedience (which define the "compliance" pole of the continuum) and emphasis on achievement and self-reliance (which define "assertiveness"). They discover that, where economic systems permit the accumulation and storage of food (pastoral and agricultural economies), the culture tends to emphasize the value of compliance. But in hunting, gathering, or fishing economies food cannot usually be stored

[5] Pearlin (1971) and others have measured closeness of supervision and the requirement of self-reliance in work separately, and Olsen (1974) uses only the concept of supervision as the basis for her generalization of the theory. However, these two variables appear to be closely related on the conceptual level and are also negatively correlated empirically (Pearlin 1971, p. 65). A high degree of self-reliance in work implies a low degree of supervision, almost by definition. These two concepts will thus often be employed interchangeably or in combination in the present analysis.

[6] For related studies, see Barry 1969; Barry et al. 1976; Berry 1967; Draper 1975; and Munroe, Munroe, and Daniels 1973. The basic logic of each of these studies is similar to that of Barry, Child, and Bacon (1959); see also Barry et al. (1967) and Inkeles (1968).

or accumulated. Here the value structure places a higher premium on individual achievement and self-reliance.

The explanation offered for the association between socialization values and type of economy is that economic systems which permit the accumulation of food generally require routinized, cooperative, and organized labor. This implies supervision and "faithful adherence to routines" (Barry et al. 1959, p. 62). Individual innovativeness in method may threaten the organization and functional effectiveness of the work group. Hunting and gathering, though, are usually more individualistic activities. Aggressiveness and autonomy are prerequisities for success; cooperation and adherence to routines are not so clearly required. Since parents value traits in their children which they perceive to be instrumental in their own activities, self-reliance is valued in hunting and gathering societies, obedience (conformity) in pastoral and agricultural societies.

The logic employed by Barry et al. (1959) may be integrated with a part of Kohn's theory. In cross-cultural terms, type of economy may be conceived as an indirect measure of closeness of supervision in subsistence activities. Where adult workers are closely supervised (i.e., in high-accumulation technologies), they value conformity in their children. Where their work is more autonomous, self-reliance is the more valued outcome of socialization.

A second anthropological observation on sources of variation in socialization values implicates the political structure. Stephens (1963, p. 372) shows that societies with autocratic political states tend to be characterized by relatively "severe" socialization practices, including clear-cut power and deference relationships and strong emphasis on obedience. Tribal societies, which have no centralized or autocratic political structure, require less obedience and conformity of their children. To the extent that autocratic political structures imply close supervision of adults, this relationship may be interpreted using the same general logic. Barry et al. (1976), in another study employing societies contained in the Standard Cross-cultural Sample, report that complexity of political structure is positively related to a measure of socialization for obedience and negatively related to socialization for self-reliance. However, they offer no explanation for these associations. Aberle (1961) also suggests that degree of political supervision is correlated with inhibition of aggression. Finally, Bronfenbrenner (1970) points out that socialization in the Soviet Union is much more oriented toward producing conformity than is the case in the United States; This difference is consistent with Stephens's (1963) observation on the effects of autocratic political systems (see also Ellis 1977).

From this brief review, it is apparent that anthropologists and sociologists have actually been employing a very similar basic theory in their

attempts to explain parental values in socialization (see Lee [1977, pp. 258–74] for a more detailed explication of this point). The theory has been applied to different behavioral domains by the two disciplines, thus implicating different independent variables. However, each set of empirical relationships can be at least partially interpreted through the more abstract concept of closeness of supervision and the requirement of self-reliance. Where adults are closely supervised in their economic, political, or family roles, they tend to value conformity in their children; where they are more autonomous in these roles, self-reliance becomes a primary socialization value. This similarity of theory is not the result of interdisciplinary exchange, since the correspondence has rarely been recognized (for a partial exception, see Olsen 1974). Instead, the theory appears to have been independently generated within each discipline.

The central proposition of this general theory is that parental valuation of conformity relative to self-reliance in children is positively related to the extent to which adults are themselves closely supervised in the performance of their own roles. If this is true we may deduce hypotheses about the effects of supervision of adults on socialization values in areas of social life which have not yet been explored from this perspective.

One such behavioral domain is religion. Perceptions of the supernatural may vary along a continuum corresponding to an abstract conceptualization of supervision over human affairs. At one extreme, the god or gods may be culturally endowed with great powers over human destiny which they exercise for human benefit or detriment according to the morality of human behavior. Deviation from culturally defined moral behavior may be believed to invoke negative sanctions from the supernatural forces. In this situation, the supernatural constitutes an external authority, conformity to which is perceived as crucial to human well-being. At the other extreme, the supernatural may be endowed with insufficient power to enforce behavioral standards. Also, in many cultures the deities are believed to be quite arbitrary in their administration of positive and negative sanctions; there is little that human beings can do to influence their fate. Here, conformity to the perceived wishes or prescriptions of the supernatural is much less of a virtue; human fate is largely independent of one's relationship with the deity or deities.

If the relationship between supervision and socialization values is generalizable beyond the occupational domain, we would expect closeness of supervision by the supernatural to influence these values.[7] Consistent with the basic theory, we hypothesize that closeness of supervision by the super-

[7] Lambert, Triandis and Wolf (1959) found that a belief in aggressive (as opposed to benevolent) gods is positively related to socialization for independence and self-reliance However, aggressiveness by the deities is not the same as closeness of supervision and is not clearly related to the theory discussed here.

natural is positively related to conformity relative to self-reliance as a value in socialization of children.

In summary, a considerable body of research and theory in several disciplines suggests that parental values in socialization are responsive to variation in the criteria of success for adults. These criteria pertain to diverse behavioral domains, including the family, politics, and religion, as well as the economic or occupational realm. In each case, adult experience may be arrayed along a continuum representing closeness of supervision or, alternatively, self-reliance. The central proposition of this study, which results from a partial synthesis of theory in both sociology and anthropology, is that parents value conformity over self-reliance in children to the extent that conformity supersedes self-reliance as a criterion for success in their own endeavors. The importance of conformity in adult behavior may be estimated by indirect measures of closeness of supervision in the various behavioral domains discussed above; these provide multiple measures of the value of conformity in adult life.

METHODS

Sampling

The empirical objective of this study is to bring cross-cultural data to bear upon the theory developed above.[8] Accordingly, the Standard Cross-cultural Sample (Murdock and White 1969) was employed as one source of data. The sample ($N = 186$) is representative of all geographic and cultural regions in the world; furthermore, the societies which constitute the sample were selected in such a way as to minimize proximity between members of the sample. This reduces the probability that correlations between cultural traits observed on this sample are attributable to "cultural borrowing" or diffusion (see Marsh 1967, pp. 274–303; Naroll 1968, pp. 258–62; Lee 1977, pp. 44–47).

Most of the independent variables implicated by the theory are available in the Standard Cross-cultural Sample (SCS). These data do not, however, contain satisfactory indices of parental values in socalization or closeness of supervision in the religious domain. These variables were therefore coded from the Human Relations Area Files (HRAF; see measurement section below for coding procedures). The HRAF consists of classified ethnographic data in verbal form. Of the 186 societies in the SCS, 122 also appear in the available microfiche version of the HRAF. Since both data sets were re-

[8] By "cross-cultural" data we mean, in this case, information gleaned from ethnographic reports. Such data provide one observation per variable for each society or culture included in the sample. For a more detailed explication of the nature, potentials, and limitations of cross-cultural data in sociological research, see Lee (1977, pp. 10–11, 22–24, and 31–34.)

quired for this study, the actual sample consists of the 122 societies which are included in both.

This sample is not random, nor can it be considered representative of all cultures in the world. However, it is impossible to obtain a random or representative cross-cultural sample (Lee 1977, pp. 22–23). Nonetheless, with the exception of industrial societies, the sample contains a complete range of societal types and is fully appropriate for testing hypotheses regarding relationships between supervision and socialization values. It is also notably larger than the samples of other cross-cultural studies requiring primary coding of ethnographic data.

Measurement

Five indicators of closeness of supervision were obtained from the precoded data in the SCS. They are intended to index closeness of supervision in the economic, familial, and political domains.

The first independent variable, closeness of supervision in the economic sphere, is indexed by type of economy. The various primary subsistence bases represented in our cross-cultural data sets may be ranked along a continuum ranging from highly individualistic activities, in which group cooperation is minimal, to highly structured and routinized cooperative endeavors. The cooperative activities require greater interpersonal coordination, planning, and therefore supervision. As Barry et al. (1959, p. 52) point out, activities such as fishing and hunting are more likely to yield immediate rewards for individual initiative. In accord with this logic, the various types of subsistence economies were ranked in rough order of increasing supervision as follows: fishing, hunting, gathering, animal husbandry, incipient agriculture, extensive agriculture, and intensive agriculture.[9] There are undoubtedly many instances in which this ordering does not precisely conform to a continuum of closeness of supervision; for example, fishing and hunting may occasionally be done in large groups which require coordination, leadership, and therefore supervision. However, it is reasonable to contend that cooperative group activities requiring supervision are more essential to and frequent in agricultural economies than fishing or hunting (see Steward 1955; Barry et al. 1959; Nimkoff and Middleton 1960; Blumberg and Winch 1972; Lee 1977). Furthermore, group cooperation and interpersonal coordination are more clearly required in the more complex forms of agriculture (such as intensive agriculture,

[9] Our ordering of subsistence types is similar to but not identical with that of Barry et al. (1959). The differences are attributable to the fact that their order was intended to index ability to store and accumulate food, while ours is an attempt to tap closeness of supervision. These two abstract concepts should be positively, but not perfectly, correlated.

which implies permanent fields, the use of rudimentary mechanical devices, and often irrigation) than in extensive or, particularly, incipient agriculture. In this order, the scale corresponds roughly to an index of economic complexity (the complexity of the collective technology, not of individual tasks) and will be labeled as such. More complex technologies require greater coordination, supervision, and conformity to established routines.

Three variables are available in the SCS as measures of closeness of supervision in the familial domain. The first is a simple index of the dominant family structure, categorized as nuclear, stem, small extended, and fully extended. In this order, the categories represent increasing structural complexity of the family in terms of the number of social positions contained in the family structure (see Nimkoff [1965, p. 19] and Lee [1977, pp. 112–15] for detailed definitions of each type). In any of the three types of extended families the parental generation is not the senior generation and is subject to the supervision of its own parents or in-laws. The extent of supervision, then, is presumed to increase as the structural complexity of the family increases.

A second family-related variable measures the dominant mode of tracing descent. Societies with bilateral descent systems have no corporate kin groups; individual families are autonomous with respect to kinship structure. We therefore take bilateral descent as indicative of low supervision and any form of unilineal descent as reflective of higher supervision. It is also possible, however, to distinguish between patrilineal and matrilineal systems along this dimension. In virtually all cultures, the mother is the primary agent of socialization (Minturn and Lambert 1964; Olsen 1973, p. 513). The mother is clearly more subject to supervision by senior members of kin groups in patrilineal systems than in the matrilineal case. The wife-mother in a patrilineal group is an "outsider," at least until she attains some seniority over other in-marrying females such as daughters-in-law. But in matrilineal systems the wife-mother retains membership in her natal kin group throughout her life. If she is subject to supervisory control, it is the control of senior members of her own kin group. This is likely to be much less strict and rigorous than in the patrilineal case. Furthermore, the matrilineal mode of tracing descent cannot be combined with any single custom of postmarital residence in a manner which produces localized kin groups (see Aberle 1961; Fox 1967; Lee 1977, pp. 161–62). This reduces the potential for supervision of parents by senior members of their kin groups. Therefore, supervision of the primary socializing agent is highest in patrilineal kinship systems, lowest in bilateral, and intermediate in matrilineal.

A third measure of familial supervision has to do with cultural control over mate selection. This variable indexes cultural rules regarding com-

munity endogamy and exogamy. Some cultures require that communities be strictly endogamous or exogamous, others have rules that are similar but loosely enforced, while still others have no effective custom on the matter. We posit that strict rules requiring either community endogamy or community exogamy are indicative of high degrees of supervision over the mate-selection process, while the absence of such rules indicates low supervision. Societies with flexible customs were classified as intermediate on this variable.

Finally, an index of supervision in the political realm is given by the complexity of the political structure, measured in terms of number of distinct jurisdictional levels. We assume that political structures increase in complexity as the need for control and organization above the local level increases. A highly differentiated political system exercises more control and supervision than a less differentiated or nonexistent structure. A five-point scale is employed as a measure of political supervision, which ranges from stateless societies where authority is not centralized even at the local level to societies with three or more administrative levels above the local community. This scale was used by Barry et al. (1976, p. 101) and is consistent with Stephens's (1963) logic, but is much more discriminating than the "kingdom" versus "tribe" dichotomy he employed.

Satisfactory measures of supervision in the religious domain were not available in the SCS and were thus coded from the HRAF. Two variables were employed. The first indexes religious taboos, or the extent to which supernatural forces are believed to negatively sanction certain behaviors, thereby directing the behavior of human beings.[10] This variable was coded independently by two judges on a five-point scale, ranging from no supernaturally sanctioned behaviors to the existence of taboos pertaining to virtually every aspect of daily life. The Pearson product-moment correlation between the two coders' ratings was determined to be +.662, based on 115 cases for which sufficient information was available.[11] To assign a final score on this variable to each society, the two coders' ratings were

[10] Space precludes the presentation of complete coding procedures and instructions here. Copies of these instructions may be obtained by writing to Godfrey J. Ellis, Dept of FRCD, 241 HEW, O.S.U., Stillwater, Oklahoma 74074.

[11] This estimate of coder reliability, while acceptable according to standards of cross-cultural research, nonetheless indicates a substantial amount of coder disagreement As a check for the effects of coder error, all cases in which coders disagreed by more than 2 points on the five-point scale were dropped from the analysis. This resulted in the loss of 14 cases; the intercoder correlation coefficient for the remaining 101 societies was + .808. All analyses reported below were then run with only these 101 cases; however, no differences occurred in the behavior of the religious-taboo variable when compared to analyses employing all 115 cases. Therefore the results reported below were obtained by using the entire sample for which sufficient data are available

summed, creating a nine-point scale; higher scores indicate closer supervision.

A second measure of religious supervision indexed the presence or absence of ancestor worship. We were specifically interested in the extent to which the ancestors are believed to play a part in the affairs of the living by controlling their fate, rewarding good behavior, punishing immorality, etc. This variable was originally coded as a simple dichotomy differentiating between the presence and absence of ancestor worship. There was, however, substantial disagreement between coders in the application of this dichotomy, as indicated by an intercoder correlation coefficient of +.614, based on 120 cases. The disagreements uniformly represented cases where the religion stipulated the existence of ancestral spirits but accorded them minimal influence over human affairs. It was therefore decided to employ cases of coder disagreement as an intermediate category on a three-point scale, created by summing the two codes for each society. A high score on this variable represents a clear belief in ancestors who have the power, and the proclivity, to control the outcomes of human endeavors according to their approval or disapproval of human behavior; a low score indicates the absence of ancestor worship.

The dependent variable, emphasis on conformity relative to self-reliance in socialization, was also coded from the HRAF. This variable was coded on a seven-point scale, with high scores indicating a greater emphasis on conformity (obedience, compliance) than self-reliance and low scores a greater emphasis on self-reliance (independence, self-sufficiency) than conformity. The emphasis in coding this variable was upon the kinds of behaviors for which children were rewarded or punished. High scores (representing conformity) were assigned where children were encouraged to be cooperative and obedient to parental authority, or were punished for failure to follow parental directives or group norms involving compliance and responsibility to others. Low scores (indicative of a value on self-reliance) were assigned to cases where children were encouraged to be independent, self-sufficient, or autonomous. This is perhaps most clearly exemplified by initiation rites which require individual survival under arduous conditions. These codes were, of course, made in the absence of any knowledge of the independent variables from either the SCS or from other sections of the HRAF. The two coders for each case worked independently at all times. The current conceptualization of this variable is compatible, although not identical, with those of Kohn (1969, p. 24), Pearlin (1971, p. 57), Barry et al. (1959, p. 58; 1967), and Olsen (1974, p. 1405).

Separate ratings were made for the socialization of males and females,

since we anticipated that many societies would differentiate strongly between the traits deemed appropriate for each sex. However, the original correlations between socialization codes for boys and girls were in excess of +.9, and the two variables behaved in virtually identical fashion in relation to each of the independent variables. This paper therefore reports only the correlates of socialization values for boys; however, the results for the socialization of girls are identical for all relevant purposes. The intercoder correlation for socialization values was +.503, based on 100 analyzable cases.[12] Final scores were assigned by summing the two coders' ratings.

The data were analyzed by means of partial correlation techniques. This method produces readily interpretable estimates of the correlation between the dependent variable and each independent variable net of the effects of other independent variables. In addition, an estimate of total variation explained by all independent variables was obtained from a regression analysis. Methods of causal modeling, including path analysis, are not employed here since we are interested solely in the extent to which socialization values may be explained by the multiple indices of closeness of supervision over adults. The theory to be tested does not require specification of a causal order among the independent variables.

FINDINGS

Pearson product-moment correlation coefficients between all possible pairs of variables in our model are reported in table 1.[13] Correlations between conformity versus self-reliance in socialization and the independent variables are reported in column 8. At the bivariate level each hypothesis except one is clearly supported. Conformity relative to self-reliance as a value in socialization is positively related to economic complexity ($r=.479$), political complexity ($r=.411$), mode of descent ($r=.360$), an-

[12] As a check for the effects of coder error, 11 cases on which coder disagreement exceeded 2 points on the seven-point scale were temporarily dropped from the analysis. The intercoder correlation for the remaining 89 cases was + .751. All analyses were then performed for these 89 societies, and results compared with those obtained from the 100 cases with nonmissing data on this variable. Again, there were no significant differences in the behavior of the socialization-values variable. In the interest of maximizing sample size, we therefore report results obtained from the total sample for which data are available in spite of the relatively low intercoder correlation.

[13] All correlations were originally computed using both listwise and pairwise deletion of missing values (Nie et al. 1975, pp. 312-13). The results obtained with each option were essentially identical. Consequently the analysis with pairwise deletion is reported here. This has the advantage of utilizing as much of the data as possible. In addition, an analysis-of-variance test for nonlinearity was conducted for all possible bivariate relationships (Blalock 1972, pp. 411-12). No significant violations of the linearity assumption were detected. However, see table 3 in the Discussion section below.

TABLE 1

BIVARIATE CORRELATIONS AND UNIVARIATE DISTRIBUTIONS

Variable	1	2	3	4	5	6	7	8	$\bar{X}$	SD	N
1. Economic complexity . . .		598	204	190	.099	.367	.178	.479	5.03	1.99	121
2. Political complexity. .			.035	.029	.057	272	124	.411	2.03	1.18	122
3. Religious taboos. .				345	.109	.193	.094	.313	6.64	2.15	115
4. Ancestor worship. . .					183	.248	.112	.335	3.43	.82	120
5. Family complexity. .						.055	.112	.016	2.37	1.17	122
6. Mode of descent*							232	.360	2.07	.92	108
7. Control over mate choice								.322	2.01	.73	122
8. Conformity/self-reliance†									8.00	2.74	100

* High scores are assigned to patrilineal systems, low scores to bilateral systems. Matrilineal systems are intermediate.

† High scores indicate emphasis on conformity.

cestor worship ($r = .335$), control over mate choice ($r = .322$), and religious taboos ($r = .313$); each of these relationships was predicted by the theory. One hypothesis is not supported: conformity relative to self-reliance is unrelated to family complexity ($r = .016$).

Since the independent variables are intercorrelated to varying degrees, it is possible that they account for some of the same variation in the dependent variables. To determine the correlation between each independent variable and the dependent variable net of all other independent variables, partial correlation analysis was employed. Table 2 reports the sixth-order partial correlations between conformity/self-reliance and each independent variable controlling for all other independent variables. In most cases the relationships are attenuated by the multivariate controls; however, in each case a substantial association remains.

Family complexity, as in the bivariate analysis, is uncorrelated with conformity versus self-reliance when the other variables are controlled ($r = .009$). Also, the correlation between mode of descent and conformity is reduced to a nonsignificant level ($r = .114$) by controlling for the other independent variables.[14] However, the remaining partial correlations are significant in both statistical and substantive terms. The magnitudes of these partial correlations are similar, ranging from .188 (religious taboos) to .239 (control over mate choice). The seven independent variables in the

TABLE 2

BIVARIATE AND SIXTH-ORDER PARTIAL CORRELATIONS BETWEEN INDEPENDENT VARIABLES AND CONFORMITY VERSUS SELF-RELIANCE

Independent Variable	Bivariate Correlation	Partial Correlation	N
Economic complexity . .	479	193	82
Political complexity . .	.411	.225	82
Religious taboos . . .	.313	188	82
Ancestor worship	335	213	82
Family complexity . .	016	009	82
Mode of descent* . .	360	.114	82
Control over mate choice . . .	322	239	82
	Multiple $R = .635$	$R^2 = .403$	

NOTE.—High scores indicate an emphasis on conformity.
* See table 1 for variable metric.

[14] This partial correlation has a probability of .148 (one-tailed test) and is, therefore, not significant at conventional levels. All other partial correlations are significant beyond the .05 level, and all bivariate correlations are significant beyond the .001 level. However, tests of significance have limited meaning, given the nature of cross-cultural data. While the societies included in the Standard Cross-cultural sample and the Human Relations Area Files are generally considered to be fairly representative of known nonindustrial societies, they do not constitute a probability sample. Hence significance levels are not reported in the tables.

analysis yield a multiple correlation coefficient of .635 and collectively explain 40.3% of the variation in the dependent variable.

These results support the proposition that an emphasis on conformity over self-reliance in the socialization process is fostered by recurring structured situations in economic, political, religious, and familial domains which require adults to conform to external authority. Where the behavior of adults is more autonomous in these areas, self-reliance is more highly valued in children.

DISCUSSION

Of the seven hypotheses relating closeness of supervision to emphasis on conformity over self-reliance in socialization, five were clearly supported. The two variables which do not behave according to the predictions of the theory are both family related. Family complexity is simply uncorrelated with socialization values, and the correlation between mode of descent and socialization values is greatly reduced when the effects of the other independent variables are removed.

The ineffectiveness of family complexity as an antecedent of socialization values is particularly surprising, since it is clearly implicated by the theory and since Olsen (1974) found marked differences in socialization values between nuclear and extended families in her Taiwanese sample. Her results supported the theory in this respect; ours do not. The relevant question here is whether the problem lies with the theory or the measurement—that is, the fit between the abstract concept of closeness of supervision and the empirical indicator, family complexity.

Olsen (1974) compared the values of mothers in nuclear families with those of mothers in extended families. However, the extended families in her sample were actually of only one specific type: patrilocal stem families. Our measure of family complexity distinguishes between nuclear, stem, small extended (lineal), and fully extended family systems, without regard to postmarital residence patterns. Mean conformity scores by family complexity and postmarital residence are shown in table 3. The difference observed by Olsen between nuclear and small extended families in patrilocal societies is clearly replicated by these cross-cultural data: in patrilocal systems socialization for conformity relative to self-reliance is highest in small extended families and lowest in nuclear family systems. However, societies with fully extended patrilocal families place *less* emphasis on conformity than those with small extended families. In societies with residence systems other than patrilocal (neolocal, bilocal, matrilocal, or avunculocal), conformity scores are uniformly lower and do not covary with family structure in any systematic fashion predictable from the theory.

TABLE 3

CONFORMITY VERSUS SELF-RELIANCE BY FAMILY COMPLEXITY AND POSTMARITAL RESIDENCE

Residence	Family Complexity		
	Nuclear	Small Extended*	Fully Extended
Nonpatrilocal	7.7 (15)	6 7 (12)	7.2 (9)
Patrilocal.	7 9 (21)	8.8 (23)	8 4 (11)

NOTE.—High mean scores indicate greater relative emphasis on conformity, N's in parentheses.

* Includes both stem and lineal family types.

The problem here may well be one of measurement—that is, the epistemic correlation between our abstract construct (supervision) and our empirical indicator (family complexity). The relevant variable appears to be the extent to which the mother is directly and immediately supervised by senior members of her husband's family, particularly her mother-in-law. In nonpatrilocal residence systems the mother-in-law and the daughter-in-law are members of different households and families, regardless of family structure. In nuclear family systems, patrilocal postmarital residence usually means that the couple resides in the same community as the husband's parents, but not in the same household. The implications of this for supervision of the mother by her husband's mother are, it appears, minimal. In fully extended families, several daughters-in-law may be simultaneously present, and family size is also likely to be quite large. This may reduce the immediacy of the supervision of each mother by senior females. Thus, while our logic was not sufficiently refined to predict the pattern shown in table 3, these results are not necessarily contrary to the general theory. Supervision of the mother may well be highest in small, extended, patrilocal families. The nonlinear relationships and interaction effects observable in this table also show why no linear relationship is observed between family complexity and socialization values. Our results do not contradict those of Olsen (1974) but rather show that the relationship becomes much more complex when the range of the independent variable (family complexity) is expanded (see also Minturn and Lambert 1964).

It is also important to consider here the fact that our measurement of the properties of family complexity, as in any cross-cultural study, pertains solely to the cultural level of analysis. That is, system properties reflected in the measurement of family complexity are indicative of ideal or preferred family types. As Levy (1965) and others have shown, extended family systems are not uniformly populated by extended families; there

are many conditions (such as low life expectancy) which inhibit the general attainment of this ideal by individual families. This means that, in any given extended family system, only a variable proportion of families are in fact extended (see Goode 1963). Thus there is considerable slippage between the variable of family complexity and the abstract concept of supervision by family elders. This may account, in part, for the small correlation between family complexity and socialization values.

This study provides further evidence that, at least in terms of selected parameters, Kohn's theory of social class and socialization values may be generalizable beyond the bounds of American and Western cultures. But more important, the range of independent variables implicated by this theory has been expanded. The seven independent variables employed here are admittedly rough indicators of the extent to which adults are subject to requirements for conformity in several aspects of their daily lives; alternatively, they may be viewed as inverse indicators of the utility of self-reliance or autonomy. Collectively, they explain approximately 40% of the variation in socialization values along a dimension of conformity/self-reliance. The findings of this study support the proposition that adults value in their children traits or qualities which they have found to be instrumental to their own success in a variety of behavioral domains, including the familial, religious, and political, as well as the economic.

This study is obviously not a replication or extension of Kohn's complete theory. Instead, we have concentrated on certain aspects of this theory which relate most directly to previous theoretical formulations in anthropology and comparative sociology. However, the fact that hypotheses pertaining to many areas of social life may be derived from only a small portion of Kohn's theory, and were supported on a cross-cultural sample such as this, indicates that the scope and explanatory utility of the theory may be much broader than we have previously believed.

An ability to explain a significant proportion of the variation in socialization values is, however, of little theoretic utility unless these values themselves have demonstrable consequences for either parental behaviors or the outcomes of the socialization process. Possible connections between socialization values and parental behaviors in the child-rearing process are currently being investigated through cross-cultural research and will be the subject of a subsequent paper.

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Growth, Politics, and the Stratification of Places[1]

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Systematic inequalities among interdependent places are described here as a dimension of stratification of persons and organizations. The "stratification of places" is compared with the classic dimensions of class and status as a basis of collective action, and it is argued that the competition of places is a significant cause of the territorial differentiation of human communities.

This is an essay on the process of spatial differentiation of human communities. I argue that the differentiation of places implies sets of advantages and disadvantages for persons who are tied to each place and thus affects the chances for individual upward or downward mobility. A common response to this fact is a continuing collective effort to influence the pattern of development among places through political action. Places with early advantages, by making full political use of their superior resources, can potentially reinforce their relative position within the system of places. I hypothesize therefore that spatial differentiation tends to be transformed over time into an increasingly rigid stratification of places.

The study of the development of systems of places found its classical formulation in human ecology. By emphasizing the stratification aspect of spatial differentiation I am proposing a reorientation toward a more political human ecology, with spatial differentiation seen not only as the population's natural selective response to its habitat but also as a means of organizing inequality.

I. HUMAN ECOLOGY: A CRITICAL NOTE

In this essay I develop an ecological dimension of stratification, taking into account the functional interdependence of systems of places. However, the present perspective contrasts in important ways with human ecology as developed by Park and his associates (see Park, Burgess, and McKenzie 1967), especially in my emphasis on the political determinants of territorial

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differentiation. Zorbaugh ([1926] 1961) specifically discounted the sociological relevance of what he called administrative areas, as distinct from natural areas, and this distinction is carried over in Hawley (1950, pp. 258–59). Ecologists need not necessarily exclude political factors; McKenzie ([1926] 1961) in fact counted "political and administrative measures" among the "ecological factors" which shape the spatial relations among persons. Elsewhere (McKenzie 1933, pp. 158–70) he explicitly considered the competition among cities for favorable positions in an increasingly interdependent system of cities and such phenomena as local boosterism and conflicts over federal tax and expenditure policies. Yet even here McKenzie was primarily interested in the economic forces leading toward system integration, seeing political competition as a passing phenomenon, and as a rule human ecologists have ignored geopolitical units as corporate groups.

Whereas I stress the effects of collective action by communities in competition with one another, ecological theories of spatial differentiation have been based upon the analysis of the microeconomic competition of individual land users (Park 1936).

These differences are associated with a more basic divergence in perspective on the nature of community growth. The Chicago School sought explicitly to identify the processes of development at the "biotic level," that is, those processes which manifest the response of the human population to the same constraints faced by all living populations. (On this point see the criticisms made by Alihan [1938], Hollingshead [1947], and Firey [1947].) The community was understood as an adaptive mechanism which maximizes the efficient use of space and other resources under the pressure of population growth (Hawley 1950, pp. 66–68).

Assuming free competition for space, resolved according to the relative marginal utility of particular locations for competing land users, ecologists of the Chicago School could assert that the final highly differentiated ordering of space would be the most effective for the population. Thus, in Park's words, the process "results in the regulation of numbers, the distribution of vocations, putting every individual and every race into the particular niche where it will meet the least competition and contribute most to the life of the community" (Park 1952, p. 161). This perspective is complementary to the functionalist theory of class stratification, which is by now better known (Davis and Moore 1945). The similarity is visible in Hawley's discussion of the stratification aspect of spatial differentiation, where political inequality among places is seen as a natural, system-maintaining consequence of differentiation:

> A hierarchy of power relations emerges among differentiated units. Two consequences of differentiation contribute to that result. In the first place, inequality is an inevitable accompaniment of functional differentiation. Certain functions are by their nature more influential than others; they

> are strategically placed in the division of labor and thus impinge directly upon a larger number of other functions. . . . Secondly, mutual supplementation through functional differentiation necessitates a centralization of control. To insure the regular operation of the system there must be a sufficient governing and coordinating power vested in some one function. [Hawley 1950, p. 221]

The point of my discussion of the competition of places is that persons and organizations constantly seek to affect the growth process in order to maintain or create inequalities among places to their own advantage. The consequent stratification of places is therefore constructed by political action. Political, social, and economic inequality among places should be understood not only as the *result* of differentiation, but also as a *cause* of the particular pattern of differentiation which evolves. More precisely, the competition among places normally reinforces the existing stratification, because initial advantages—translated into political power—can be maintained.

This hypothesis of increasing stratification of places resulting from political conflict can be explored in a wide variety of cases. I will present two examples here, one at the level of suburban communities in a metropolitan system, the other at the level of nations in the world system. In both cases there is evidence of increasing inequality in recent years.

The data on suburban inequalities are taken from the case of the 89 suburban communities in the Nassau-Suffolk SMSA (Long Island, N.Y.) which were reported in the census in both 1960 and 1970. The indicator of wealth is median family income, which is relevant to both the social status and the fiscal strength of the community. During the period 1960–70, the standard deviation of median incomes—one indicator of the degree of inequality among suburbs—more than doubled. Even with the 1970 value deflated to control for increases in the mean (due to both inflation and real increases in personal income), the standard deviation increased by 58.9%, from $2,121 to $3,370 (adjusted) in the 10-year period. Initial differences among suburbs were consolidated and reinforced as a result of the growth process of the 1960s. Elsewhere (Logan 1976) I have shown how such structural changes in the spatial differentiation of the metropolis can be understood in terms of the interaction between competition among potential land users for desirable locations and collective action by communities to promote favorable growth patterns.

The data on international stratification are based on the 88 countries for which information is provided by Banks's (1971) cross-polity survey for both 1956 and 1966. Here the indicator of wealth is gross domestic product (GDP) per capita, a measure of the total goods and services produced by the national economy. During 1956–66 the standard deviation of GDP per capita increased by 34.2%, from $469 to $580 (again controlling for

changes in the mean). In the section on international migration below, I apply to systems of countries the same theoretical logic by which I have treated systems of communities. At both geopolitical levels, the flow of goods and people among interdependent places affects the relative position of those places in a stratified order, creating the conditions for politicization of the development process.

II. THE BASES AND PROCESSES OF THE POLITICS OF PLACE

I have asserted that patterns of territorial differentiation can be understood as stratified systems reflecting the power relations among places. In the following sections I develop this proposition theoretically, considering the relationship between the stratification of places and other dimensions of social stratification, the modes of aggregation and expression of place-based interests, and the relationship of the stratification and competition of places to the social system in which they are embedded.

My purpose at this point is not to propose a theory of places, but rather to put forward as a general orienting concept the notion of places as collective actors.

Class, Status, and Place

The uses of spatial relationships to express the class and status differences among individuals are well known. Physical proximity often represents social similarity or intimacy in face-to-face interaction. Even whole cities have been shown to be structured partly according to this principle, as the degree of residential segregation of class and status groups is directly associated with differences in their social position (Duncan and Duncan 1955; Lieberson 1961; Guest and Weed 1976): "The urban neighborhood becomes a highly visible manifestation of the status structure, and individual occupational careers come to be mirrored in one's residential movements. A home is not just where you live; it is a location in a well developed status ecology and, inferentially, a telltale clue to one's location in the occupational hierarchy" (Laumann, Siegel, and Hodge 1970, p. 524; see also Barber 1957, pp. 144–46).

Residential segregation creates a status hierarchy of neighborhoods defined simply by the characteristics of their residents, at the same time as common class or status becomes a symbol through which people identify their physical area as a community. The status hierarchy of places is reinforced by people's individual decisions to translate upward social mobility into change of place of residence.

But the spatial organization of persons is more than a representation of class and status differences acquired by birth, education, etc. Place of

residence itself affects the chances for social rewards to the degree that persons are tied to the advantages and disadvantages of places—for example, opportunities of employment and housing, level of income, cost of living, public services and tax rates, and legal rights and obligations. Like class and status groupings, and more substantially than many other kinds of associations, places are "communities of fate" (Stinchcombe 1965). Among others in the study of community, Molotch (1967, pp. 336–37) has emphasized the notion that "once people of the metropolis relate themselves to a certain area, their fortunes and futures become dependent upon the fate of the geographical unit to which they have become attached." More recently, Spilerman and Habib (1976) have shown that the stratification of types of communities reinforces the class stratification between established residents and recent migrants within Israeli society. This is not simply to argue the case for contextual effects (i.e., that one's aspirations and behavior are constrained by interpersonal relations within closed, homogeneous communities). There are characteristics definable at the level of a place itself—consequences of the place's economic and political relations with other places—which directly affect the quality of life and life chances of residents. Place is therefore a partially autonomous dimension of stratification in the same sense as the more familiar dimensions of class and status.

Competition of Places

It is because their fortunes are directly affected by the inequalities among places that persons and organizations continuously attempt to influence the development process through political action. Such action takes the form of efforts to determine the goals of local growth policies as well as competition among places to affect decision making at higher geopolitical levels. In either case, interests of place are commonly aggregated according to the territorial boundaries of politically defined places. Such boundaries bind together the many otherwise divergent elements within places, subjecting all within to significant characteristics of the geopolitical unit itself, the most fateful being its power in relation to other places. A "place" then is defined as much by its position in a particular web of political institutions (such as boundaries and constitutionally determined legal powers) as by the physical area it occupies. That is, whatever the inherent physical and population characteristics of naturally defined communities, these characteristics become resources and liabilities for residents according in part to the political organization of society.

Illustrations of this point are numerous. The concentration of employment in some suburban communities is a resource if public services are financed through local property taxes and if zoning can be used to externalize service burdens by forcing many local employees to live elsewhere.

Ghettoization in the metropolitan core is a liability to central cities if welfare costs are borne by the city; therefore urban-based individuals and groups have pressed hard for federal takeover of these costs. Discovery of oil in Alaska benefits that state if it can tax mineral resources, it benefits Chicago if the federal government can be made to mandate a Trans-Canadian Pipeline to distribute the oil to the Midwest, or it benefits California and Japan if oil companies are free to choose a more profitable Trans-Alaskan route. The great physical resources of Angola became a benefit to that territory only after decolonization, and production of commodities for export is being converted from a liability to a resource only to the degree that Third World countries organize effective international cartels.

In conflicts over boundaries, constitutional powers, allocations of public resources, taxation policies, land use controls, etc., places compete for development outcomes which would maintain or improve their relative position in the hierarchy of places. More precisely, coalitions of local interests—recruited and organized along territorial lines determined by political boundaries—compete for outcomes in which coalition partners have a mutual interest, even when at another level their interests may diverge. This is not to say that all internal groups have the same interest in development or that all are equally represented in the definition of local goals. A large research literature on local and national power structures suggests quite the opposite. My point here is that, by providing a communality of interests among internal groups, place accounts for political behavior which cannot be understood in terms of class conflict (e.g., the cooperation of banks, municipal unions, and city government to forestall bankruptcy of New York City). Granted that appeals to national patriotism or local pride are sometimes manipulated as ideological symbols to defuse internal conflict, people and organizations are in fact bound together by the places in which they live or have invested. To this extent it is meaningful to analyze the development process in terms of the competition of places.

Because places tend to be functionally differentiated from one another, it may be difficult to distinguish between those common interests of local persons and organizations which derive from *place* and those which derive from *economic sector*. "Downtown" and "the ghetto," "Detroit" and "the Farm Belt" are all place designations which carry a clear functional connotation. The fortunes of almost everyone in Detroit, for example, depend upon the fortunes of the automobile industry. Probably the common interests of Detroiters are more powerfully represented by General Motors than by the Detroit city government. Thus the competition of places is closely tied to intersectoral conflicts which are channeled through major business or labor organizations. In principle, however, these are two quite different bases of political action. Intersectoral conflicts correspond to the interest

group politics traditionally studied in political science. They become relevant to the competition of places insofar as (1) they affect spatial differentiation, (2) they become understood and acted upon as issues of place, or (3) the definition of local interests is imposed by the organization which controls the local economy, so that local government becomes an instrument of intersectoral conflict.

Social Movements of Place

I have argued that systems of place become ordered partly as a result of competition among places which represents really the search for locational advantages by persons and organizations. That is, the growth process is an interaction of social movements, protecting territory or advancing claims for collective advantage, simultaneously within and among communities (see Molotch [1976, p. 311] and Harvey [1973, pp. 72–73], for statements of a similar position).

The shifting coalitions of political actors referred to here have at least some of the characteristics of social movements even when they do not involve public mobilization. The most important in my view is that the diverse members of these coalitions act to promote the *collective* good in which they share. Sometimes, nonetheless, issues of the competition of place give rise to or reinforce broad popular movements. In these cases the movements are strengthened by the normal overlap of stratification of place with the geographic segregation of persons by class and status. The development process which causes places to be differentiated from one another on the bases of class and status has consequences for the actualization of these classic bases of collective action. Place may so overlap with class and status (race, religion, culture, language) that it may provide an ecological support for organization as well as a symbolic sense of community, at the same time as being itself an objective basis for common action. Berry et al. (1976), noting the common use of place of residence to support the "status claims" of individuals, use this fact to explain the frequency and intensity of solidary action by community residents in opposition to racial integration. Similar reasoning could be applied to movements against busing between white and black neighborhoods or to apartment construction in single-family areas. Blauner (1969) attributes ghetto revolt to a corresponding protest in the black community against external exploitation and containment in the central city. At another geographic level, the best-known recent examples of violent regional nationalism (as in Northern Ireland, Biafra, and the Basque region of Spain) have resulted from a sense of central government exploitation of places which have a distinct language, culture, religion, and/or economic structure, and much the same may be said for colonial independence movements (see Hechter 1975).

Migration: Individual Mobility in the System of Places

Social movements of place occur to the degree that persons and organizations identify their interests with the future development of the places with which they are associated. There are alternative individual responses to the stratification of places, by which persons seek to manipulate the inequalities among places to their own advantage. Studies of the international system, for example, have long recognized that part of the stability of dependency relationships is due to the cooperation of persons in dependent countries who are able to adapt their interests to continued foreign domination (Baran 1968, pp. 194–96). Perhaps more commonly, residents of disadvantaged places perceive migration as the most effective means of upward mobility.

The phenomenon of migration is especially interesting theoretically because it involves the stratification of both individuals and places and the interaction that occcurs on both the individual and the community level in the search for advantages of place. From the perspective of free-market theories of population movement (e.g., Tiebout 1956) migration would minimize the disparities among places as the distribution of persons came to match the distribution of resources. But of course migration is not free. Whether by residential zoning policies or police control of their borders, places regulate migration according to their own interest, and individual efforts to move upward through migration may actually be made to reinforce the system of inequalities among places. Thus the stratification of places can be maintained not only by the outcomes of conflicts among places but also by the ways in which noncollective responses are structured.

Consider the example of migration of workers between the industrial countries of the Common Market and the less-developed Mediterranean countries in the postwar period. First, migrants in this system are assigned social and legal status inferior to that of native workers. Their presence (given full employment) provides a relative class and status advantage to natives, increasing the chances of upward mobility for them and defusing native class militancy (Castles and Kosack 1973). Emigration in turn makes available a nonpolitical response to economic discontent in disadvantaged places (MacDonald 1963). Migration complicates individuals' perceptions of the lines of stratification within and between places and reduces potential opposition to the system as a whole. Second, the division of labor by which some places provide worker reserves for others can be manipulated to the advantage of the latter. Migration can guarantee a sufficient work force to an expanding economy and allow externalization of the political and economic costs of unemployment in periods of contraction; both processes can reinforce the initial division of labor (Castells 1975).

Systems of Places: Levels of Analysis

The notion of competition of places is applicable to systems of places at any geopolitical level—to systems of neighborhoods, cities, regions, and nations. It reflects the fact that within any system (whether the Western world order described by Wallerstein [1974], or the New York metropolis analyzed by Wood [1961]) the growth potential of places is affected by their political-economic position in relation to other places.

Despite their similarities, there are important differences among levels. One is the strength of the political institutions which integrate the systems and the degree of sovereignty of the places within them. The nation-state is clearly the strongest political unit in much of the modern world, determining by law the formal channels of competition of places within the national system, while conflict among nations is only loosely regulated by international structures. The smallest geopolitical units—administrative areas within cities, for example—have the least sovereignty but are the most homogeneous in terms of the interests they must serve.

But beyond comparing these levels as parallel systems of places, it is crucial to discern the ways in which they are interrelated. Systems of neighborhoods are nested within systems of cities within systems of nations. The political process within any system involves not only local places but also interests which are organized at the system level. The latter groups—especially those at the national level—can greatly influence the pattern of interlocal competition by setting the legal framework within which it occurs (see Holden [1964] and Farkas [1971] for discussions of the effects of federal law on interlocal conflict in the United States).

Using a Marxian perspective, some theorists have suggested that the inequalities among places are most relevant, not for their effects on interests based within competing places, but for their consequences for the maintenance of the system as a whole. In his study of migratory labor systems, for example, Burawoy notes that the geographic separation of the sites of renewal and maintenance of the labor force—made possible by the unequal power relations among places—makes possible a reduction in the total costs of reproduction of the labor force. "It is cheaper to educate and bring up a family, and so forth, in a Bantustan or a Mexican shantytown than in Johannesburg or California, where the reproduction of labor power is organized for higher-income groups and where, as a result, lower-income groups are penalized" (Burawoy 1976, p. 1082).

Harvey (1973, pp. 261–84) has made a much broader claim for the role of stratification of places in maintaining capitalism. Describing the spatial ordering of Western capitalism as a global structure in which the industrial metropolis rests at the top of a chain of exploitation of places, he argues

that such a hierarchical organization of territories has been necessary for the concentration and efficient circulation of surplus value. The particular geographic pattern depends upon both economic and political factors of the type discussed in this essay:

> The exhaustion of a key resource and the opening up of new resources (through technology or the opening up of new trade routes) can bring about rapid shifts in the circulation of surplus and bring powerful and important cities into being, and can just as quickly destroy them . . . In contemporary times, the shifting allegiance of nations, the interdiction of trade through political action (the partition of Germany, the closure of the Suez Canal) have all affected circulation of surplus. Competition between cities, between sets of cities (such as the Hanse) or between countries, for control over the circulation of surplus will itself alter the geographic pattern of circulation as one side dominates the other . . . [Harvey 1973, p. 247]

But Harvey is less interested in the particular geographic pattern than in the fact of territorial inequality itself.

Like other recent theorists, he emphasizes the increasing role of the state in preserving this system: by protecting the continued flow of surplus through military and police control, by assuming responsibility for the provision of facilities and services for the maintenance of the population and reproduction of the labor force, and by providing infrastructural support and financing for the profitable expansion of private industry. As others have pointed out, there are potential contradictions in these roles, because costs of social control are inherently unproductive expenses and because state revenues cannot keep up with expenditures as long as profit is privately appropriated. O'Connor (1973) and Castells (1972) have suggested that the interrelated fiscal crises and urban political movements which result from these contradictions jeopardize the system itself. On the contrary, I would point to other characteristics of the political organization of the stratification of places which tend to insulate it from such challenges:

1. Governmental fragmentation enables public resources to be concentrated in certain jurisdictions. Thus the fiscal crisis, because it is unequally distributed both within and across metropolitan regions, affects various economic sectors and social classes differentially.

2. Potential opposition to the system tends to become organized within places rather than at the system level. Such action is relatively ineffectual, because disadvantaged places have neither the internal resources nor the power in relation to other places to resolve the problems which generated their political activity.

III. CONCLUSION

At every geographic level the competition of places affects the pattern of development and differentiation of the human community. Spatial differentiation does in practice imply inequalities among places, and thereby advantages and disadvantages to the persons and organizations whose fortunes are linked to specific places. The more powerful of these actors typically established political structures which reinforce the stratification of places to their own advantage. The routine working of such structures insulates the system from challenge. It is rare that places disadvantaged by the system can oppose it directly, as the OPEC countries have done temporarily within the world system. And opposition from within advantaged places is muted by the fact that the advantages of place are widely shared by its residents.

I have suggested that place is often an important basis of collective action and that the notion of stratification of places can usefully supplement the more traditional dimensions of class and status. The interactions among these three dimensions provide a rich field for theoretical development.

I have also argued that in order to understand how growth takes place and is socially ordered, one must take into account the conscious efforts of places to influence growth. This approach does not deny the ecological variables of distance and time, but asserts the importance of another political set of variables. It does no more than make positive use of what the Chicago School also recognized (Park 1936; Wirth 1945; Hawley 1950, pp. 55–63; and esp. Hawley 1971, pp. 49–54) but seems too often to have neglected, that the human community itself more than any other life form creates the conditions of its own development.

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Is It True What They Say about Daddy?[1]

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The traditional method for assessing generational mobility has been exposed to remeasurement studies but not to the test of an alternative questioning strategy. This paper describes a design in which intergenerational mobility is studied from two perspectives. In the conventional approach sons report on their own statuses and give proxy reports for parental statuses. In our second perspective parents are questioned about themselves and give proxy information on their children. Estimates of a five-variable status-attainment model for each perspective yield roughly similar results, but some correlations and regressions differ significantly because nonrandom errors influence the reporting of socioeconomic data among adults. Our anomalous findings suggest that mobility researchers might consider employing both questioning strategies as a guide to potential biases until a definitive test shows that the caution prescribed in this paper is unwarranted. Consumers of mobility studies are entitled to reserve judgment on data that depend entirely on the traditional approach even though current research adheres to it almost without exception.

Most research on mobility rates between the generations and patterns of status attainment has been based on sample surveys of men who are asked about their background and current status (Broom and McDonnell 1974 [1977]). It is assumed that adults can readily recollect salient events that occurred during their adolescence and report accurately their father's job at that time, his schooling, and other family information. Although Bielby, Hauser, and Featherman (1977) show that responses to questions asked on a second occasion may agree closely with responses given at a first questioning, consistency of response may mean merely that the past is reconstructed in much the same way each time a question is asked. Remeasurement cannot address the troublesome question of bias in the original answers on parental status. Adults can be expected to produce much the same "construction" of their childhood when questioned twice, especially if the remeasurement interval is short.

Where data on fathers are obtained from sons, one could test the accuracy of the responses by searching out surviving fathers and securing the same information from them. Assuming that the father reports his own

[1] We express our thanks to Trevor Williams for helpful comments.

present and prior statuses reasonably accurately, a direct and independent check on the son's responses would be available. But there are obvious disadvantages to this procedure apart from the cost of finding fathers who might live far from their son's residence. The crucial test of the accuracy of proxy reporting would be based on the oldest respondents' reports since they are asked to recall facts from a distant adolescence. But many of the fathers would be dead or untraceable for interview. To reduce costs, sons could be asked to forward a brief protocol to their fathers, but such an approach would reduce coverage and probably introduce sampling bias. It is difficult to conceive of a design that would avoid such severe practical problems.

There are some studies in which both fathers and sons were interviewed, but they largely concern students coresident with their parents (e.g., Niemi 1974, chap. 2). Mason et al. (1976) deploy sophisticated measurement models to assess the reliability of proxy reports on parental socioeconomic statuses by school-age sons, with encouraging results for whites in the last year of secondary schooling, less encouraging but still reassuring results for younger white adolescents, and rather disappointing results for blacks. In their case the availability of reports on parental status by both parents and sons allows them to test directly the plausibility of various assumptions about measurement error in proxy reports. But we have no way of knowing whether these findings can be generalized to cases where respondents are no longer living with those about whom questions are asked.

This paper reports the results of a study which used two questioning strategies to obtain data on the social statuses conventionally used in status-attainment research. In the first approach proxy reports are secured for parents, in the second proxy reports are secured for children. As we show below, the two strategies give different results.

TWO PERSPECTIVES ON SOCIAL MOBILITY

A survey of social mobility and stratification in Australia (referred to as ANU 1973) was designed to permit analysis of status transmission over three rather than the customary two generations. The data afford an opportunity to deal with father-to-son mobility both from the traditional perspective 1 (per 1) in which the son reports on himself and his father, and from our perspective 2 (per 2) in which the father reports on himself and his sons.

Asking fathers about their children (per 2) has the drawback that a child's occupational career may not have begun, much less stabilized, at the time the father is interviewed. This was partly solved by including respondents up to age 69, old enough to have children well launched on their

careers. An older ceiling would have been desirable, but in view of the brevity of the human life span, the frailties of body and mind, and differential mortality by socioeconomic status, a cutting off point of 69 seemed the best compromise. Respondents were asked to report the following information for each child: age at leaving school, first job, country of residence at that time, and, if 21 years or older, present job and residence. We also asked the respondent to report the facts ordinarily elicited in social mobility surveys: his own job history and educational attainment, his father's job when the respondent was 14 years old, and the age at which his father completed his full-time education.

A father's description of his son's career should be at least as accurate and possibly more accurate than a son's report of his father's. By the time the son first perceived his father as an employed person the father's career was well developed. But fathers are mature at the start of their sons' careers. A father is presumably a closer and more interested observer in his son's career, which he had a chance to influence. From the standpoint of knowledge and awareness of adult roles, the father seems the more reliable and accurate source if a choice must be made.

DATA, MODELS, AND MEASUREMENT

The details of the ANU 1973 national survey are presented elsewhere (Broom et al. 1977*a*, chaps. 2 and 3). The sample design was based on the Current Population Survey of the Australian Bureau of Statistics. The final sample consisted of 3,166 men and 1,773 women between the ages of 30 and 69.

In this paper we assess whether the two perspectives yield different results by comparing the parameters in a basic model of status attainment estimated from two data sets: one from per 1, in which sons report on fathers, and the other from per 2, in which fathers report on sons. Such a test requires comparable subsamples of father-son pairings, and we briefly review the procedure followed to achieve comparability.

The per 1 respondents selected were those between 30 and 49 years old in 1973 whose fathers were between 50 and 69 and resided in Australia in 1973. Thus, this subsample is restricted to sons whose fathers could have been included as respondents in the survey. The per 2 subsample consists of respondents between 50 and 69 years old who had one or more sons aged between 30 and 49 living in Australia in 1973, that is, fathers whose sons could have been respondents in the survey.

The exclusion of ineligible respondents left 447 respondents aged 30–49 in per 1 and 304 respondents aged 50–69 in per 2. Some of the latter are represented by more than one eligible son: 199 of the older respondents

had one eligible son, 72 had two, 25 had three, six had four, and two had five. Only cases for which data are complete have been retained for analysis. We should note that the data reported here are a little different from an earlier statement because of slightly modified inclusion rules (Broom et al. 1977*a*, chap. 8, and 1977*b*).

Since only one interview was conducted in any household, these subsamples can be viewed as independent samplings of father-son pairs where information was obtained from a father or a son but not both. This independence may be compromised by two possibilities. Any tendency for fathers and sons to live in the same block might result in unequal probability of selection for some father-son pairs. However, we think this source of distortion is negligible because on the average interviews were conducted in only one-fourth of the dwellings in sampled blocks. Second, whereas in per 1 the chance of a father being represented in the sample is proportional to the number of his eligible sons, in per 2 a father is represented as many times as he has eligible sons. Thus, a degree of status homogeneity is introduced into per 2. For multiply-represented father respondents education is invariant, and job status nearly so, while for sons groupings of brothers will be more alike in the statuses than men taken at random (Sweetser 1975; Sweetser and McDonnell 1978). Our results are not affected by such intraclass homogeneity (see below).

We emphasize that the father-son pairs in per 1 are not the same as those in per 2. The specified inclusion rules generate independent samplings of an eligible population of father-son pairs. If these pairs were the same in both perspectives a direct assessment of measurement error would have been possible.

Occupational data were scored according to the ANU II status scale (Broom et al. 1977*a*, chap. 5, and 1977*c*). In per 1, father's occupation when the respondent was 14 years old, respondent's first job, and respondent's job in 1973 were asked directly. Respondent's first job was defined as the first regular job held in the first full year out of school. (Only 5% of men sampled returned to full-time education after such a period of employment.) For per 2, first job is defined simply as the first job after completing full-time education, so that there is some small scope for discrepancy between perspectives in the measurement of first job. In per 2, the father respondent's job "when the son was 14 years old" is inferred from the respondent's career profile and closely approximates that age (see Broom et al. 1977*a*, p. 151).

Before discussing results, we should comment on significance level. Since we have small subsamples selected from a multistage areally clustered sample, a simplistic application of conventional test statistics is inappropriate. Previous work using the jackknife technique indicates that deflating

the number of cases by a factor of .73 will allow significance tests to be applied fairly accurately (Broom et al. 1977*a*, p. 181, and 1977*b*, p. 156). While the subsamples are designed to be comparable, certain minor differences in the information collected encourage the choice of a stringent significance level, and we use the 1% level in this paper.

ANALYSIS AND RESULTS

Table 1 presents for both perspectives the means, standard deviations, and correlations for the basic five-variable Blau-Duncan model of status attainment. No pair of means is significantly different in terms of the usual t-statistic. However, standard deviations are uniformly smaller in per 2, significantly so in the case of son's education, using the conventional χ^2-test. The difference in standard deviations for father's education approaches significance ($\chi^2 = 3.970$; $.05 > P > .02$). Common sense suggests that one should expect larger error and thus larger dispersion for proxy reports than for self-reports. Respondent fathers reporting on themselves in per 2 do have lower variance on education than the proxy reports of per 1, but there is less variance in the proxy report of per 2 for son's education than in the self-report of per 1.

In an analysis not reported here we selected one son at random in per 2. In only one case was the size of a correlation reduced (r_{23} fell from .407 to .401), and other differences were similarly trivial. We therefore conclude that intraclass homogeneity does not materially affect our analysis. One of the two significant differences in table 1 is central to the analysis of intergenerational mobility. In per 1 we find that father's job when his son was 14 years old accounts for only 8.4% of the variance in his son's current occupational status, whereas in per 2 the proportion of variance explained is three times as great. A glance at table 1 shows that in general per 2 has higher correlations.

The implications of the differences between perspectives for models of status attainment are reported in table 2, which presents path (standardized) and path regression (metric) coefficients for three structural equations: determinants of son's education, son's first job, and son's present job. The main differences between perspectives can be readily summarized: in per 2 (asking fathers about sons) social origins explain twice as much of the variance in son's education as in per 1 (asking sons about fathers), a difference with substantive import for assessing ascription versus achievement in status-attainment processes. The influence of father's education emerges as more important when fathers give the information. Education is scored with a range of 20 years and occupation on an extended scale with an effective range of about 500 status points. Thus a difference as

TABLE 1

ZERO-ORDER CORRELATIONS, MEANS, AND STANDARD DEVIATIONS FOR A STATUS-ATTAINMENT MODEL IN TWO PERSPECTIVES

	Status Variable					Perspective 1	
	1	2	3	4	5	X	SD
Status variable:							
1. Father's education..	1 000	350	.277*	.149	197	14.578	2 236
2. Father's job.....	358	1 000	268	.350	*.290*	512 373	104 020
3. Son's education..	.439*	.407	1 000	*585*	.607	16.131	2.837
4. Son's first job..	.308	.434	*727*	1 000	607	499 351	100.740
5. Son's present job.......	310	*503*	598	649	1.000	544.369	126.660
Perspective 2:							
$\bar{X}$..........	14.578	501.837	15 973	493 633	535 674	..	
SD.............	2.040	103.588	2.226	92.335	119.784	...	...

NOTE.—Perspective 1 values above diagonal; perspective 2 values below diagonal. Asterisked coefficients differ at 5% level, italicized ones at 1% level. The first two variables are proxy reports in perspective 1 but self-reports in perspective 2; the last three are self-reports in perspective 1 but proxy reports in perspective 2.

TABLE 2

STANDARDIZED AND METRIC COEFFICIENTS FOR A STATUS-ATTAINMENT MODEL IN TWO PERSPECTIVES

Dependent Variable	Persp.	Independent Variable: DADED	DADJOB	EDUC	JOB1	R^2/Intercept
		Standardized Coefficients				
EDUC. .	1	.209	.196	.	...	.110
	2	336	287	.	. .	.264
JOB1....	1	−*.083*	.233	545*	...	.388
	2	−*.053*	.177	.679*	...	.554
JOBN...	1	*.020*	*.055**	.372	.367	.468
	2	.009	.246*	.216	.383	.504
		Metric Coefficients				
EDUC .	1	.253	005			9.702
	2	367	006	.	. .	7.535
JOB1 .	1	−*3 596*	.225	19.366*		123.946
	2	−*2 399*	.158	28 148*		− 452
JOBN ..	1	1.111	*.066**	16 591	462	−4.175
	2	*.558*	.284*	11 594	.496	−45.459

NOTE.—Italicized coefficients do not exceed three times their standard error, asterisked ones differ at 1% level. Persp = perspective; DADED = father's education, DADJOB = father's job; EDUC = son's education, JOB1 = son's first job; JOBN = son's present job.

small as 0.001 in the job effect (table 2, lower panel) represents nearly a year in the predicted level of son's education if his father was an upper professional (Broom et al. 1977*a*, p. 103).

Among the examined determinants of first job status, son's education is by far the most important influence and is significantly stronger in per 2. While the effect of father's job is less important, it is stronger in per 1 than in per 2. As we would expect from the higher average correlations, per 2 accounts for more of the variance.

For present job son's education is also an important influence, but in this case it is stronger in per 1 whereas father's job is significantly more important in per 2. This is a reversal of the pattern observed for the determinants of first job. In terms of real status effect we do not want to overemphasize the differences in the model between the perspectives. Nevertheless, the higher intergenerational correlations of per 2 lead to at least three consequential differences: (1) the greater dependence of educational attainment on social background in per 2, (2) the relative effect of son's education on son's first job, and (3) the relative effect of father's job on son's present job.

As we mentioned, this result is not caused by the multiple inclusion of some fathers in per 2. Nor is it caused by random error in self- versus proxy reports. In per 1 variables 3, 4, and 5 are self-reports, and in per 2 variables

1 and 2 are self-reports. If self-reports have less error than proxy reports, the correlation coefficients r_{34}, r_{35}, and r_{45} should be higher in per 1 than in per 2, whereas the correlation r_{12} should be higher in per 2. But as table 2 shows, the correlations r_{34} and r_{45} are higher in per 2 (in one case significantly so), while r_{12} shows no appreciable difference between perspectives. On the basis of this evidence we should either draw the counterintuitive conclusion that proxy reports are more accurate than self-reports or assume that measurement errors are not random in these data.

We cannot investigate directly the effects of measurement error on our data. However, we can illustrate the implications of these discrepant results with a simple model of measurement error. Consider only the solid lines in figure 1 and assume that measurement error is greater in proxy than in retrospective reporting and that measurement error is greater in proxy than in self-reports. Using the central correlation between father's job and son's current job, the model says that there are two fallible reports of a father's and a son's job. If the fathers in per 1 were the same as those in per 2 we could make a direct attack on measurement error. As it is we can only illustrate the substantive differences yielded by the two perspectives and show that they cannot be resolved in terms of a simple model.
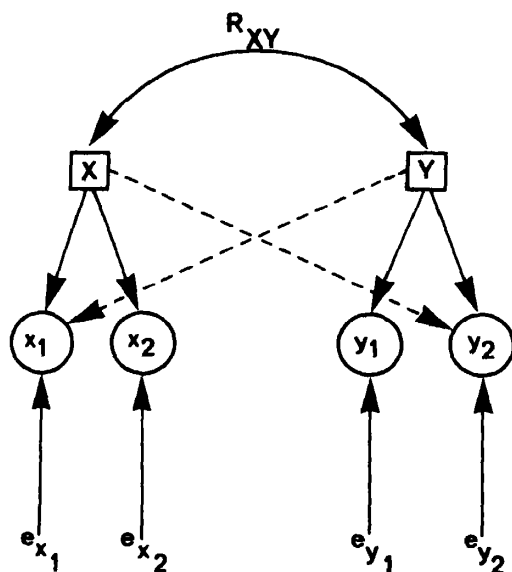
FIG. 1.—Simple measurement model for the correlation between father's and son's jobs, where x_1 and x_2 are fallible reports of father's job status when the son was 14 years old in the first and second perspectives, respectively, and y_1 and y_2 fallible reports of the son's current job. X and Y are the true statuses, and R_{XY} the true father-son correlation.

The two reports of father's and son's job reflect the true statuses and differential measurement error. Using the rules of path analysis we can estimate the father-son correlation in per 1 as $r_{x(1)y(1)} = p_{x(1)X} \cdot R_{XY} \cdot p_{y(1)Y}$ and estimate the same correlation in per 2 as $r_{x(2)y(2)} = p_{x(2)X} \cdot R_{XY} \cdot p_{y(2)Y}$. These two equations have two "knowns" (the observed father-son correlations) and five unknowns (all the terms on the right-hand side of the equations). We can estimate reliabilities only by making restrictive assumptions. Setting the "true" correlation to .550 and further assuming reliabilities of self-reported jobs held by the respondent (y_1 and x_2) of .85 yields implied validity coefficients $p_{x(2)X}$ and $p_{y(1)Y}$ of .922 (the square roots of the reliabilities).

Solving the two equations gives reliabilities of .327 for father's job in per 1 and .984 for son's job in per 2. Thus for two proxy reports we estimate one very low reliability and one reliability absurdly high—higher than that assumed for self-reports.

Because equally implausible and mutually inconsistent estimates result from similar analyses of all correlations in table 2, measurement error in these data must be nonrandom. It is possible, for example, that the respondent's own status affects his reports on his kin. Such a possibility is represented by the dashed lines in figure 1. This model is so underidentified that without direct evidence on the reliability of reports and comparative reports from the same father-son pairs there is no way to choose parameters from the wide range of alternatives.

CONCLUSION

The findings presented in this paper and the observations of others (see Blau and Duncan 1967, p. 321; Ridge 1973, p. 94; and Mason et al. 1976) suggest that proxy, and perhaps self-, reports of socioeconomic status may be biased. In our case respondents report (perhaps unconsciously) their socioeconomic careers and the careers of their kin in such ways as either to maximize differences between themselves and their fathers or to minimize differences between themselves and their sons. The ideology of mobility and achievement in open-class societies may create its own illusion as well as its reality. Some similar process might also explain Blau and Duncan's anomalous finding that the educational level of the respondent's oldest brother was more closely linked to social background variables than that of the respondent himself.

We cannot say how far the present findings can be generalized, but they do not endorse the proposition that proxy reports are as accurate as self-reports. They encourage as much attention to design as to analysis. Remeasurement studies need to be extended to obtain two-way information

on the stratification process. But two independent concurrent measurements are better than any remeasurement. Only studies deliberately designed to get information from both members of father-son pairs will definitively deal with the problem of proxy versus self-reports Until such direct tests are made one cannot confidently claim that what they say about daddy is true.

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Homophily, Selection, and Socialization in Adolescent Friendships[1]

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Longitudinal sociometric data on adolescent friendship pairs, friends-to-be, and former friends are examined to assess levels of homophily on four attributes (frequency of current marijuana use, level of educational aspirations, political orientation, and participation in minor delinquency) at various stages of friendship formation and dissolution. In addition, estimates are developed of the extent to which observed homophily in friendship dyads results from a process of selection (assortative pairing), in which similarity precedes association and the extent to which it results from a process of socialization in which association leads to similarity. The implications of the results for interpreting estimates of peer influence derived from cross-sectional data are discussed.

Friendship is frequently used as an explanatory variable in a large body of sociological literature concerned with interpersonal and contextual influences where a person's behavior or attitude is attributed to the influence of his peers. Individuals are consistently most likely to hold a particular attitude or engage in a particular behavior when their close associates also hold that attitude or engage in that behavior. For example, persons who report that their three best friends are Democrats are more likely to vote Democratic in a presidential election than persons with Republican friends (Berelson, Lazarsfeld, and McPhee 1954). With social class held constant adolescents whose friends aspire to college are more likely to aspire to college themselves (Duncan, Featherman, and Duncan 1972). Similarly those whose friends use illegal drugs are also more likely to use drugs (Kandel 1973). Lazarsfeld and Merton (1954) proposed the term "homophily" to refer to the tendency for similarity on various attributes among persons who affiliate with each other.

A fundamental but most often implicit assumption underlying these analyses is that actual or perceived similarity among friends results from a process of influence in which one or more persons ("alter") influence

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another ("ego"). This is a somewhat circular explanation, for a focal respondent (ego) is someone else's friend (alter) and a potential source of influence. Furthermore, the implied causality is based upon cross-sectional data. But observations of similarity among individuals at one point in time could result from either or both of two processes: selection and socialization. Homophily among friends could result from a selection process leading to assortative pairing, such that individuals with prior similarity on some attributes of mutual importance purposefully select each other as friends. Alternatively, homophily could result from a socialization process in which individuals who associate with each other, irrespective of their prior similarity, influence one another. The two processes are not mutually exclusive, and both could play a role at different points of the process of friendship formation, maintenance, and dissolution. Only longitudinal data on real-life friendships can help determine which process predominates.

Not only have few longitudinal studies of real-life friendships been carried out (e.g., Newcomb 1961; Curry and Kenny 1974), but to date there exists no systematic assessment of the relative importance of selection and socialization as determinants of observed similarity among friends. The various theories of interpersonal behavior (Homans 1961; Thibaut and Kelly 1959; Heider 1958; Newcomb 1961) agree that similarity in attitudes induces liking (interaction, attraction) and correlatively that liking engenders similarity. There is much empirical evidence to support the statement that strength of liking is related to degree of similarity on a variety of attributes such as attitudes, values, and personality traits (see Berscheid and Walster 1969; Lazarsfeld and Merton 1954; Newcomb 1961). However, since most studies are based on data obtained in a laboratory and/or at one point in time and not over time, the evidence is correlational. The causal sequence linking homophily and interpersonal attraction has not been established.

This paper represents an attempt to address this issue and to assess the role of socialization and selection in interpersonal attraction. I examine homophily on a variety of behaviors and attitudes among high school students for whom a unique set of longitudinal data make it possible to investigate the role of similarity in the development, maintenance, and dissolution of adolescent friendships. For several hundred friendship pairs, data are available on what adolescents are like not only when they choose each other as friends, but also on prospective friends before they choose each other and on former friends after they cease being friends. Descriptive information is available on the degree of actual similarity between members of friendship pairs at various stages of friendship formation and dissolution. Most important, attempts are made in this analysis to express in quantifi-

able terms the relative importance of the processes of selection and of socialization in adolescent friendships.[2]

METHODS

As part of a longitudinal survey of adolescent illicit drug use carried out among New York State high school students (Kandel, Single, and Kessler 1976), a panel sample of adolescent best-schoolfriend dyads ($N = 957$) was obtained with adolescents matched to themselves and to an identifiable friend in both surveys. Structured, self-administered questionnaires were given in classrooms to the entire student body in five schools at the beginning and at the end of the academic year 1971–72. At each time period, each student was asked to identify his/her best friend in school (for further details about the study, see Kandel 1973; Kandel, Single, and Kessler 1976). Of the 957 friendship pairs, 668 were stable friendships with the same members over time. In 289 pairs, the adolescent named a different best friend at each survey. In a number of cases, information was available in the time 1 sample ($N = 266$) for the friend-to-be (i.e., newly chosen at time 2) and information at time 2 ($N = 233$) about the former friend (chosen at time 1 but not time 2). A total of 217 adolescents could be matched both to a friend-to-be at time 1 and to a former friend at time 2. At time 1, 40% of the dyads comprised reciprocated choices. The proportion of different friends at time 2 was twice as high among unreciprocated as among reciprocated pairs (39% versus 19%). Identical data were available on each member of the dyads.

In this paper, I consider two behaviors and two attitudes: self-reported frequency of marijuana use in the past 30 days, involvement in minor delinquent activities, level of educational aspirations, and political identification.[3] Homophily was measured by Kendall's τ_b, a symmetric measure of association for ordinal data, and by the ϕ coefficient.

[2] After the present article was submitted and accepted for publication, the related article by Cohen (1977) appeared. Using a different approach, Cohen addressed issues closely related to those discussed in the present study and reached similar conclusions. Homogeneity on 18 dimensions in 49 school cliques was analyzed over the course of one school year, from data in one of the 10 schools in Coleman's *The Adolescent Society* (1961). Analyses were carried out on the over-time characteristics of stable, new, and former members of surviving and new cliques. Cohen concluded that similarity prior to clique association, what he calls "homophilic selection," was the most important contributor to clique homogeneity. The effects of conformity pressures were small. Leaving of the group by deviates was unimportant. In a rare case of independent convergence in sociological research, Cohen also reached conclusions similar to those reached in this study about the broader implications of the results.

[3] Frequency of current marijuana use determined from a question asking whether marijuana had been used in the past 30 days "never," "1–2 times," "3–9 times," "10–39 times," "40–59 times," or "60 times or over." Involvement in minor delinquency is

RESULTS

The analyses of the data proceed in two parts. First, the relationships between homophily and friendship at various stages of friendship formation and dissolution are described. Second, estimates of the relative strength of selection and socialization in friendships are discussed.

Homophily and the Formation and Dissolution of Friendships

Three types of dyads are compared at two points in time. At time 1, there are the (1) friendship pairs that will remain stable over time, (2) those that will dissolve by time 2, and (3) dyads that will become friends at time 2. At time 2, there are the (1) friendship pairs who remained stable over time, (2) pairs newly formed at time 2, and (3) pairs of former friends. Measurements of similarity on behaviors and attitudes among these six types of dyads, as well as among reciprocated and unreciprocated stable pairs, appear in table 1.

Several important conclusions can be reached:

1. At time 1, prior to any subsequent change, pairs that will remain stable over time are much more similar in their behaviors and values than the subsequently unstable pairs (compare rows 4 and 1).

2. At time 1, pairs that will break up over time are less similar in their behaviors and attitudes than pairs in the process of formation, with the one exception of political orientation (compare rows 1 and 2).

3. Homophily among stable pairs increases over time (compare rows 4 and 5).

4. Homophily among newly formed pairs is greater after the pairs have formed (T2) than before (T1), except for political orientation (compare rows 3 and 2).

5. At time 2, homophily among former friends is lower than among new friendship pairs or stable pairs (compare rows 6 and 5).

6. Homophily is higher among stable pairs with reciprocated choices than among pairs with unreciprocated choices (compare rows 7 and 8).

These patterns are clearest with respect to self-reported current (past 30 days) marijuana use and may be related to the role of this behavior in adolescent development and friendship formation. On no other characteristic is homophily between adolescent friends as great as on the use of illegal drugs (Kandel 1978). Drug use by one's peers is one of the strongest

measured by a seven-item index. Educational aspirations distinguish six levels of achievement ranging from not finishing high school to going to graduate or professional school. Political orientation is measured by a six-category classification ranging from "very conservative" to "radical."

TABLE 1

HOMOPHILY* ON BEHAVIORS AND ATTITUDES AMONG FRIENDSHIP DYADS AT VARIOUS STAGES OF FORMATION AND DISSOLUTION

Type of Friendship Dyad†		Time When Homophily Assessed‡	Frequency Current Marijuana	Educational Aspirations	Political Orientation	Minor Delinquency
1. Unstable: changed friend at time 2	A_1F_1	Time 1	.239	.327	.145	.158
2. Friends-to-be at time 2	A_1N_1	Time 1	327	.348	.107	.248
3. New friends	A_2F_2	Time 2	405	.406	.106	.296
4. Stable pairs	A_1F_1	Time 1	451	.349	.201	.255
5. Stable pairs	A_2F_2	Time 2	.505	.382	.248	.286
6. Former friends (were friends at T1)	A_2O_2	Time 2	220	380	.185	.167
7. Stable pairs: reciprocated	A_1F_1	Time 1	.580	.369	.263	.307
8. Stable pairs: unreciprocated	A_1F_1	Time 1	.302	.327	.127	.203

* As measured by Kendall's τ_b.

† A = adolescent chooser; F = friend chosen; N = friend-to-be; O = former friend.

‡ Time 1 = fall; time 2 = spring.

predictors of adolescent initiation into various forms of drug use (Kandel, Kessler, and Margulies 1978).

Estimates of Selection and Socialization in Friendships

The findings reported in table 1 indicate that processes of selection and socialization are both involved in friendships. To obtain more precise estimates of each process, I examined the joint distributions over time of the behaviors and attitudes of each member in stable and unstable friendship pairs. Among the latter, I examined separately the joint behavior over time of unstable friendship pairs from the time they were friends to the time they ceased being friends and, correlatively, the behavior of pairs before they were friends to the time they became friends. Each variable was transformed to a dichotomy to give three 16-fold tables for each of the four variables of interest. The data for current marijuana use are displayed in table 2. (Data for the other three variables are available from the author on request.)

TABLE 2

JOINT PATTERNS OF CURRENT MARIJUANA USE OVER TIME BETWEEN STABLE FRIENDS, FORMER FRIENDS, AND FRIENDS-TO-BE

Current Use at Time 1		Current Marijuana Use at Time 2				
		Ego Yes		Ego No		
Ego	Alter	Alter Yes	Alter No	Alter Yes	Alter No	Total N
Stable pairs with same friend at time 1 and time 2:						
Yes	Yes	27	5	6	5	43
Yes	No	7	16	1	9	33
No	Yes	6	1	20	18	45
No	No	19	14	15	436	484
Total N.		59	36	42	468	605
Pairs with different friend at time 2: old friend at time 2.						
Yes	Yes	3	3	4	1	11
Yes	No	1	6	2	6	15
No	Yes	6	1	9	7	23
No	No	3	7	10	109	129
Total N.		13	17	25	123	178
Pairs with different friend at time 2: friend-to-be at time 1:						
Yes	Yes	6	. .	2	2	10
Yes	No	3	4	1	8	16
No	Yes	4	2	7	7	20
No	No	3	8	9	112	132
Total N..		16	14	19	129	178

To estimate the processes of selection and socialization, I assessed whether increased homophily of friends on a particular variable is determined more by changes in friendships or by changes in the behavior (or attitude) among those who are already friends. Two methods are used, a gross and a net method.[4]

To estimate selection with the gross method, determinations are made of how many cases, for each category of the joint initial distribution of variables in the friendship pairs, are lost through the breaking of friendships and of how many cases are gained through new friendships. This gives a hypothetical distribution that would exist if friendship changes (i.e., selection) were the only factor affecting the distribution of joint patterns in the friendships. To estimate socialization, another hypothetical distribution is estimated which assumes that drug use changes (i.e., socialization) among stable and unstable friendships constitute the only factor responsible for changes in the distribution. The calculations for marijuana use appear in table 3. Similarity on the behavior within friendship pairs in each hypothetical distribution of joint patterns, as measured by a ϕ coefficient, is the statistical measure of each process. Comparison of the two coefficients indicates which process produces the most similarity. In the gross method, the friendship changes include those pairs that changed also in drug use, and the drug use changes include those pairs that changed also in friendships. A second set of calculations was computed according to the net method, in which changes which could be due to either process were excluded.

Estimates of the two processes according to each method for the four attributes of interest are presented in table 4. Three trends are apparent. Results are similar whether computed according to the gross or the net method. All estimates are statistically significant. Finally, both selection and socialization appear to be approximately of equal importance. None of the differences between the socialization and selection estimates in each pair of ϕ coefficients is statistically significant, as computed by the method suggested by Bishop, Fienberg, and Holland (1976, p. 381). Because the basic assumption of nonindependence of the samples is not met in the present analysis, the results are conservative. There is a slight suggestion that socialization is more important than selection for marijuana use and political orientation.

CONCLUSION

The results support the general conclusion that adolescents coordinate their choices of friends and their behaviors, in particular the use of marijuana, so as to maximize congruency within the friendship pair. If there is a state

[4] James S. Coleman devised the methods described in this section.

TABLE 3

HYPOTHETICAL DISTRIBUTIONS ON CURRENT MARIJUANA USE IN FRIENDSHIP PAIRS AT TIME 2 IF DUE TO PROCESS OF SELECTION OR PROCESS OF SOCIALIZATION

Pattern of Use in Friendship Pairs		Actual Initial Distribution T1	Changes in Friendship in Selection			Changes in Marijuana Use in Socialization		
Ego	Alter	(N)	Old Friends Lost (N)	New Friends Gained (N)	New Hypothetical Distribution T2 (N)	Changed Away from Pattern (N)	Changed toward Pattern (N)	New Hypothetical Distribution T2 (N)
Uses	Uses	54	11	16	59	24	42	72
Uses	No use	48	15	14	47	26	31	53
No use	Uses	68	23	19	64	39	38	67
No use	No use	613	129	129	613	68	46	591
Total N	. .	783	178	178	783	157	157	783

NOTE.—Based on data in table 2.

TABLE 4

ESTIMATES OF SELECTION AND SOCIALIZATION UNDERLYING HOMOPHILY ON BEHAVIORS AND VALUES IN ADOLESCENT FRIENDSHIP PAIRS*

Method	Frequency of Current Marijuana Use	Level of Educational Aspirations	Political Orientation	Participation in Delinquency
Gross method:				
Selection	.434	.385	235	.263
Socialization	.454	335	.248	.186
Ratio· selection/socialization	.96	1 15	.95	1.41
Net method:				
Selection	424	.383	253	.232
Socialization	460	355	.273	.231
Ratio: selection/socialization	.92	1 08	.93	1.00
	($N = 783$)	($N = 867$)	($N = 766$)	($N = 870$)

NOTE.—As measured by ϕ-coefficients As described in text, measure similarity of hypothetical distributions in dyads Gross method includes all changes Net method excludes pairs that changed simultaneously on characteristic and in friend. Estimates are based on data in tables 2 and 3 and unpublished data.

* All ϕ-coefficients are statistically significant at $P < .01$. None of the differences between estimates of selection and of socialization for each of the dimensions are statistically significant.

of unbalance such that the friend's attitude or behavior is incongruent with the adolescent's, the adolescent will either break off the friendship and seek another friend or will keep the friend and modify his own drug behavior.

These longitudinal sociometric data provide insights about processes involved in the formation and dissolution of friendships and about processes of peer influence which are not apparent from consideration of cross-sectional data. Prior homophily on a variety of behaviors and attitudes is a determinant of interpersonal attraction as documented by the finding that friendships that will dissolve are less similar than friendships in the process of formation. Homophily increases further as the result of sustained association. Thus, homophily between friends at one point in time results from two complementary processes whereby adolescents who share certain prior attributes in common tend to associate with each other and tend to influence each other as the result of continued association. While the importance of these processes varies for different areas of adolescents' lives, the respective importance of selection and socialization within each area is approximately the same.

These estimates of selection and socialization are particularly useful to assess to what extent the effects attributed to peers in cross-sectional models of interpersonal influence actually reflect interpersonal influence versus assortative pairing. This is especially important for educational attainment, where the most sophisticated causal models have been developed to date. Such models as that of Duncan et al. (1972) attribute the similarity in aspirations between friends, after controlling for the background characteristics of ego and alter, to the mutual influence of friends on each other.

The present analyses establish that such models overestimate by about 100% the influence of friends by failing to take into account the role of prior similarity in friendship formation.

The finding that the socialization effects of peers are strongest for marijuana use, together with what we already know about the epidemiology of illicit drug use (namely, that its onset is in adolescence), suggests that drug use may be a particularly strategic behavior around which to study basic processes of peer influence and of friendship formation and dissolution in adolescence.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

NONPARAMETRIC PATH ANALYSIS: COMMENTS ON KIM'S "MULTIVARIATE ANALYSIS OF ORDINAL VARIABLES"[1]

In "Multivariate Analysis of Ordinal Variables" (*AJS* 81 [September 1975]: 261–98), Kim examines the ordinal and parametric strategies for the analysis of ordinal variables. He prefers the latter, in part because he found that: (1) the ordinal partial path coefficients cannot detect a known spurious relationship or a causal chain among underlying interval variables, (2) the squared multiple τ (T^2) is less than unity when there is a completely determined underlying linear relationship, and (3) the ordinal partial path coefficients are insensitive to the constant c (for $c = 5, 10$) in the equation $t_y = t_x + ct_z$ used to generate simulated data.

This comment aims to clarify the use of τ_b and ρ_b in nonparametric path analyses of ordinal data. Its generic theme is that ordinal strategy statistics can be useful in the process of successive approximation and refinement of normal social science research. I point out that some of Kim's conclusions are based on erroneously calculated values of Kendall's τ_b and suggest that some of his findings that he interprets as unfavorable can be interpreted as favorable to the ordinal strategy. Four aspects of Kim's analysis are discussed:

[1] The research reported here was supported in part by grants from the Academic Senate, University of California, Santa Barbara, and the Exxon Education Foundation. I am indebted to Robert H. Somers and Milton Sobel for helpful comments on an earlier draft and to Andrew Lasser and John Bonno for research assistance. I wish to thank Erlinda Parks, of Abt Associates Inc., for skillful typing.

(1) the relationship between ρ_b and $\bar{r}$, (2) causal interpretation and ordinal and rank partials, (3) complete linear determination and multiple association, and (4) relative causal importance and stability of ordinal and rank partials. No parametric strategy statistics in Kim's sense are calculated or discussed.

The results support Ploch's (1974, p. 366) contention that ordinal (and rank) statistics are useful in the process of successive approximation and refinement—the purported inappropriateness of the ordinal strategy for this purpose led Kim to reject its use. Development of interval or quasi-interval measures is likely to be difficult and arduous. The ordinal and rank statistics can be used as screening devices in ordered contingency tables of exploratory research. If these statistics indicate relationships between certain sets of ordinal variables but not others, methodologists can then try to improve the level of measurement of the potent variables so that parametric statistics can be used to further specify causal laws. It is probably advantageous to utilize the τ_b-based statistics in multivariate analyses when there are several intervening and dependent variables, and the effects of the categories of the independent variables have different effect-proportional consequences on the various dependent variables. If each independent variable has equal-interval effects on each dependent variable, the Spearman statistics are preferred because they utilize information about the magnitudes of the differences in rank, as well as the sign of the differences.[2]

Relationship between ρ_b and $\bar{r}$

Contrary to Kim's statement (p. 288), applying parametric statistics to data in which ranks are treated as metric values does not necessarily produce Pearsonian correlation coefficients (designated $\bar{r}$) that are equivalent to Spearman's ρ_b's. Kim's reported values of ρ_b (p. 288) are, strictly speaking, $\bar{r}$ correlations that do not fulfill the structural conditions for $\bar{r} = \rho_b$. Following Reynolds (1971, pp. 106–7), $\bar{r}$ is defined as a Pearsonian correlation coefficient calculated over equal-interval scales assigned to the categories of variables. Smith (in preparation) has found, with the help of James Robertson, that for each of two polytomous variables $\bar{r}$ calculated over the assigned equal-interval scales equals ρ_b, when for each variable all odd-numbered categories have the same number of cases and all even-numbered categories

[2] This recommendation reflects what might be called an "extrinsic" approach to measurement as developed by Coleman (1964) and Boyle (1970). This approach judges the level of measurement of a variable in part on the basis of its effects. Applying this guideline to rankings, one would require that the equidistant rank numbers have equal-interval effects on dependent variables. Contrariwise, what might be called the "intrinsic" approach to measurement views the level of measurement of a variable primarily in terms of the intrinsic characteristics of the operational device. If the variable is a ranking, then the rank numbers are just that, rank numbers, and there is no need to require that they have equal-interval effects. From this point of view the Spearman statistics can be used whenever the variables are rankings, regardless of the pattern of effects.

have the same number of cases. More formally,

$$\hat{r} = \rho_b \quad \text{if (and only if)}$$
$$n_{i.} = n_{(i+2).} \quad \text{for } i = 1, 2, \ldots, r-2$$

and

$$n_{j} = n_{.(j+2)} \text{ for } j = 1, 2, \ldots, c-2$$

for r rows and c columns. This result implies that $\hat{r} = \rho_b$ when for each variable the combined number of cases falling in any two adjacent categories is the same.

Kim's data for m_2, m_3, and m_4, reported in his table 5 (p. 287), do not conform to this structural condition. Consequently, Kim's r' correlations (p. 288) are not equivalent to Spearman's ρ_b's.

Causal Interpretation and Ordinal and Rank Partials

Kim's finding (pp. 274–78) that the ordinal partial path coefficient can not detect an underlying spurious relationship or a causal chain involving three known interval variables is based on four erroneously calculated values of τ_b; see table 1.

Figure 1 reports corrected calculations of Kim's erroneous analysis of a spurious relationship presented in his figure 8*a* (p. 277) and, for purposes of comparison, a parallel analysis based on ρ_b. Here the dashed curved lines represent residual correlation not accounted for by the specified causal relations. The correct value of τ_{23} in (*a*) equals .105. Given this causal model of a spurious relationship, the product $\tau_{12}\ \tau_{13} = .363 \times .289 = .105$ is the predicted value of τ_{23}. Contrary to Kim, there is no unaccounted-for covariation between X_2 and X_3; the difference between the actual and predicted values is zero (.105 − .105 = .000). The ordinal partial path coefficients detect perfectly this spurious relationship. With X_3 as the ultimate dependent variable, $P_{31\cdot 2} = .289$ and $P_{32\cdot 1} = .000$.

The path coefficients based on ρ_b produce similar results. The ρ_b correlation between X_2 and X_3 is .134. Given that $\rho_{21} = .425$ and $\rho_{31} = .316$, the predicted value of ρ_{23} is their product, .134, which equals the observed correlation. Again, there is no unaccounted-for covariation. The values of the

TABLE 1

CORRECTED MATRIX OF τ_b'S

	X_1	X_2	X_3	X_4
X_1.		.363 (363)	.289 (321)	204 (.253)
X_2. . .			.105 (.210)	.461 (.411)

NOTE.—Kim's values are enclosed in parentheses.

partial path coefficients with X_3 as the ultimate dependent variable corroborate this result: $P_{32.1} = .000$; $P_{31.2} = .316$.

The correctly calculated results for Kim's causal chain model are not as perfect—see figure 2—but they are more favorable to the ordinal strategy than are Kim's erroneous results. The reanalysis results in a smaller value of residual correlation, .037, compared with Kim's .104. The new value is obtained by first calculating a predicted value of τ_{14} from the product $\tau_{12}\tau_{24}$; this equals .167. This number is then subtracted from the actual value of τ_{14}, which is .204. The difference of .037 represents a direct path from X_1

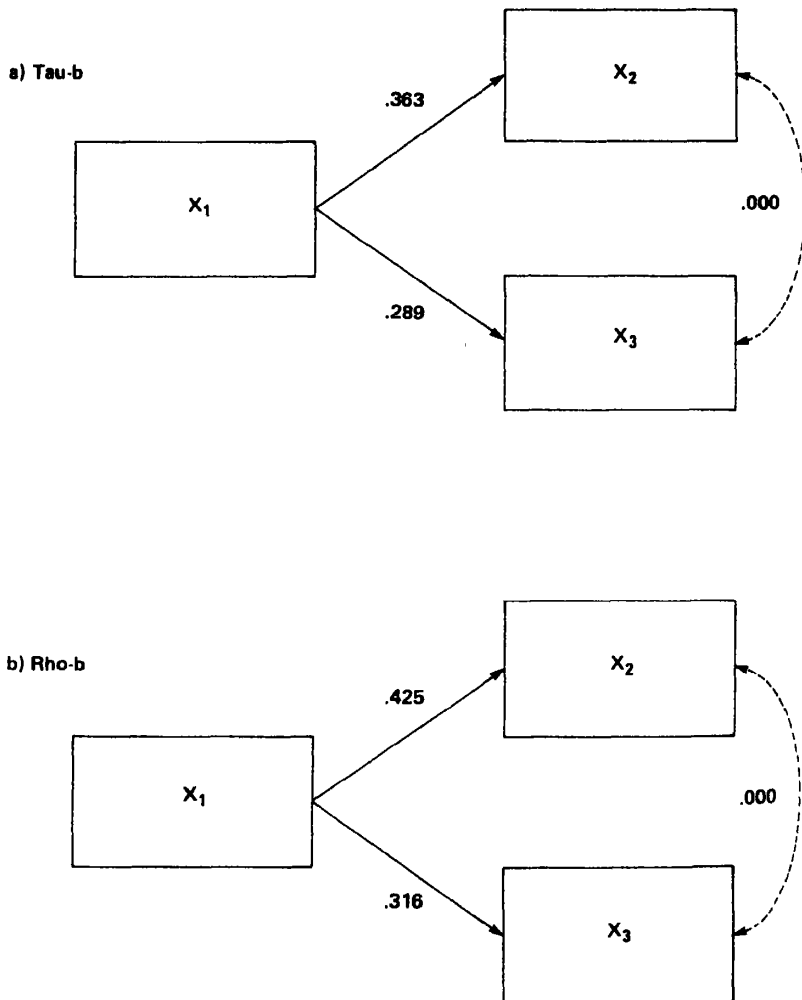



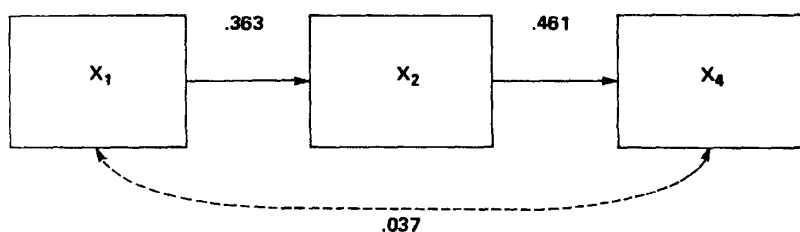
Fig. 1.—Replication of Kim's fig. 8*a*

to X_4 that does not exist given the underlying causal chain structure. The ordinal partial path coefficients calculated from correctly calculated bivariate τ_b's are within rounding error of the deduced values: $P_{42 \cdot 1} = .46$ and $P_{41 \cdot 2} = .04$.

The residual unaccounted-for covariation using ρ_b is only .01. This value is obtained by first calculating a predicted value of ρ_{14} from the product $\rho_{12}\rho_{24}$; this equals $.425 \times .572 = .243$. The observed value of $\rho_{14} = .251$. The difference, .008, rounds to .01. These results are corroborated by the values of the partial path coefficients calculated from the zero-order ρ_b's: $P_{41 \cdot 2} = .01$ and $P_{42 \cdot 1} = .57$.

These results are paradoxical because, statistically, a spurious relationship and a causal chain are identical. In both, a bivariate association disappears when a test variable is controlled. The difference is substantive. In the former the test variable is antecedent to X and Y; in the latter, it intervenes.

A tentative resolution of this paradox can be inferred from inspection of

a) Tau-b

b) Rho-b

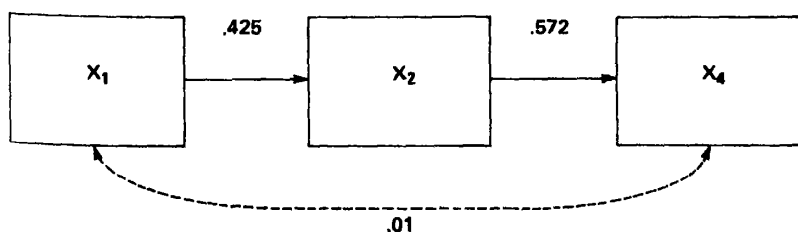



Fig. 2.—Replication of Kim's fig. 8*b*

TABLE 2

CONDITIONAL COVARIANCES AND VARIANCES FOR KIM'S SPURIOUSNESS EXAMPLE

	Category of X_1		
	$X_1=1$	$X_1=2$	$X_1=3$
Ordinal:			
Conditional ordinal covariances	0	0	0
X_2 conditional ordinal variances	$\frac{27}{2\cdot 81}$	$\frac{27}{2\cdot 81}$	$\frac{27}{2\cdot 81}$
X_3 conditional ordinal variances	$\frac{27}{2\cdot 81}$	$\frac{27}{2\cdot 81}$	$\frac{27}{2\cdot 81}$
Rank:			
Conditional rank covariances	0	0	0
X_2 conditional rank variances	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$
X_3 conditional rank variances	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$
Interval:			
Conditional interval covariances	0	0	0
X_2 conditional interval variances	$\frac{27\cdot 16}{2\cdot 81}$	$\frac{27\cdot 16}{2\cdot 81}$	$\frac{27\cdot 16}{2\cdot 81}$
X_3 conditional interval variances	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$	$\frac{27\cdot 36}{2\cdot 81}$

summaries of the conditional distributions. Table 2 reports the ordinal, rank, and interval variances of X_2 and X_3, given variable X_1, for Kim's spuriousness example. The ordinal and rank conditional variances can be looked at in two ways: (1) within each conditional subtable the variances of the independent and dependent variables are equal, and (2) the conditional variances of the independent variable across conditional subtables are equal, and likewise for the dependent variable. One or the other, or perhaps both, of these structural conditions may be necessary in order for the ordinal and rank path coefficients to detect perfectly an underlying spurious relationship or causal chain.

The Wright path coefficients calculated for the fully interval observed data also detect perfectly the underlying spurious relationship. There is perfect detection even though only the second of the two preconditions is fulfilled—within each conditional subtable the conditional variances of X_2 and X_3 are not equal.

For Kim's causal chain example, table 3 presents the summarizing covariances and variances. In every condition of X_2, regardless of level of measurement, the X_1 conditional variance does not equal the X_4 conditional variance, that is, all the conditional variances are heteroscedastic. Moreover, the variances of X_2 across conditions are not equal, and likewise for X_4. Under these conditions only the Wright path coefficients calculated over the observed interval data perfectly detect the underlying causal chain.

TABLE 3

CONDITIONAL COVARIANCES AND VARIANCES FOR KIM'S CAUSAL CHAIN EXAMPLE

	Category of X_2						
	$X_2=3$	$X_2=4$	$X_2=5$	$X_2=6$	$X_2=7$	$X_2=8$	$X_2=9$
Ordinal:							
Conditional covariances	0	0	0	0	0	0	0
X_1 conditional ordinal variances.	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$	$\frac{9}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{9}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$
X_4 conditional ordinal variances.	$\frac{3}{2\cdot 9}$	$\frac{3}{2\cdot 9}$	$\frac{12}{2\cdot 36}$	$\frac{3}{2\cdot 9}$	$\frac{12}{2\cdot 36}$	$\frac{3}{2\cdot 9}$	$\frac{3}{2\cdot 9}$
Rank:							
Conditional covariances	0	0	0	0	0	0	0
X_1 conditional rank variances.	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$	$\frac{9\cdot 6}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{9\cdot 6}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$
X_4 conditional rank variances.	$\frac{3\cdot 4}{2\cdot 9}$	$\frac{3\cdot 4}{2\cdot 9}$	$\frac{12\cdot 16}{2\cdot 36}$	$\frac{3\cdot 4}{2\cdot 9}$	$\frac{12\cdot 16}{2\cdot 36}$	$\frac{3\cdot 4}{2\cdot 9}$	$\frac{3}{2\cdot 9}$
Interval:							
Conditional covariances	0	0	0	0	0	0	0
X_1 conditional interval variances.	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$	$\frac{9\cdot 8}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{9\cdot 8}{2\cdot 36}$	$\frac{0}{2\cdot 9}$	$\frac{0}{2\cdot 9}$
X_4 conditional interval variances.	$\frac{3\cdot 4\cdot 9}{2\cdot 9}$	$\frac{3\cdot 4\cdot 9}{2\cdot 9}$	$\frac{12\cdot 36}{2\cdot 36}$	$\frac{3\cdot 4\cdot 9}{2\cdot 9}$	$\frac{12\cdot 36}{2\cdot 36}$	$\frac{3\cdot 4\cdot 9}{2\cdot 9}$	$\frac{3\cdot 4\cdot 9}{2\cdot 9}$

Thus, Kim's first conclusion needs modification. The ordinal strategy (and rank) path coefficients can detect perfectly an underlying spurious relationship or causal chain if certain structural preconditions are fulfilled. Further research is needed to pinpoint these conditions mathematically.

Complete Linear Determination and Multiple Association

Multiple τ.—Kim (p. 273) further implies that T^2 and R^2 are not equivalent in meaning. In one sense he is right: as he correctly illustrates, T^2 is less than unity (.774) in a distribution where, by the usual meaning of R^2, one might expect it to equal unity. In another sense, however, Kim is incorrect. Smith (1977*a*) shows that the square of bivariate Daniel's Γ is a proportional reduction in error (PRE) statistic. Since the squared generalized multiple correlation coefficient, designated M^2, is the zero-order Γ^2 correlation between actual (y_{ij}) and predicted ($\hat{y}_{ij}$) pairwise differences on the dependent variable, using $\hat{y}_{ij} = D_{yx\cdot z}X_{ij} + D_{yz\cdot x}Z_{ij}$, the generalized regression equation for pairwise differences (Smith 1974, pp. 209–10), to predict y_{ij}, it follows that M^2 is a PRE statistic (Smith 1977*b*). The PRE form of M^2 is:

$$M^2_{\text{PRE}} = \Sigma y^2_{ij} - \Sigma(y_{ij} - \hat{y}_{ij})^2/\Sigma y^2_{ij}\,. \quad (1)$$

This general expression provides PRE interpretations for various special cases of M^2, including R^2, R_s^2 (the square of multiple Spearman's ρ), T^2, and for dichotomous systems, Φ^2 (the square of multiple ϕ), etc.[3] In this sense it is correct to say that T^2 is equivalent in meaning to the R^2 in ordinary regression.

Kim's (p. 273) request for clarification of the meaning of perfect ordinal multiple association with respect to the underlying form of the relationship can be answered in terms of equation (1) above. Just as R^2 can be calculated from equation (1) as a bivariate Γ^2 between predicted and actual interval pairwise differences, T^2 can be calculated from (1) as a bivariate τ_b^2 between predicted and actual ordinal scores. The T^2 will equal unity when this τ_b^2 equals unity. Now, as Kim (p. 271) notes, one very attractive feature of bivariate τ_b is that its magnitude reaches unity if Y is a strictly monotonic function of X, that is, if the value of Y increases as X increases, and conversely. This implies that for each value of X there is a unique value of Y, and vice versa (Somers 1962, p. 801). Thus $T^2 = 1$ when there is a strictly monotonic relationship between predicted ($\hat{y}_{ij}$) and actual (y_{ij}) ordinal pairscores, and $T^2 < 1$ when this relationship is nonmonotonic.

This insight clarifies Kim's example. Recall that he created an interval variable $t_y = t_x + ct_y$ in which the score on t_y is a linear function of, and completely determined by, the scores of variable t_x and $c = 1 \times t_y$. Then he showed that $T^2 = .774$ when $R^2 = 1$. The first column of table 4 reports these statistics and those for R_s^2 as part of a parameter study discussed below. There are two properties of the ordinal scoring system that in this example contribute to the lack of monotonicity implied by $T^2 < 1$: (1) the matrix of ordinal scores for y_{ij} is not equal to the element by element sum of the scores in x_{ij} and z_{ij}—although $t_y = t_x + t_z$, score $y_{ij} \neq$ score x_{ij} + score z_{ij}; and (2) in constructing under ordinal scoring rules the y_{ij} matrix as the sum of x_{ij}'s and z_{ij}'s, $x_{ij} = 0 + z_{ij} = 1$ is scored as $a + 1$, as is $x_{ij} = 1 + z_{ij} = 1$.

The relationship will become strictly monotonic if these two rules are modified so that: (1) score y_{ij} = score x_{ij} + score z_{ij}, and (2) the sum of

[3] The square of the generalized multiple correlation coefficient, M^2, can be expressed as $M^2 = \Sigma^n_{j=2} \pi^2_{1j} + 2\Sigma^n_{j=2} \Sigma^n_{k=j+1} \pi_{1j}\pi_{1k}\Gamma_{jk}$. In this expression variable X_1 is dependent, the π^2_{1j}'s are generalized path coefficients (Smith 1974), and the Γ_{jk}'s are Daniel's generalized correlations. The M^2 has two components, direct and correlational determination (Land 1969, pp. 18–19; Smith 1974, pp. 226–27). The direct determination is quantified by the sum of squares of the generalized path coefficients. Each quantifies the proportion of var (X_1) which an independent variable explains by direct causal paths. The second summation quantifies correlational determination, the component of M^2 that is due to correlation among the independent variables. If the independent variables are uncorrelated, then this term will usually be nonzero, and M^2 will be the sum of the direct and correlational determination. It is pertinent to analyze both components of M^2 in any path analysis, because if the correlational determination is large relative to the direct determination, M^2 and π^2_{1u} will be poor measures of explained and unexplained causal effect. This caveat also applies to any statistical analysis that relies on the squared multiple correlation coefficient as the measure of explained variance.

TABLE 4

PARAMETER STUDY OF KIM'S TABLE 3

	t_x+t_z	t_x+2t_z	t_x+3t_z	t_x+4t_z	t_x+5t_z	t_x+10t_z	t_x+20t_z	t_x+30t_z	t_x+40t_z	t_x+50t_z
Ordinal:										
$P_{yx \cdot z}$	.622	.363	.289	.289	.289	.289	.289	.289	.289	.289
$P_{yz \cdot x}$	.622	.825	.866	.866	.866	.866	.866	.866	.866	.866
T^2	.774	.813	.834	.834	.834	.834	834	.834	.834	.834
Rank:										
$P_{yx \cdot z}$	.703	.425	.316	.316	.316	.316	.316	.316	.316	.316
$P_{yz \cdot x}$	.703	904	.949	.949	.949	.949	.949	.949	.949	.949
R^2_s	.988	997	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Interval:										
$P_{yx \cdot z}$	.707	.447	.316	.243	.196	.0995	.050	.0333	.0249	.0199
$P_{yz \cdot x}$	.707	.894	.949	.970	.981	.995	.999	.9994	.9996	.9998
R^2	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

$a + 1$ with $a + 1$ is scored as $a + 2$, rather than $+1$. These changes convert the ordinal scores into interval pairwise differences, causing the partial path coefficients to have the same value as those for the interval statistics, $P_{yx \cdot z} = P_{yz \cdot x} = .707$ and $M^2 = R^2 = 1$.

The above explications of T^2 indicate that: (1) T^2 and R^2 are equivalent in meaning in the sense that they are both special cases of M^2, and both are covered by the same general PRE interpretation; (2) $T^2 = 1$ when the form of the relationship between y_{ij} and $\hat{y}_{ij}$ is strictly monotonic, and $T^2 < 1$ when there is a lack of monotonicity; and (3) a $T^2 = 1$ may occur only in "ridiculously simple situations" (Hawkes 1971, p. 918) because the ordinal scoring rules themselves contribute to the lack of monotonicity. Although the concept of perfect ordinal multiple correlation is now more clearly defined than in early writings, the structural conditions that affect the behavior of T^2 are in need of further mathematical explication.

Multiple Spearman's ρ.—Inspection of table 4 indicates that R_s^2 does not quite reach unity: for $t_y = t_x + t_z$ and $t_y = t_x + 2t_z$, the coefficients are .988 and .997, respectively. But R_s^2 does equal one for $c \geq 3$, when the data are full ranked on t_y. This fact suggests that the midrank method used to correct for ties may cause some error in prediction that prevents R_s^2 from reaching one. As mentioned earlier, the midrank method assigns to tied observations the average of the ranks they would have had if there were no ties. For example, the interval scores for t_y when $t_y = t_x + t_z$ are 1, 2, 2, 3, 3, 3, 4, 4, 5. When these interval numbers are treated as ranks and the midrank method is utilized, they become 1, 2.5, 2.5, 5, 5, 5, 7.5, 7.5, 9. Similarly, the interval numbers for t_x and t_z are 1, 1, 1, 2, 2, 2, 3, 3, 3. When these numbers are treated as ranks and the midrank method is used, these numbers become 2, 2, 2, 5, 5, 5, 8, 8, 8. Note that the interval numbers for t_x, t_y, and t_z are equal interval: the distance is 1 between 2 and 1, 3 and 2, 4 and 3, and 5 and 4. Similarly, when X and Z are treated as ranks, there is an equal distance between the midrank scores: $5 - 2 = 3$ and $8 - 5 = 3$. But there is not an equal distance between the rank numbers for Y: $2.5 - 1 = 1.5$, $5 - 2.5 = 2.5$, $7.5 - 5 = 2.5$, and $9 - 7.5 - 1.5$. When the Pearson product-moment system is applied to the data generated by $t_y = t_x + ct_z$, there is no error in prediction using the regression equation $\hat{y} = b_{yx \cdot z}X + b_{yz \cdot x}Z$, and $R^2 = 1.000$. The Spearman ρ system is essentially the Pearson system applied to these data but treated as rankings with ties corrected by the midrank method. When the rank on Y is predicted by the regression equation $\hat{y} = b_{yx \cdot z}X + b_{yz \cdot x}Z$, there is a slight error in prediction and $R_s^2 = .988$, as it should because the rank on y is not exactly equal to the sum of the rank on X and the rank on Z because of the correction for ties.

When $t_y = t_x + 3t_z$ the data become fully ranked on Y, in other words, there is an equal number of cases in each category of Y, namely one. Also, the distances between the midrank scores on X are equal, as are those on Y

These conditions are consistent with the structural requirements for $\bar{r} = \rho$. Here r calculated over the true scores equals $\bar{r} = \rho$ calculated over the rankings. As table 4 reports, the interval and rank statistics converge and $R^2 = R_s^2 = 1.000$.

Relative Causal Importance and Stability of Ordinal and Rank Partials

Kim (pp. 273-74) notes that the ordinal path coefficients do not always gauge correctly the relative causal importance of the underlying interval variables. He based this conclusion on an analysis of $t_y = t_x + ct_z$ for values of $c = 1$, 2, 5, and 10. Table 4 reports his results, as well as those for $c = 3, 4, 20, 30,$ 40, and 50. Kim noted that for $c = 5$ and $c = 10$ the values of the ordinal partials are identical and not in the same ratio as the underlying metric variables. Kim did not explain the reasons for this stability, but the tone of his report implies that he considers it undesirable.

Table 4 enables us to assess the reasons for this behavior. First, inspecting the Wright path coefficients calculated for the observed interval data note that the value of $P_{yz \cdot x}$ always equals $c \cdot P_{yx \cdot z}$. There is a perfect correspondence between the ratio of $P_{yx \cdot z}$ to $P_{yz \cdot x}$ and that of t_x to ct_z. Except for $c = 1$ and 3, the ordinal and rank path coefficients do not have this ratio. When $c = 3$ the data become fully ranked on Y and the ordinal and rank path coefficients are in the same ratio as 1 to c. Once the observations are fully ranked on Y, the ordinal and rank statistics become nonparametric with respect to c and stabilize, while the Wright path coefficients continually change to reflect changes in c.

From Kim's point of view this stability of the ordinal and rank partials is not a desirable property. But this behavior is exactly what one desires from a "sturdy" or nonparametric statistic. One uses these statistics when the underlying distributions are unknown and one wants to summarize the manifest information with a minimum of assumptions about the underlying patterns.

Implications

In effect, multivariate statistics based on τ_b or ρ_b may be conceptualized as belonging to the ordinal strategy. Both are useful for summarizing information in ordered contingency tables resulting from cross-tabulations of rankings with many ties. These statistics are particularly appropriate for exploratory analyses of data describing statistical populations. Statistics in the τ_b family may be called weakly ordinal, because they are based only on signs of differences in rank between elements in pairwise comparisons. That is, if X is a ranking, then the assigned (weakly) ordinal pairscore is $x_{ij} =$ sign $(X_i - X_j) = \pm 1, 0$. Statistics in the ρ_b family, on the other hand, are

strongly ordinal, being based on the signed differences in rank between elements in pairwise comparisons. That is, if X is a ranking, then the assigned (strongly) ordinal pairscore is $x_{ij} = X_i - X_j$. This score reflects both differences in sign and in rank. None of these statistics are parametric in Kim's sense.

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MULTIVARIATE ANALYSIS OF ORDINAL VARIABLES REVISITED[1]

Several points raised by Smith can be misleading and, consequently, require some comments. But before presenting these substantive comments, I would like to acknowledge the errors in some of the τ_b values in figure 8 of my original article (1975), as indicated by Smith. I owe a sincere apology to the research community as a whole and a note of gratitude to Smith for identifying these errors.

Ironically, however, this incident gives me the opportunity to reaffirm my basic position, perhaps more clearly and strongly. As will be demonstrated,

[1] I am grateful to James Duane for his editorial and typing assistance.

none of the basic arguments in my article are undermined by the presence of these specific errors or by the counterarguments of Smith. On the contrary, the latter seem to reinforce my original conclusion—that the ordinal strategy is no better than the parametric strategy at meeting some of the basic requirements of multivariate analysis.

I will now try to respond to Smith's comments individually in much the same order as he has presented them, but with some slight reorganization.

Spearman's ρ and Ordered Categories

It is well known that Spearman's ρ was originally developed for full rankings without ties, and a modified formula exists for rankings with a few ties. Spearman himself never generalized the coefficient to ordered categories or rankings with numerous ties. In fact, no standard textbook seems to generalize ρ for ordered categories. Perhaps it was too cavalier on my part to have claimed without explanation that Spearman's ρ applied to ordered categories is equivalent to Pearson's r applied to such data after the ordered categories are changed into equal-interval scores. In my opinion, my generalization of ρ is more in line with the spirit of the original formulation of ρ than Smith's; one way or the other, however, the issue is not very important.

It is very informative, however, to examine the rationale behind Smith's generalization of ρ for ordered polytomies, because it clearly reveals the measurement assumptions implicit in the original formulation of ρ and the fact that the use of ρ imposes metric scores on ordinal data in a similar manner as in the use of parametric strategy.

Consider that there are two full rankings. The magnitude of Spearman's ρ applied to such rankings is equivalent to the magnitude of Pearson's r applied to the same data after the rankings are converted into metric values by arbitrarily assuming that the distances between all adjacent pairs of ranks are equivalent. When there are a few ties in the rankings, the original Spearman's formulation is to assign midranking to the tied ranks. But this is also equivalent to assigning the mean values for the tied cases on the assumption that (1) all the cases are fully rankable, and (2) the distances between all pairs of adjacent cases are equal. If one generalizes these two measurement assumptions to the ordered categories, one arrives at Smith's generalization of ρ.

Such a generalization to ordered polytomy is illustrated below. Consider that a variable contains three ordered categories and is distributed as follows: agree, 85 cases; neutral, 15 cases; and disagree, 5 cases. The midrank method or the mean-value method would assign the following values (or any linear functions of them) to each category: agree, 43; neutral, 93; disagree, 103. The assumed distance between the first two categories is 50

and the assumed distance between the last two is 10. Smith's generalization of Spearman's ρ involving this variable would be the same as the Pearson's r applied to the same variable after it is "scored" or "quantified" as shown above. The measurement assumptions implied by the use of Spearman's ρ are as questionable as the assumptions of the so-called parametric strategy.

Therefore, the discussion of the relationship between ρ and r is at best irrelevant and at worst misleading. Or, I should say that, ironically, this discussion helps clarify this issue (1) by providing a clear illustration of the nature of the implied measurement assumptions, and (2) by reminding us to look into the underlying and sometimes implicit assumptions of a method instead of accepting any formulation purely on the grounds provided by the author or the inventor of the method.

What Are the Conditions under Which Ordinal Partials Can Detect Underlying Causal Structure?

One of the criteria I employed in evaluating the ordinal strategy was to examine whether the use of the strategy can correctly identify some of the simplest underlying causal structures—a spurious causal relationship and a simple causal chain. My conclusion was that it does not lead to correct identification of such an underlying causal structure. My conclusion still stands, but it requires a new and better demonstration than the examples shown in figure 8 of my original article. Those examples not only contain erroneously calculated τ values but also have some undesirable measurement features.

As Smith correctly points out, the use of either τ or ρ for the multivariate data shown in figure 8*a* would have identified the underlying spurious relationship correctly. But neither method would identify correctly the underlying simple causal chain shown in figure 8*b*, although Smith is inclined to consider the errors in identification to be minor or negligible. The example of the spurious relationship was a strategically bad one, because the data are contrived in such a way that every pair of adjacent rankings has equal distance in the underlying model. Because of this peculiarity, all the measurement assumptions implicitly imposed on the data by the use of ρ are met by the data. (For the same reason, the use of r, or parametric strategy, would also have correctly identified the underlying causal structure.) The reason the ordinal strategy cannot identify the underlying simple chain shown in figure 8*b* is precisely that the data do not fit the measurement assumptions imposed by the use of ρ; some of the adjacent rankings do not have the same distance.

Does the use of partials lead to a correct identification of the underlying structures when these measurement assumptions are not met? Or is it possible to specify some ordinal conditions under which the ordinal partials

lead to correct identifications? Although appropriately tentative, Smith suggests that ordinal strategy can detect perfectly an underlying simple causal structure "if certain structural preconditions are fulfilled." By comparing the structural conditions of the data illustrating the spurious causal relationship (fig. 8*a*) and those for the simple causal chain (fig. 8*b*), Smith identifies tentatively these structural preconditions as follows: (1) within each category of the control variable, the *ordinal* variances of the independent and dependent variables are equal, and/or (2) the conditional *ordinal* variances of the independent variable across the categories of the control variable are equal. The fact that the required structural conditions have nothing to do with ordinal variances can be demonstrated easily. Let us consider two different examples.

First, consider a situation in which there are no ties in any variable. (I did not present simulations based on such data in my 1975 article, not because I did not try them, but because I wanted to make the case with the simplest possible example. Unfortunately, the simplest example I selected was marred by the errors in the calculation of the τ_b values and was not an appropriate one even without such errors.) Then the conditions Smith has specified become irrelevant, because there is no longer conditional variance to discuss. Or, does this situation lead to correct identification of the underlying causal structure by the ordinal strategy? I have simulated the simplest spurious causal relationship and the simplest causal chain, as I did in my 1975 article, but this time, instead of using source variables with three integer values, I have used three variables that are "continuous" and orthogonal. The simulated values are given in table 1, and the causal analyses using the model and various strategies are presented in figure 1.

None of the strategies can correctly identify the underlying causal structure. Some might think that the errors are minor; remember, however, that we are dealing with situations in which sampling and measurement errors are absent. All of these strategies will lead to the correct identification only when all of the variables meet the equal-interval assumptions.

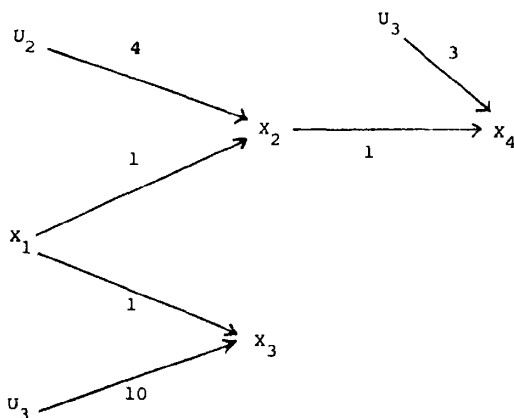
For a second example, consider a situation of ordered categories in which the underlying data do not meet the assumptions of equal interval (between adjacent cases or rankings) but do meet all the structural conditions specified by Smith. An example of a spurious causal relationship meeting all of these conditions is presented in table 2. Note that, within each category of X_1, both X_2 and X_3 have equal ordinal variance and that, across the categories of X_1, both X_2 and X_3 have equal ordinal variances.

But applying ordinal strategy to these data (X_1, X_2, X_3) does not lead to a correct identification of the underlying model. The results are shown in figure 2. Some might consider the discrepancy minor; remember, however, that we are dealing with data without sampling error.

The important message is that the structural preconditions suggested by

TABLE 1

ORTHOGONAL DATA SET USED IN SIMULATING CAUSAL RELATIONSHIPS

Cases	U_1	U_2	U_3
1	1.51017	.31629	1.61861
2	81169	2.46931	− .17871
3	.48073	1 10860	.21756
4	−.95903	−.22883	.02820
5	−1 98510	46637	−1.09310
6	32453	−.34779	−.11435
7	−.30740	−1.11030	.16704
8	2.38757	−1.96640	−.66055
9	−.71892	−.83686	−2 66410
10	−.26934	− 40114	.87571
11	.65720	81650	− 68332
12	1.03308	.08955	−.86942
13	.23588	93641	−1 16640
14	−1 36590	−1 50540	1 13601
15	−.06627	− 26861	1 00107
16	−.40755	− 06544	23990
17	−.44014	.12002	− 52019
18	−.28146	−.71181	.91719
19	.51975	−.20829	44801
20	−1 16020	1 32786	1 25584

a) Standardized path coefficients and Pearson's r:

$r_{23} = .02412 = b_{21}b_{31} = .24258(.09946)$

$r_{14} = .19612 = b_{21}b_{42} = .24258(.80861)$

b) Ordinal strategy (using Kendall's τ):

$r_{23} = -.08421 \neq -.00798 = b_{21}b_{31} = .25263(-.03158)$

$r_{14} = .33684 \neq .15158 = b_{21}b_{42} = .25263(.60000)$

c) Parametric strategy (using ρ or $\bar{r}$):

$r_{23} = -.09323 \neq -.01077 = b_{21}b_{31} = .31128(-.03459)$

$r_{14} = .38797 \neq .24386 = b_{21}b_{42} = .31128(.78346)$

(X_1, U_2, and U_3 are presented in table 1)

FIG. 1.—The true model

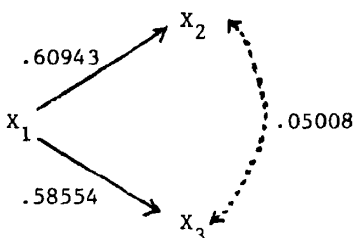
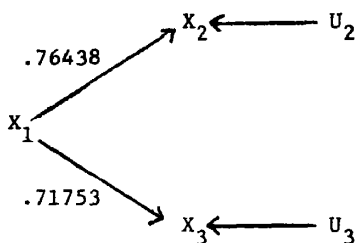
TABLE 2

A SPURIOUS CAUSAL RELATIONSHIP: $X_1 = U_1$; $X_2 = U_1 + U_2$; $X_3 = U_1 + U_3$

Cases	U_1	U_2	U_3	X_1	X_2	X_3
1	1	1	1	1	2	2
2	1	1	4	1	2	5
3	1	1	9	1	2	10
4	1	5	1	1	6	2
5	1	5	4	1	6	5
6	1	5	9	1	6	10
7	1	8	1	1	9	2
8	1	8	4	1	9	5
9	1	8	9	1	9	10
10	3	1	1	3	4	4
11	3	1	4	3	4	7
12	3	1	9	3	4	12
13	3	5	1	3	8	4
14	3	5	4	3	8	7
15	3	5	9	3	8	12
16	3	8	1	3	11	4
17	3	8	4	3	11	7
18	3	8	9	3	11	12
19	9	1	1	9	10	10
20	9	1	4	9	10	13
21	9	1	9	9	10	18
22	9	5	1	9	14	10
23	9	5	4	9	14	13
24	9	5	9	9	14	18
25	9	8	1	9	17	10
26	9	8	4	9	17	13
27	9	8	9	9	17	18

Smith are not structural conditions, but another implicit imposition of measurement assumptions. So far I have not been able to contrive a situation in which ordinal and rank partials lead to correct identification unless the following measurement assumptions are met: (1) The distribution of the underlying variable is such that the distances between all pairs of adjacent *categories* are equal—in the case of τ_b. (2) The distribution of the underlying variables is such that the distances between all pairs of adjacent *cases* are equal—in the case of ρ. That is, they can correctly identify the underlying causal structure only when the assumption of equal interval of one sort or another is met. Consequently, these alternative strategies cannot be claimed to be better on the grounds that they are not based on unwarranted measurement assumptions.

Relative Magnitude of Partials and Multiple r in Ordinal Strategy

Smith concedes that ordinal strategy does not lead to a correct identification of relative causal effect and that the definition of perfect association in ordinal strategy does not subsume under it a perfect linear association as a

The expected correlation between X_2 and X_3 = .76438 (.71753) = .54846 = the observed correlation = .54846.

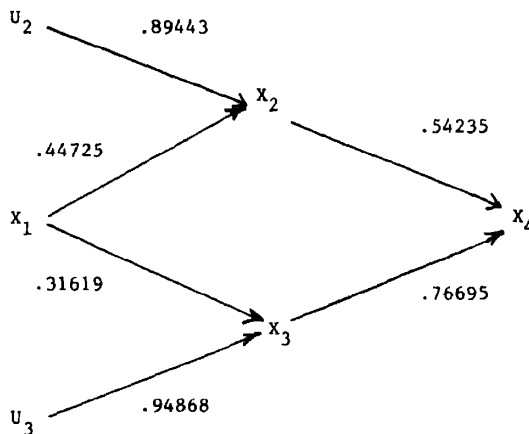
The expected correlation = .60943 (.58554) = .35685 $\neq$ the observed correlation = .40693.

FIG. 2.—Top, the true model. Bottom, Kendall's τ

special case. He is free to consider this lack of correspondence as a virtue rather than a defect of ordinal strategy. It is certainly defensible to argue that the ordinal strategy may establish only a weak order between any two effects, but it does not make sense to have an ordinal measure of perfect association that does not include a perfect linear determination as a special case.

Given the possible introduction of these errors (which some might consider negligible), the logical question is whether they remain negligible—or even minor—when ordinal strategy is applied to more complex situations. I did not consider this sort of examination to be necessary when I wrote my article. Since that view was based in part on what I presumed to be a clear demonstration of the inadequacy of ordinal strategy in the simplest possible situation, it now seems worthwhile to examine a more complex situation.

Consider an example which is relatively simple but more complex than a simple causal chain—the case of the double causal chain shown in figure

3. To keep the situation still simple enough, we will use the same source variables shown in table 2. Use of ordinal strategy will in general identify none of the following facts: (1) all the relationships between X_1 and X_4 are mediated by X_2 and X_3; (2) there is no association between X_2 and X_3 other than through X_1; and (3) X_4 is completely determined by X_2 and X_3 (setting aside the issue of relative importance of X_2 and X_3 for X_4). Nor will the use of the so-called parametric strategy lead to correct identification. That is why I did not endorse the philosophy that applying correlation on ordinal data is acceptable in multivariate analysis. Still, the use of parametric strategy can be considered better on the grounds that, *when* the measurement assumptions are met by the data, or closely approximated, such an analysis can reveal all three conclusions indicated above, plus the relative magnitude of the effects of X_2 and X_3 on X_4, whereas under the same circumstances the ordinal strategy can specify correctly only conclusions 1 and 2.

a) Various correlation coefficients:
$R^2_{4\ 23} = 1$
$r_{23} = r_{12}r_{13} = .14141$
$r_{14} = b_{21}b_{42} + b_{31}b_{43} = .48506$

b) Ordinal strategy (using Kendall's τ):
$R^2_{4.23} = .50543$
$r_{23} = .01053 \neq r_{12}r_{13} = .05053$
$r_{14} = .43158 \neq b_{21}b_{42} + b_{31}b_{43} = .26320$

c) Parametric strategy (Using Spearman's ρ or $\bar{r}$):
$R^2_{4.23} = .93125$
$r_{23} = .01654 \neq r_{12}r_{13} = .09286$
$r_{14} = .55038 \neq b_{21}b_{42} + b_{31}b_{43} = .44763$
(X_1, U_2, and U_3 are presented in table 1)

FIG. 3. The true model with standardized coefficients

Concluding Remarks

I stand firmly behind what I said in my original paper: that the so-called ordinal strategy is no better than the parametric strategy at meeting problems created by the presence of variables whose metric bases are not clearly established. The parametric strategy handles these problems by treating the distances between the adjacent categories of an ordinal variable as equivalent, while the ordinal strategy handles the situation by treating any differences between ordered categories as equivalent as long as the differences are in the same direction. Neither strategy can correctly identify in general the underlying causal structure and in particular a deterministic relationship—the fact that a dependent variable is a perfectly linear (and therefore also a strictly monotonic) function of the independent variables. In practice, however, we are forced to use one or the other, despite awareness of their inadequacy. No one would advocate not using them for such a philistine reason; we improve our knowledge through successive approximations and refinements.

I did make the claim, however, that parametric strategy, when used in the right perspective, is preferable to the ordinal strategy. The reason for this claim is as follows: in using parametric strategy, one is clearly aware of the arbitrary nature of the equal-interval assumptions, and the justification for using this strategy is based not upon the claim that such measurement assumptions are correct but on the fact that the error introduced into certain statistics (for example, correlation coefficients) by errors in measurement may not be substantial. No one claims, however, that parametric strategy does not introduce errors. Such an error can be especially serious in multivariate analysis. But these types of errors can be eliminated or reduced by an improvement of the measurement properties of the variables under examination.

In contrast, the advocates of the ordinal strategy (I used to be one) claim that it does not violate measurement assumptions and is therefore compatible with the ordinal data. The major part of my earlier paper was devoted to the demonstration that there is an inherent contradiction in using this strategy in multivariate analysis. The logical difficulty lies in trying to add a certain ordinal difference in one variable to one in another variable. Consequently, if one accepts the proposition that the ordinal strategy violates some of the measurement assumptions of ordinal scale, there is no particular reason to assume that the ordinal strategy is superior to the parametric strategy. On the other hand, if one believes that the multivariate ordinal strategy does not violate the measurement assumptions, such a stand is not conducive to refining and improving the measurement properties of one's variables.

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Retrospective Review Essay: Neighborhood Life and Urban Environment

Death and Life of Great American Cities. By Jane Jacobs. New York: Random House, Vintage Books, 1961. Pp. 458. $2.45 (paper).

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Still in print 17 years after publication, Jane Jacobs's *Death and Life of Great American Cities* is cited in every current urban sociology text and appears consistently in the columns of the *Social Science Citation Index*. The author deals with many contemporary themes in urban social life, particularly the nature of neighborhood in the city and the social effects of the physical urban environment—buildings, sidewalks, streets, and parks. This reconsideration reviews the context of *Cities*, its central ideas, and its reception. The essay discusses the relationship of the work to urban sociology at the time of its writing and in the current situation.

Jacobs's book emerges from the big-city milieu of the 1950s: its politics, social life, and architecture. The effects of the Housing Acts of 1947 and 1954 had become apparent, as had those of the Interstate Highways Act of 1956. Large-scale slum clearance had taken place, and working-class housing had also been razed in some places. Numerous federally financed low-income, multistoried apartment buildings had been erected. Urban renewal land was being used for upper-middle-class housing and for such institutional purposes as hospitals, universities, and cultural centers. Expressways of unprecedented dimensions were being constructed through the cities. Suburbs were growing rapidly, with residential environments of single-family detached houses, shopping centers, and other facilities tied to the technology of the automobile.

Jacobs was highly aware of all these physical developments, but there were also major social developments underway which eluded her glance. The mass migration of blacks from the rural South to the cities went unnoted. The baby boom was in progress but not mentioned in the book. On the other hand, Jacobs anticipated one social development which was about to happen: the urban explosion of crime and fear. One British reviewer wrote that, when he saw chapter headings about safety, he expected a discussion of environmental accidents, and he was surprised to read about the prevention of violent crimes. It made him wonder about American society.

Leading ideas of urban planners, architects, and reformers provide the intellectual context of the work. Ebenezer Howard's English "garden city" ideas imported by Lewis Mumford, Clarence Stein, Elizabeth Bauer Wurster, and others, with their advocacy of greenbelts, low densities, and

separate zones for different activities, drew her ire. She also attacked the concept of the tower in the city surrounded by parklike greenswards and cut through by expressways, as proposed in LeCorbusier's "radiant city." Her enemies list is a who's who of 20th-century city planners, housing reformers, and urban architects. She rejected advocacy of fundamental housing reform, slum clearance, and new housing projects for the poor. She rejected the idea of large-scale city planning, the "city beautiful" style. "Make no large plans" might be her watchword.

The book was widely reviewed, and it evoked two types of responses. Some reviewers dismissed the author as incompetent and the argument as trivial. More than one sexist reviewer put down the author as a simple localistic housewife, albeit an angry one, despite the fact that she had been an associate editor of *Architectural Forum*, a leading journal, for several years. Others recognized the scope and depth of her analysis and her challenge to planning orthodoxies; they too acknowledged the fundamental problems of public housing and urban renewal and echoed her call for a fresh approach to urban policy. Many reviewers appreciated the hard-hitting, pithy writing and the vivid descriptions of city life in the volume.

While the author dealt with the leading planning ideas of the time, she made no use of urban sociology. The index includes only a few sociologists. By the 1950s, the main corpus of the Chicago School was complete, and there were numerous works available on neighborhoods. Although the decade was not a particularly lively one in urban sociology, there was some important new work coming out. But Jacobs's lengthy book, which reflects several years of work, takes no cognizance of sociology. It is as though Wirth, McKenzie, Firey, and the others had said nothing useful.

Despite the author's slighting of the discipline, the work is sociologically compelling. Her method is observation, and she has a keen eye for detail. She advocates looking closely at ordinary scenes and events. She is also a participant, as a parent and resident in the neighborhood and a leader in disputes between the locality and city officials. Clearly she is not a neutral observer, but she does present a good deal of fine-grained description of urban life.

Her methodology has been criticized by those who say the book is a case study of an atypical neighborhood, Greenwich Village. Although the work is grounded there, her analysis refers to a type of community, the "great American city," and she consistently brings in detailed material from several cities as well as from other parts of New York City. Great cities, while undefined, are large and dense: Boston, Philadelphia, and Baltimore belong in the set. Los Angeles does not, in Jacobs's estimation. She notes that towns, little cities, and suburbs constitute kinds of communities different from great cities and require different explanations and solutions (p. 16).

A central theme of her work is that the physical environment of the city has fundamental, direct effects on social life, especially at the neighborhood level. For example, wide sidewalks permit neighborly visiting and children's

play in a dense residential area. Ill-designed parks—ones that are too large, for example—foster illicit antisocial activities such as assaults, gang fighting, drinking, and vandalism. She does not say that the environment causes the activities but that it permits them.

A second theme ties the environment to the spatial and temporal mix of social activities. Attacking a basic idea of city planning, she advocates mixed land uses. Separation of uses yields specialized activity areas in time and space—residential areas, institutional areas, shopping areas, and others. She says that such segregation leads to emptiness at given points in the diurnal cycle: residential areas depopulated by day, shopping centers empty at night. This creates a potential for crime during the empty hours or "dullness" in general. Her argument mixes environmental concern with the pattern of activities. If activities are mixed, the environment works better. A park near a shopping center would be less lively than one near offices and a school, in addition to shops. Such a complex would generate crowds and activities at different times of the day. Her appreciation of the timing of urban social life is reminiscent of Suttles's (1968) time analysis of the groups using a neighborhood park and Breese's (1964) examination of the flow of workers in a metropolis.

Jacobs stresses the positive effects of population heterogeneity at the neighborhood level. Her model neighborhood would mix people by age, ethnicity, and socioeconomic level. It would have children as well as old people, WASPs as well as Italian Americans, tailors as well as physicians. They would watch out for each other while keeping a proper urban distance.

The author recognizes an undesigned social order in the life of the city. She doubts the abilities of social or environmental engineers to produce a community superior to the one which emerges without a plan. She contends that, even though it is difficult to perceive, the city has an underlying order. Our ignorance and limited observations prevent an appreciation of the intricacies of the city as a working mechanism.

This assumption of unplanned order supports her proposals in public policy and environmental design. Her overall statement to municipal and federal governments and to city planners is do less; do smaller things; let individuals do things; preserve, do not destroy; favor pedestrians and residents over cars and trucks; do not build residential areas apart from shops, factories, and entertainment facilities.

One central element in her argument is vague and elusive: the concept of the Great Blight of Dullness. Typically, according to Jacobs, a section of a city declines because it is dull. The Bronx is dull, Queens is dull. Los Angeles is very dull. Dullness is the opposite of vitality. A place is dull if it is uniform, if there is no mix of land uses. Any place that is totally residential is dull. This concept is terribly difficult to operationalize; Jacobs gives very few clues which would help a researcher to classify actual residential areas as dull or not dull. Nonetheless, the idea of dullness is at the center of her argument.

In recent sociology, the most useful part of *Cities* has been the discussion

of density and crowding. Jacobs advocates high densities as a feature of a healthy residential area. This is recommended in the context of streets with wide sidewalks and shops and a nontransient population. The combination would promote satisfying contact and adult supervision of children's outdoor activities. It would suppress crime because strangers and deviant acts would be observable by the shopkeepers and denizens of the sidewalks.

Crowding within households, on the other hand, is harmful and to be avoided. Crowding and high density are independent. "Overcrowding within dwellings or rooms, in our country, is almost always a symptom of poverty or of being discriminated against. . . . Everyone hates overcrowding and those who must endure it hate it worst. Almost nobody overcrowds by choice. But people often do live in high-density neighborhoods by choice" (p. 208).

Several studies in the intervening years confirm her position on density and crowding. Ecological studies of cities fail to find a correlation between high density and pathologies which is not eliminated by multivariate analysis incorporating socioeconomic status and race and ethnicity. Ecological studies and some surveys do show negative effects of crowding. (There are some serious and unresolved differences among the crowding studies at this point.)

The main sociological points in the book deal with the neighborhood in contemporary cities and with the relationship of the built environment and social life.

Jacobs states that there are two functionally important levels of neighborhood within the city as a whole: "street neighborhoods" and districts. Day-to-day household life takes place in street neighborhoods which may be as small as one block. Child rearing and visiting are two local activities. The larger neighborhood, the district, would house about 100,000 people. Such districts do not exist effectively in contemporary cities, but she recommends them for political purposes. They would facilitate the political articulation of the residents, giving them a level of government more accessible than City Hall.

Urban sociology has long struggled with the concept of neighborhood and generates a different model. Residential areas of the metropolis are divided by socioeconomic level, race, and ethnicity. While the division between areas is not perfect, there are clearly upper-middle-class areas, just as there are poverty areas. There are distinctively Jewish areas, Polish areas, and certainly black ones. To some extent there is also separation by age, sex, and household type; some areas are likely to house families with children while others have more young singles, older persons, etc. This segregation may not be severe, but it is statistically discernible. Despite the advocacy of heterogeneity by urbanists such as Jacobs, this is not consistent with the observed preferences and behaviors of people within urban housing markets. Furthermore, for over 50 years, urban whites have resisted residential integration with blacks.

Jacobs rejects the idea of urban "turf," saying that territorial boundaries

tend to arise when poor design produces housing projects for a single type of resident. This assertion flies in the face of U.S. urban history, in which different nationality groups always had boundaries which were well known at least to the local adolescents, who learned where they could and could not go safely. Suttles shows the continuing existence of such ethnic boundaries in his Chicago slum study (1968), and he suggests that the "defended neighborhood" is a common form of urban organization (1972). A local population may coalesce to defend itself against some nearby alien group which they are resisting in housing or school integration or against some city agency which is about to act in the area.

Neighborhood research shows that different categories of residents have differential involvement in the local area, that the area performs different functions for different people. In general, instrumental uses such as shopping, banking, professional services, etc., have been on the decline in the neighborhood ever since the widespread diffusion of the automobile and the postwar building of facilities adapted to it. The only categories of people who continue to use local facilities are the aged and the very poor. Entertainment and churchgoing are no longer necessarily local activities. The main neighborhood activity seems to be child rearing and elementary education. Small children and mothers, along with old people, are the most likely to carry on localized social activities.

Neighborhood organization has not been well researched, and more is known about membership and participation than about the organizations themselves. Length of residence is closely related to involvement; people who have lived longer in the area are more likely to belong to organizations and to be integrated into the social life of the area by means of friendships and acquaintanceships. Parents of school-aged children are much more likely than others to belong to some local organization. Evidently community organizations come and go depending upon the issues of the day. Some are devoted to defense purposes—preventing clearance or keeping out blacks. Others deal with external forces such as banks (for "red-lining") or City Hall or the board of education.

The difficult part of current thinking about neighborhoods lies in reconciling it with the network model of urban social relations. This model takes note of the fact that contemporary technology, especially the telephone and automobile, frees sociability from proximity. Kinship, friendship, professional and other social networks are dispersed over the area of the metropolis and still maintain their integrity. Thus the task for the understanding of the social life of the residential area is to discover the extent to which some activities are still localized for some categories of people.

A final element of the current sociological conception of the local area emanates from the work of Janowitz (1952) and Hunter (1974). It states that the neighborhood is to some extent a symbolic creation which enables some metropolitan residents to identify with some local community within the larger agglomeration.

Jacobs's blind spot was the suburbs and the new type of low-density

development in the growing urban areas of California. She did not conceal her dislike of such communities, but she was also unable to understand their attraction to millions of families. Her explanation was that the builders and planners had created dullness in the central cities which was driving people away. If they had created vitality instead, the city would have continued to attract and hold people. Unfortunately, she did not reckon with the spatial environmental preferences of Americans.

Numerous surveys have shown that most Americans prefer single-family detached houses for family life and child rearing. The suburban environment is chosen for this stage in the family life cycle. When Jacobs wrote, the nation had experienced the tremendous family formation after the world war and was more than 10 years into the baby boom. Suburban developments were providing housing which filled the deficit created by the lack of home building during the depression and the 1940s. As Gans (1967) first showed in his study of middle- and working-class families leaving Philadelphia for Levittown, residents were leaving the city in pursuit of an environment in which they could raise children in the style to which the parents aspired. Recent research by Michelson (1977) has examined the situation of urban families who reside in apartment buildings. He finds that most are there out of necessity and view their tenancy as temporary, since they aspire to move to single-family detached houses.

The new theory of proxemics, which appeared after the publication of *Cities*, provides one interpretation of the situation in Greenwich Village. Different cultures contain different proxemic formulas. Italians have a high-contact culture, whereas Americans prefer a good deal of space. In this aspect of her work Jacobs might have been misled by what she saw in the Italian-American population.

Research on the interplay between social life and the built environment has expanded in recent years. A few sociologists, including H. Gans, R. Gutman, S. Keller, W. Michelson, and J. Zeisel, have contributed to the new literature, but they are greatly outnumbered by psychologists. Architects, to the extent that they admit human factors in their paradigm, have adopted individual psychology, particularly perception.

Two central concepts in the sociological approach to environment are choice and congruence. The sociologists reject any form of architectural or environmental determinism. People, except for the very poor, choose residential environments in which they can express life-styles which emerge from their socioeconomic situation and their stage in the family life cycle. The environment does not determine behavior, but different physical situations may facilitate particular behavioral patterns and social arrangements. To the extent that the environment facilitates the chosen patterns of the users, environment and activity are congruent. The sociologists see the environment as a less powerful influence than Jacobs did.

In some cases the environment may be a central element in a social process. The most dramatic case is Newman's (1972) analysis of differential crime rates in New York City housing projects. He argues that certain

designated features of the apartment buildings facilitate criminal behavio: The individual buildings are sited in open areas off streets; they includ double stairwells with many outlets; they have hidden nooks such as mai box areas; and these features plus others allow the criminal to act and t escape unobserved. Alternative designs are shown to suppress crime t some extent. In this analysis, Newman is perfectly in line with Jacobs argument, but most cases are not so neat.

The production of the built environment is a social process. Cranz (1971 shows that ideology affects the scope and location of city parks. Zeis (1975) explains that architects designing civic projects do not encounter th ultimate occupants but deal with bureaucrats and civic officials. Th designers, then, are likely to be more or less ignorant of the cultural patterr and behavioral norms of the users of a project, particularly if these are poc families in housing projects. Zeisel advocates the introduction of soci science into the design process to discover the spatial patterns of use groups and to bring this information to the designers. This approach consistent with the direction implied by Jacobs when she decried the distanc between planners in large agencies and local-level residents.

What is the contemporary sociological value of *Death and Life of Gre American Cities*? Certainly it survives as urban ethnography, rich i description of a way of life that still exists in isolated parts of old citie It reminds us of myriad unplanned small social arrangements which mak community life possible. And it represents one of the first documents whic should convince us that the peculiar built environment of the city makes difference in the way social life is conducted there.

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Book Reviews

A Poetic for Sociology: Toward a Logic of Discovery for the Human Sciences. By Richard H. Brown. Cambridge: Cambridge University Press, 1977. Pp. x+302. $19.95.

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For Richard Brown, artist and scientist, hermeneutic philosopher and survey researcher, the Marxist and the Parsonian are both engaged in the same fundamental task. They both play games of inquiry, games which, in the Wittgensteinian sense, are means to get on with explanation and social practice and are linked through the aesthetic quality of their undertaking. The differences among these and other such games derive from their different points of view, use of metaphor, and use of irony. Their similarity derives from the same factors. They both adopt a point of view and use it as a resource and instrument of reflection. They both must utilize metaphor as model, and they both use irony as an essential logic of discovery.

Thus, in sociology, point of view, conceived as aesthetic distancing, becomes a way to distinguish between and then integrate the language of the survey researcher (superior language vis-à-vis the subjects researched) and the language of the ethnographer (realistic language vis-à-vis the subjects researched). Neither is seen to be more scientific than the other. Each enriches our understanding of human life from its own aesthetic point of view.

Likewise, metaphor, the representation of something in terms of something else, becomes a way to understand the relationship among various main trends in sociology—for example, society as organism, as machine, as drama—and becomes a way of understanding the adequacy of the sociology practiced within these trends. Naive notions of the representational validity of sociological imagery of the "real" world become circumscribed by questions of the illuminating power of particular metaphors for our understanding of the various worlds in which we live.

Irony, conceived as the metaphor of opposites, a representation of something in terms of its antithesis, becomes for Brown the means of sociological discovery. Major new sociological problems become evident, not simply with discovery of covariance, but with the ironic incongruity of covariance —for example, most prominently, the Protestant ethic and the spirit of capitalism.

The advantage of the aesthetic approach for understanding sociological theory—Brown's "poetic"—is that the exclusivist claims to sociological truth made by competing traditions of inquiry can be reconciled. Brown elegantly demonstrates this when explaining the metaphoric basis of both Parsons's systems analysis and Becker and Geer's participant observation

(pp. 116–19). In gaining this advantage, scientific rigor need not be sacrificed. It is, instead, reinterpreted as a particularly powerful metaphor. Testing hypothesized factors in the real world becomes the discovery and elaboration of analogic metaphors. Brown points to the metaphoric basis of all sociological thought, not to debunk experimentation, hypothesis testing, and theory building, but to make manifest that which in the main is left latent. Inquiry can then be judged according to the adequacy of the generation and elaboration of metaphoric models through standard scientific procedures.

Nonetheless, a major problem with Brown's poetic does arise. He leaves unsatisfactorily explained how the relative adequacy of one metaphoric system (model) over another can be determined. In the realms of both theory and practice, this is at issue.

In the most appealing chapter of the book, the one on irony, Brown sets forth a possible resolution to the problem at the theoretical level. Drawing from the work of Kenneth Burke, he suggests that when subject matter is taken from its conventional context and placed in its opposite context, that is, made ironic, meaning is rendered at a higher level of self-reflective awareness and this determines the adequacy of the metaphoric system. Though such a resolution of the problem, itself ironic, has a certain cogency, explaining the appeal of many seminal works and the relative adequacy of any particular theory by the degree to which it heightens self-reflective awareness, the extreme subjectivism involved is not likely to convince many practicing sociologists, including this reviewer. The problem here is not unlike the problem facing contemporary aesthetic criticism in general. When ideals are replaced by notions of the relativity of judgment, evaluation of social science, as of art, becomes highly problematic, making sound art criticism and sound sociological criticism extremely difficult, if not impossible.

At the level of social practice, the problem is even more manifest. Brown asserts that "no set of behaviors is inherently a social problem (or anything else). Social problems are constructed. They are 'symbolic public realities' that emerge from a process of inventing and imposing definitions" (p. 228). Faced with world hunger, technocracy, genocide, and the threat of nuclear and ecological holocaust, Brown has problems sustaining his assertion. He recognizes the seriousness of his dilemma. He declares, "Our poetic for sociology sees formal thought as a game, but it also recognizes that the toy soldiers have real guns" (p. 230). But how does one decide upon the reality of the guns? This he leaves unexplained.

Brown approvingly notes Camus's characterization of Stalin's barbarianism as "crimes of logic" and interprets Camus to mean that Stalin's murders were design errors in formulating a practical paradigm for a new society. Brown, though, offers no aesthetic grounds for the rejection of such designs. He calls for a humanistic debunking of such "crimes of logic," but he fails to explain why humanism will have an appeal against barbarianism. Certainly such appeal is not based simply upon point of view, metaphor, and irony, as the logic of his poetic would dictate. Rather the appeal seems to be based upon an implicit morality. Ultimately Brown's *Poetic for Sociology* underscores the need for an *Ethics for Sociology*.

Despite the aforementioned problems, this book is highly recommended both as a useful antidote to the simple-minded neopositivism which characterizes discussions of "methodology" and "theory building" and, more significantly, for its systematic consideration of the aesthetic dimension as it informs the philosophy of the social sciences and as it has influenced the history of sociology.

Social Rules and Social Behaviour. Edited by Peter Collett. Totowa, N.J.: Rowman & Littlefield, 1977. Pp. 185. $14.00.

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This slim volume contains eight papers that examine the various notions of rule—especially those exemplified in the work of generative linguistics. Seven of the authors find the concept useful not only for describing but also for explaining behavior. Of the seven, two anthropologists speak least directly to the issue; one, Robin Fox, surrenders everything to rule, and the other, Paul Heelas, reasserts the old schism between nature and *kulturwissenschaften,* between *verstehen* and *begreifen,* the intuition of actor and the opacity of observer, between the realm of the cultural and that of the social. In each of these pairs, the former term belongs to the realm of semantic anthropology, a domain governed by rules peculiar to the indigines and understood, in any meaningful sense, only by them. This approach is opposed to that of cause-imputing social science. "Semantic anthropology is non-scientific in the sense that explanatory connections are established *via* the notion of rule rather than by applying causal theories" (p. 111).

There is something very slippery in the distinctions Heelas makes in support of a semantic anthropology. "The scientist," he contends, "cannot assume that thunder is related to lightning simply because participants happen to make a conceptual link between the two. The participants might be bad scientists or might not be scientific at all" (p. 111). But surely saying this is to confuse the issue. If the question is whether thunder has some connection with lightning, then the meteorologist will have useful suggestions. But if the question is how or why different peoples explain differently the concurrence of thunder and lightning, then participants' views become pertinent indeed.

Rules that fit or guide (describe or explain) human behavior stand outside the realm of science, Heelas maintains. Does not science itself embrace a system of rules? Does it not identify and describe recurring behavior? Is not the question of how beliefs cohere a legitimate problem for scientific inquiry? These are rhetorical questions, and I think Heelas is kidding himself and his reader when he reverts to Levy-Bruhl's two logics (of participation and contradiction) and draws a spurious line between the anthropology of participant-perceived meaning and that of observer-deduced interpretation.

Heelas himself acknowledges the distinction as spurious (and his paper, therefore, as wide of the mark) when he writes that certain anthropologists manage "to combine the notion of rule (and thus the semantic approach) with a causal (and thus positivistic) approach . . . [and that] the notion of rule apparently can operate in the context of . . . a scientific orientation. [Hence] . . . the distinction we have drawn between semantic and positivistic styles of inquiry is not always that [*sic*] easy to maintain" (p. 116). Indeed not. Heelas manages to confuse the meaning of action as seen by the actor with the implication of a pattern of behavior for the group, causes (given an outcome, what meanings and motives were at work?) with consequences (given differing patterns of behavior, what differing outcomes are to be expected?), and psychological levels of explanation with sociological ones. Thus a Durkheimian view of punishment is eccentric to, not necessarily incompatible with, reports of offenders' motives and perceptions of the deed.

Heelas should meet Fox, who, in the next chapter, "The Inherent Rules of Violence," finds it quite possible for the observer to describe the rules of ritualized aggression among 300 Gaelic-speaking Catholic inhabitants of Tory Island. He is persuaded that under ordinary, stable conditions, violence and aggression become ritualized according to definite if unarticulated rules that govern the occasion for violence, its stages of progression, allocation of roles (to women, to men, to holders back, to mediators), and mode of termination.

But Fox does not understand rules as do his seven coauthors. Rules are not the Hobbesian controls imposed to soften the war of each against all. According to Fox, who here echoes Aristotle, people are by nature social and by nature engage in enough collaborative activity to make life tolerable. So rules, whether constitutive (descriptive, taxonomic) or regulative (if this, then that), condition life, and not only the lives of *Homo sapiens*. And what the observer calls rules are simply "labelling devices for things that would happen anyway" (p. 134).

But the central issue is whether and how the structure of language is generated by rules that also map the structure of social life. Of the remaining six authors, five find that linguistics and the notion of rule are usefully invoked. The sixth, Roger Lindsay ("Rules as a Bridge between Speech and Action"), demurs.

Peter Collett, the editor, provides a persuasive invitation to the inquiry, Can we make reliable inferences about the rules that guide behavior (and their sanctions) from linguistic patterns? Can we detect isomorphisms between the structure of language and the structure of action? Can we represent sequences of behavior, in words and actions, by the same grammar of formal logic and in such mathematics as Markovian chains?

Rom Harré ("Rules in the Explanation of Social Behavior") invokes "the new sociology"—apparently Goffman and the ethnomethodologists—to suggest, first, that we have rules because the subject is able to monitor himself (his actions) as object, and second, that in our dealings with others we must see the meanings (rules) that transcend our idiosyncracies. Harré deplores work in the stimulus-response tradition that fails to take into ac-

count the shaping of human exchange by the culture-mediating actor. (Dewey expressed a similar position in 1896. And Mead, some 50 years ago, insisted that each of us shares in constructing the grammar—the rules—of social relationships.)

If Harré's piece is less than revelatory (and marred by a disconcerting tendency to cite a book he coauthored with Secord), so is the delightful piece by Peter Robinson ("The Rise of the Rule: Mode or Node?"). But if the caveats are familiar, they are also germane. Ignorance of history condemns us to repeat it; let us not preen ourselves over the rediscovery of the wheel. Furthermore, to speak of *the* rule is surely to understate its variety and complexity. We will err if we disregard either developmental complexities or the diverse ways in which rules are used by different categories of people—including those for whom secret rules yield power.

The most substantial contributions are those by David Clarke ("Rules and Sequences in Conversation") and Jerome Bruner ("The Ontogenesis of Speech Acts") and the final chapter in which Lindsay dismisses the idea of rule as either otiose (in describing regularities) or false in purporting to explain recurrences.

Building on his Oxford dissertation, Clarke observes that a sequence of actions may be arranged in chunks and stages and that relations may be discovered between these sequential behavioral units. Analysis of the temporal structure of a behavior stream can yield "structural regularities akin to a grammar" (p. 68).

Bruner's stimulating chapter links the generation of speech rules to the development of the mother-child relationship. (His discussion—Fox's is the only other such—builds on his own empirical research.) He argues, quoting Wittgenstein (and he might have cited Mead), that the meaning of language is given through its use. There is such a parallel between the structure of language and patterns of activity (patterns implying rules) that one must conclude that it is in the interplay between the slowly crystallizing rules of symboling and their successes (and failures) in eliciting cooperative responses from others that one finds the origins of language. It is "an extension to the semiotic sphere of rules learned in action. [In sum] grammatical rules are learned by analogy with rules of action and attention" (p. 108).

Lindsay's final chapter is tight, critical, and to the point. We may use the idea of rule, if we like, to suggest a taxonomy of regularities. But the imputation that rules cause recurrent behaviors is inadmissible. Here we hark back to Hume for whom there was nothing, beyond the establishment of covariance or concomitance, which could be called cause except by an act of faith. Lindsay is most skeptical of the *verstehen* part of Weberian method. He opts instead for *begreifen*. "It is unhelpful," he says, "to suppose that rules are in the mind of the behaver" (p. 171). He continues: ". . . idealised rules of language and behaviour fail to increase the predictability of relevant behaviour . . . where there are reasons for some regularity in behaviour, then rules are superfluous elements of its explanation, and

where there are no reasons it is hard to see why people follow the rule apart from 'mere habit' " (p. 171).

I have always admired the conviction, commitment, and clarity of the behavioral position. Yet it would be disastrous, were it possible, to forego the heuristic uses of theory, including inferred mental states. If Bruner is right, social intercourse engenders structured mental states that shape effective talk.

But in the end this must be wrong, an example of man putting asunder what God has joined. For social intercourse *is* effective talk. Symbolic interchange *is* social behavior. Language is the squeak of moving parts and the grammar of one must be the grammar of both.

Social psychologists, psycho- and sociolinguists, philosophers, and students of socialization will find this a provocative, if loose-jointed, set of papers. The book has a high stimulus quotient, offsetting the clutch of typos and a cost of 7½ cents per page.

Social Control of the Welfare State. By Morris Janowitz. New York: Elsevier Scientific Publishing Co., 1976. Pp. xxii+170. $9.95.

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This tightly packed volume presents a provocative set of diagnoses and commentaries on the emerging dilemmas of policy and direction faced by industrialized nations now governed by democratic states. Central to the analysis is the concept of social control as self-regulation based on moral consensus, in contrast to coercive control. As Janowitz sees the problem, underlying the obvious current issues of fiscal deficits, inflation, and unemployment are deeper crises of confidence, legitimacy, and coherent control. Indeed, the multiple dilemmas may remain unresolved until the difficulties lead to imposed authoritarian regimes; as a self-identified social democrat, Janowitz hopes for a successful process of institution building that can avoid such highly coercive solutions.

In about 150 pages of main text, the book tackles no fewer than six major tasks of description and analysis of the welfare state: (1) its ideological and symbolic origins and contexts, (2) its growth as an institution, (3) its political economy, including fiscal crisis and "stagflation," (4) its consequences for stratification and social organization, (5) its political effects, and (6) possible approaches for developing more adequate institutions of decision making and guidance. An enterprise of this scope inevitably deals with broad outlines and has to omit many important details; its virtues come from its incisive grasp of strategic social processes and structures.

The author's definition of a welfare state is explicitly value laden—intended to emphasize a linkage of democratic political arrangements with the welfare-state institutions. The welfare state is seen as the culmination of long-term processes of secularization, urbanization, and industrialization,

operating in conjunction with political philosophies that have justified popular participation and parliamentary regimes. A bridge between parliamentary governance and the welfare state has been supplied by religiously based "socialism." Secondary emphasis is given to science and secular rationalism, while economic analysis is given credit for devising the essential regulative mechanisms. But Janowitz holds that because it lacks "social and political adaptation," the practice of Keynesian economics contributes both to the emergence of chronic inflation and to a decline in concern with increased productivity. The current outcome is an entrenched system of deficit spending in the public accounts, while the "private" sector experiences high rates of both inflation and unemployment.

This "evolutionary" account is paralleled by a crisis, or "threshold events," version of the same developments. The main events in the latter are the Great Depression and World War II. Total war not only emphasized equalitarian (and universalistic) norms and stimulated demands for social and economic betterment, it also required that the masses be promised postwar reforms, and it highlighted social defects, which political elites now felt able to remedy by building and operating the welfare state.

The resulting system requires (1) a productive surplus in the economy, (2) an effective system of taxation, and (3) an effective and legitimized system of redistribution of goods and services. The present massive difficulties have emerged, so the diagnosis holds, as a consequence of the disappearance of the economic surplus because of high levels of private consumption, lowered rates of increase in productivity, and rising public expenditures. Janowitz's discussion of the complex problems of data and analysis, which impede such diagnosis, reinforces once again our urgent need for a genuine sociology of accounting and taxation.

The book focuses explicitly on the impact of the welfare state on the socioeconomic structure, political institutions, and psychological well-being of contemporary society. Actually much more of the text can be read from just the opposite standpoint: the impact of the society on the welfare state. Indeed, for this reviewer, the least convincing part of the total analysis is the alleged link between the welfare state and weak governments. Janowitz assumes that welfare-state policies do lead to political instability. But the welfare state itself has been shown to be a complex outcome of new demands, "needs," pressures, and "buy-offs" of the discontented. Instability and incoherence in government have sources in some of the same fundamental social conditions that have elicited welfare policies, for example, incoherence of social organization, relative deprivation, persisting inequalities, lack of adequate political mechanisms for aggregating and resolving discontent, and lack of coherence and direction of the traditional political parties. Granted that welfare-state policies are of great importance, we must not reify "the welfare state" as a primary source of societal ills.

Contrasting evaluations of the welfare state appear early in the book. After saying that welfare programs have "failed to eliminate poverty" and have escalated "sociopolitical conflict" (p. 1), the text then tells us that the welfare state has reduced human misery and was, in its time, the only strat-

egy compatible with political freedom (pp. 3–4). Thus the welfare state is necessary and useful. The need for change now arises because the evolved systems of taxation, redistribution, fiscal and monetary management, and delivery of services are not sufficiently flexible and have not developed either full legitimacy or workable modes of self-regulation.

One must hasten to add that Janowitz is well aware of the possibility that current "crises" may not lead to clear resolutions or to any really basic change, but only to a continuing condition of tension, indecision, half measures, and grudging and ineffective compromises. It is to avoid this uninspiring prospect that he urges analysis and action directed toward building new institutions of self-regulation (social control). For example, on the (obviously debatable) assumption that national elections are key (effective?) mechanisms of political control, he looks for new ways to elicit and channel citizen participation. The primary normative assumption is that there is a crucial need for effective and authoritative decisions with a minimum of coercion by the central state. To this end, the author proposes a search for stronger modes of participation in decision-making processes in each major institutional sector, for example, a more "responsible role" for trade unions in formulating national economic policy, "an American version of the German system of codetermination," and trade union involvement in collective problem solving at the plant level. Welfare services could be shifted toward community-based programs—perhaps centered in the local school system. Such reforms would be linked with "some form of voluntary national service" (p. 132). Many other suggestions are offered for broadening citizen participation and for developing political leaders.

Social Control of the Welfare State warrants close attention in the context of other major recent analyses of related phenomena—for example, works by Robert Heilbronner, Harold Wilensky, Michael Harrington, Daniel Bell, James O'Connor. It represents macrosociology without apocalyptic pronouncements or inevitable utopias. If many of its proposals for reform seem too limited to cope with the most serious structural contradictions, it has at least advanced testable ideas and proposals for possible actions rather than grandiose and dogmatic political dicta.

Métamorphose du bourgeois. By Jacques Ellul. Paris: Calmann-Lévy, 1967. Pp. 302. Fr 16,50.

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Much of Jacques Ellul's sociology consists of very general analyses that attempt to dispel illusions current in both academic and popular thought about contemporary technological societies. He focuses on illusions characteristic of the Marxist Left because they present the greatest potential for tragedy. The reason for this is twofold. First, the ideology of the Left has increased its political dominance almost everywhere in the world, and

therefore the illusions suffered by the Left are likely to become general and highly institutionalized. Second, it is only the Left that articulates values and goals to which, on the human plane, Ellul can subscribe, but if the Left labors under illusions about the nature of political and social reality, its efforts are at best likely to be wasted and at worst likely to result in defeat for the very values and goals it supports. It is Ellul's unceasing readiness to challenge the intellectual fashionableness of Marxist thought that largely accounts for the scant attention and profound misinterpretation his work has received in intellectual circles, despite his brilliance and the fact that he is himself thoroughly schooled in the entire Marxist tradition as only European scholars can be.

In *Métamorphose du bourgeois* Ellul applies his uncommon comprehensiveness and unrelenting analytical logic to a recurrent and recently revitalized illusion central to most Leftist and Marxist thought: that the class structure (and, hence, the politically dominant groups) of technically developed societies do not differ significantly, especially in their determinants, from those analyzed by Marx in the last century. He argues that this is plainly not so. Classes have disappeared, if by "class" we mean what the term signified in Western Europe from the late Middle Ages through the 19th century: categories of individuals and families having fairly distinct economic roles that accompany rather sharp differences in life-style, modes of thought, values, and so on (and, usually, possessing class consciousness that derives from the political struggles that develop along class lines). Both bourgeoisie and proletariat constituted genuine classes in the last century. But the relationship to the means of production no longer serves to distinguish among classes, even though we can still identify distinct sets of economic roles that stand in different relations to the means of production, and even though it is certainly true that one set of roles exploits other sets of roles.

The diagnostic error underlying the illusion that class structure has not changed significantly is the failure by Marxists to identify accurately the defining characteristics of the bourgeoisie. It is not the ownership of private property that sets the bourgeois apart, nor is it his dependence on money and commercial exchange. For Ellul the bourgeois is characterized most profoundly by two factors: the ideology of *bonheur* (roughly, a sense of well-being), and the power of assimilation. What the bourgeois sought from the beginning was neither profit nor self-interest so much as, first, spiritual comfort in the face of an unyielding natural world, and second, acceptance by society at large, acceptance that was withheld for centuries because the bourgeoisie was a pariah class whose very basis of existence was deemed sinful. The ideology of bonheur eventually translated into an ever-expanding level of material consumption that satisfies the senses and entertains the mind so that spiritual anguish need not be feared. The drive for acceptance led the bourgeois class to develop means of assimilating the entire globe into its world view and technology, thereby creating, over the past four cen-

turies, a bourgeois world in which the only values and means that count are those initially formulated by the bourgeoisie.

Thus we have all become bourgeois, in action and in spirit, though not necessarily as members of a bourgeois class. Consensus on the fundamental parameters of human life is quite general. The dominators and the dominated, the exploiters and the exploited, share a common set of basic goals, values, and means: a rising standard of consumption, nationalism, striving for prestige, rapid technical development, the strong state, and so on. This situation contrasts sharply with that of genuine class society in which class identity carried with it a large set of social identity characteristics that differed among the classes, and consensus on goals, values, and means was almost unthinkable. Now the major form of social identity is occupation—"... on ne demande 'Qui est-il?' mais 'Que fait-il?' "—and "cultural" differences among occupational groups are rather trivial and diminishing. The important issue is no longer the nature of classes and class struggle but rather the identification of what Ellul calls the "successors" of the bourgeoisie.

Readers familiar with Ellul's work should have no trouble anticipating his position here: The successors of the bourgeoisie are the managers and technicians who operate the technical development process. They are engaged in bringing to fruition the project initiated by the bourgeoisie: complete assimilation of the world into the bourgeois mode and complete institutionalization of the ideology of bonheur until society becomes entirely devoted to its several elements: conspicuous consumption, convenience, ease, absence of responsibility, and avoidance of the necessity of making painful choices. Everywhere, even in the technically less developed countries, these successors have gained control of the state and embarked on the bourgeois project.

Here, then, lies the danger of the Marxists' inability to update their class analysis. The Left still focuses its energy on wresting political control from the bourgeoisie, without realizing that the bourgeoisie has metamorphosed from a social class to a societal structure that has made the Left itself thoroughly bourgeois. When the Left gains control of the state, it only hastens bourgeois development while imagining that it is proletarian, humanitarian, etc. The Left refuses to recognize that removing the outmoded bourgeois capitalists from the political scene has very little impact on the nature and direction of technical development, for it is not the capitalists who direct such development; it is the *cadres* and *techniciens*.

The final third of the book discusses the spiritual impact of embourgeoisement on the world. Ellul argues that the quest for bonheur in fact produces exactly its opposite: *le néant*, or nothingness, by which he means the prevalent drive of bourgeois technical society to eliminate even the possibility of meaning in life. He discusses this drive in relation to contemporary society's excesses in art and literature, its structuralist anthropology, its execution of God, its tendency toward totalitarianisms, its rapid creation and destruction

of ideologies, and so forth. "Chaque plus produit un moins," he says: We have more and more means of seeking happiness, but we are less happy. We have lost the art of being happy. The more we develop our means, the less we gain from them, and the more difficult it becomes to control them. Even humanism, which we usually consider as an attempt to protect the integrity of the individual and preserve meaning in individual life, has the opposite effect by serving as a future-oriented ideology that can be invoked by transgressors against humanity to justify their transgressions: "Blood must be shed now so that our children will live in the perfect society in which the individual is the highest value." These inversions of the expressed intentions of bourgeois development occur because what the bourgeois project seeks, at the deepest level, is the development of power in the pure state: power for its own sake, the expansion of means to their limits, the growth of control regardless of other considerations. The excesses of technical development derive from this essential spiritual emptiness.

Readers familiar with Ellul's other sociological writings will find this a highly convincing book. Other readers will be more skeptical because a number of key points depend on knowledge of his analyses in other places (a problem typical of his work). One serious difficulty with the book is that its underlying methodology is both very unfamiliar to American sociologists and almost completely unexplained. Ellul relies heavily on literary sources for his characterization of the bourgeois, but he does not justify his choice of sources. We must depend on his judgment, formed in the "French historical school" that has produced Bloch, Morazé, Chaunu, etc., but this school carries rather little weight among American readers. He insists on preserving the natural-language meaning of terms (linguistic usage, he says, is the reflection of the real), but again he does not justify his decisions as to when to apply this principle and when to go beyond it. For example, in arguing that the bourgeoisie as a class has disappeared, he points out that in contemporary society one never refers to oneself as a member of the bourgeoisie (even in notoriously bourgeois France); if the bourgeois class persisted as an effective social entity, this could not be the case. But he is quite ready to dismiss the prevalent use of class identity in Cuba or China because it is the result of state-directed propaganda, even though class terminology has entered the "natural" language (which is in fact partly artificial now) in those places. While I am in accord with this decision, I am troubled by his failure to explain the basis for it.

Along this same line of criticism, Ellul's steadfast refusal to define troublesome terms—in particular, the term "class"—leaves him open to the charge that classes are dead only with respect to his concept, which may not be a relevant or useful one (the characterization of class given earlier is my reading of his usage of the term). He insists on including social aspects in his concept of class, but many analysts, especially Marxists, have retreated to a purely economic definition. I think the careful reader can discern his concept clearly and see that it is in fact quite faithful to the

historical entities it characterizes, but the lack of explicit definition burdens the reader. His position is that any definition will be arbitrary and self-serving, but this leaves us to struggle with his implicit concept.

Two further problems deserve mention. By relying on literary rather than academic characterizations of the bourgeois, Ellul dismisses out of hand the common claim that the primary concern of the bourgeois has always been profit or self-interest. He gives no direct justification for this dismissal, relying instead on the force of his overall argument to support it. But under the hypothesis that the competitive capitalist system placed the bourgeois in a situation in which he had no choice but to make profit his primary concern, plausible arguments have been made that some of the characteristics emphasized by Ellul, in particular the assimilative power of the bourgeoisie, are more or less accidental results of the profit-oriented system. He develops no direct refutation of these arguments, though his analysis could well move in this direction.

A final difficulty with the book is that, while it is written from the perspective of Ellul's radical Christianity, which generally follows the tradition established by Karl Barth, he nowhere in this work explains either this perspective or how it shapes his analysis. One of the major themes in all his work is the demonstration that human beings in advanced technological milieus are no freer than those in traditional milieus. Here he shows that we are captives of the bourgeois mode of existence, despite all the pretensions to the contrary. He develops this theme because freedom is his central Christian concern, and he sees that although we are not free, we tend to believe that we are. One must read his theological writings to understand fully his concern for freedom and to see why he takes Marxists so severely to task for restricting their concerns to such issues as inequality and exploitation. Again, the problem is that the reader must carry a heavy burden. I should note, however, that the fact that he does not explain his perspective is the result not of oversight but of a deliberate choice, for reasons that cannot be elaborated here.

This is a very important but difficult book. It is Ellul's most literary work and therefore poses the greatest problems to the English-speaking reader. Its great value lies in the fact that it goes beyond most discussions of class structure by fully taking into account 20th-century changes in the world-wide technical structure whose significance Marxists refuse to recognize. It also helps alert us to problems that Marxists prefer to ignore. Ellul certainly shares the Marxist concern for the evils of exploitation, but he shows that exploitation (whose demise, contrary to Marxist thought, is a necessary result of the bourgeois project in the long run—once everyone is thoroughly bourgeois and fully involved in the technical system) is by no means the most fundamental difficulty we face. Ironically, as Ellul has shown elsewhere, the means being pursued to end exploitation create even more intransigent problems in their turn.

Sociology and Jurisprudence of Leon Petrażycki. Edited by Jan Gorecki. Urbana: University of Illinois Press, 1975. Pp. xi+144. $6.95.

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To this day, the sociology and jurisprudence of Leon Petrażycki (1867–1931) have reached not much beyond a small circle of Russian and Polish admirers and critics. Parts of the Russian jurist's work, introduced and edited by Nicholas S. Timasheff and translated by Hugh W. Babb, were made available to the English-speaking world in 1955 (*Law and Morality* [Cambridge, Mass.: Harvard University Press]), but there was no perceptible echo in American scholarship. Jan Gorecki's volume is an attempt to break the silence and have us recognize Petrażycki's place "in the first row of grand theorists of man in society—on a par with Spencer and Durkheim, Marx and Weber" (p. x). There is indeed much of value in Petrażycki's work, but this book will not help discover it.

The volume is a hastily assembled collection of eight short papers, six of which are original contributions. The writing is rough, the organization loose, the content repetitive. Three pieces (two by Gorecki and one by Jerzy Lande) and part of a fourth (by Alexander Peczenik) offer introductory summaries of Petrażycki's ideas, with much overlap. Two others sound like answers to essay questions for a graduate school examination: "Compare Parsons and Petrażycki on structure and function" (by Harry M. Johnson) and "Compare Mead and Petrażycki on social psychology" (by Norman K. Denzin). The rest consists of five pages of improvisation by Tadeusz Kotarbinski on criteria for an "adequate" theory, an excursus by Peczenik on his own views about legal reasoning, and a short critical essay by Maria Ossowaska on the relation between legal and moral norms.

More important than defects of form is a basic flaw in the conception of the volume. The papers are intended to present Petrażycki's ideas "in the context of more recent developments in scientific knowledge" and thus to "help in absorbing and utilizing all those elements of his system which surpass what has been later proposed by others" (p. x). Accordingly, the contributors' attention is focused upon identifying those of Petrażycki's formulations which are "superior" or "original" in comparison with current social science theorizing. The line of retreat is to argue that Petrażycki's ideas, such as his conception of learning, "preceded" later developments of modern science, such as Pavlovian conditioning.

On its face, this venture looks innocent enough, and one could hardly quarrel with it on general grounds. But like all intellectual efforts, this one has its own risks. The chief risk in this case lies in reading ideas, contemporary as well as past, out of their intellectual context. Some ideas do not take well to grounds other than the special environment of their birth. This is especially true of polemical writings. To one who is looking for powerful and seminal insights or close and durable formulations or incisive analytical clarifications, reading Petrażycki—that is, his translated work on which my

impressions are based—is bound to prove disappointing. No doubt Petrażycki was a creative, if not eccentric, thinker. Some of his style and vocabulary reminds one of Pareto: "impulsions" and "phantasmata" evoke the rhetoric of "residues" and "derivations." But analytical depth was not Petrażycki's forte. His work is a long and relentless polemic against "the terminology of jurists and their ideas of law." These jurists are in turn criticized for accepting "as real legal phenomena" what are mere "impulsion phantasmata": Petrażycki urges us to let the scientific study of psychological experience take the place of the "naive-projection point of view" that prevails in legal thought (*Law and Morality*, p. 62). But in polemics, the dynamics of battle distract from intellectual problems as much as they reveal them. Consumed by his passion, Petrażycki caricatures legal ideas and simplifies his own, asserts his claims more than he tests or elaborates them, hammers over the same points endlessly to such an extent that Timasheff and Babb found it a "herculean task" to cope with his "almost incredibly repetitious prolixity" and to remove all the stuff that was "important only at a particular time . . . , or . . . solely polemical" (*Law and Morality*, p. xxxix). Even after their patient editing, *Law and Morality* remains obsessively repetitive. It should be no surprise that, as Gorecki and his colleagues look in Petrażycki for theoretical formulations of enduring force, they must constantly retreat to acknowledge the obvious excesses of their subject.

Petrażycki hardly needs this sort of friendly attention. His work belongs to a familiar genre of "theoretical" writing: the programmatic manifesto. Many other students of law and social science—for example, Ehrlich, Gény, Pound, the legal realists, much of the present-day "law and society" movement—have favored that genre. All of them saw a convergence of the broad intellectual aim of furthering the empirical study of legal phenomena and an equally diffuse but peculiarly modern aspiration to rescue legal institutions from the rigidity and formalism of an earlier era. An agenda of legal reform was thus couched as a call for scientific inquiry, a call suffused with passion. And the call, like the agenda, took on the character of an exhortation: Detailed and incisive in the critical mode, it became elusive, though inspiring, when it shifted to the affirmative mode. "Let the law be more rational and it will be better. The proof is: look at all instances of bad law that can be traced to ignorance." The programmatic manifesto debunks better than it proposes. But then such are its virtues. There are times of intellectual and institutional transformation during which unsettling the received tradition is a necessary prelude to finding new directions. American legal realism is a case in point: a few gross, unanalyzed ideas, combined with massive doses of demystification, prepared a vigorous redirection of legal scholarship.

Those "law and society" movements share more than a rebellious antiformalism. Underlying the variations of language, there is a striking continuity of themes and emphases. Like all other versions of sociological jurisprudence, Petrażycki's seeks to detach the idea of law from state law,

to stress the multiplicity of sources of law, to draw attention to the embryonic manifestations of law. Where others counterposed "law on the books" and "law in action," or "*sources formelles*" and "*sources réelles*," he distinguishes between "positive" law and "intuitive" law. With other sociological jurists, he shares an abiding conviction that law can become at once less coercive, more effective, and aimed at higher moral values. Accordingly, he also joins them in expressing renewed sympathy for the natural law perspective and in calling for the development of a science of legal policy, or "legal engineering." Some of Petrażycki's faith in the laws and the promise of legal evolution reflect an era when confidence in science and Progress had not yet been shaken by the assault of modern skepticism. The legal realists of more recent times have not spoken with the same assurance. But their greater caution cannot distract from the basic similarity of their grand intellectual and moral concerns.

In other respects, however, Petrażycki's approach is strikingly unique, and indeed quite alien to the ethos of Western sociolegal theory. Though most of us would readily accept the idea that law has educational functions, among others, we are not likely to make that idea the central postulate of a "science of legal policy." But to Petrażycki, psychological knowledge of how law shapes the motives and characters of persons is the key to improved legal engineering. Furthermore, he sees the educational force of law in its capacity to induce and reinforce attraction or aversion to desirable or undesirable behavior. That approach, in turn, has critical consequences for Petrażycki's legal theory—or is it rather that his legal theory disposes him to such an approach? If we begin with the premise that reinforcement is the key to education by law, then it becomes a central requisite that the application of legal sanctions and rewards be consistent, sustained, and certain. In such a system there should be little room for those virtues of openness, flexibility, or creativity in the administration of justice that have so often and so intensely been sought by Western legal realists. The task of jurisprudence should rather be "to bring legal relationships into unity and to make them objectively definite and incontestable" (*Law and Morality*, p. 142). Hence, the science of legal policy must speak to legislation, not to the subordinate functions of administration or adjudication. But the argument can be turned upside down. Perhaps a legal theory centered on legislative omnipotence finds the notion of conditioning especially congenial when it seeks to account for the educational function of law.

In Petrażycki's vision, the relation between the legal order and its participants or subjects is direct and unmediated. There is little sense that any third set of forces, such as a group structure or some institutional dynamics, intervene between law and the person. Perhaps such a neglect is to be expected from a theory that locates the reality of law in psychological phenomena. But Petrażycki's psychology fits within a larger framework. That framework offers no place for a recognition that special interests play a part in shaping the law, limiting its reach, or frustrating its authority. Thus Ihering's "theory of interests," which was later to inspire Pound's

sociological jurisprudence, and which today is echoed in pluralist interpretations of law and politics, was a favorite target of Petrażycki's polemical impulse. He found it difficult to accommodate conflict and self-serving claims in a conception that made "active and universal love" the end of moral evolution and legal engineering. A profound aristócratic disdain for this lower side of mankind pervades Petrażycki's account of the idea of rights. Rights, he thinks, are the criterion for distinguishing morality, which only creates obligations, from law, in which obligations are backed by corollary claims of entitlement; in his language, legal "impulsions" are "imperative-attributive" and not just imperative. It turns out, however, that rights are primarily to be seen as enlisting the pressure of claimants to bolster the authority of corresponding obligations. Hence, he argues, it is "extremely naive and superficial to suppose" that "rights are established in the interests of those to whom they are given" (*Law and Morality,* p. 175). Like moral obligations, legal ones are designed to serve "the welfare and the prosperity of societies and the spiritual and cultural education of mankind" (*Law and Morality,* p. 175). Indeed in the end, mankind should reach a stage at which the sense of obligation, developed by cumulative reinforcement, will no longer have to lean upon the crutch of rights or coercion but will rest only on the highest altruistic motives. At that point, law will have rendered itself superfluous.

One is tempted to conclude that only if *Walden Two* were the bible of modern Law-and-Society could Petrażycki be elevated as a prophet of sociological jurisprudence. But our conditioned aversion to this kind of approach may well be the sign that something is there to be learned. Seeing how Petrażycki's theories reflect a social and intellectual context, including an implicit conception of the legal order, should help us discover the hidden parochialism of our own approach to law and social sciences. Besides, that exercise might eventually produce a fascinating, but still unwritten, chapter of modern intellectual history: a comparative study of those currents of "realism" that traversed and, to varying degrees, shook so many modern legal cultures in the early decades of this century. Surely Petrażycki would be one of the cases worth studying, along with Pound, Llewellyn, Gény, Ehrlich, and others of like mind. That is the kind of attention he can use: the sensitive appreciation of a student of culture.

The Structures of the Life-World. By Alfred Schütz and Thomas Luckmann. Translated by Richard M. Zaner and H. Tristram Engelhardt, Jr. Evanston, Ill.: Northwestern University Press, 1973. Pp. 335. $12.00.

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The Structures of the Life-World was published in 1973 after the death of Alfred Schütz in 1959. Thomas Luckmann, a close and devoted student of

Schütz's, undertook the difficult task of completing this volume, which Schüt had prepared in notes and outline form shortly before his death. As Luckmann states in the preface: "In a sense this book is the *Summa* of Schütz' life, and as such it is his book alone. In another sense it is the culmination of the work of many authors, among whom Schütz is the most importan and I am merely the last."

Those familiar with Schütz's earlier works, *The Phenomenology of the Social World* and his numerous essays in the three volumes of *Collected Papers,* will find this latest contribution a more fully detailed and richly descriptive effort to integrate the major themes of Schütz's life work. This is a book that will best be appreciated by those who are already sympathetic with the aims of phenomenology and who can situate Schütz's work as part of a larger programmatic effort to redefine the nature of social reality and to challenge the ultimacy of scientific method as the exclusive means for achieving knowledge of that reality. For the uninitiated and for graduate students who are beginning to take an interest in these issues, Schütz's essays "Concept and Theory Formation in the Social Sciences" and "Common Sense and Scientific Interpretation of Human Action" are recommended as preparatory reading, together with the lead essay by Maurice Natanson in volume 1 of *Phenomenology and the Social Sciences* (ed. M. Natanson, 2 vols. [Evanston, Ill.: Northwestern University Press, 1963]).

With this background, the reader will be in a better position to understand the fundamental significance of Schütz's work as he endeavors to elaborate and build on Weber's concept of *verstehende Soziologie* and extend Husserlian insights about *Lebenswelt* into a new foundation for the social sciences. For central to Schütz's effort is the conviction that the abstract, deductive, and nomothetic orientation of contemporary social science, which unreflectively accepts the applicability of the natural scientific method for an understanding of social reality, imposes an intellectual construction or theory onto that reality and interprets it in terms of those artificially constructed categories. In distinguishing between first-degree and second-degree constructs, what Schütz endeavors to demonstrate is that social reality is already constituted by a shared meaning structure in terms of which men orient their actions and understand the actions of others. The primary obligation of social science is to understand the already-constituted shared meaning structure, that is, the prescientific life world, and to limit concept formation to second-degree constructs that may elucidate the social reality so constituted. By contrast, the method of the natural sciences permits a free mental construction as an explanatory or heuristic device (i.e., first-degree constructs) precisely because the object of inquiry has no preconstituted meaning structure of its own. If the method of naturalism is transposed unreflectively to the social world, knowledge of that world consists of an abstract, intellectually imposed interpretation (from without) rather than a descriptive and subjectively empathic account (from within).

Schütz's focus on the "natural attitude" and the "life-world" is thus an attempt to describe in rich detail the everyday life world in which we live,

naively, as it were. The social world which we occupy and in which we experience ourselves and others is the world of common sense and one which is taken for granted as our shared reality. Through concepts such as "biographically determined situation," "stock of knowledge," "thou-orientation," and "we-relation," what Schütz attempts is a phenomenological analysis of that shared social reality which is otherwise unreflectively accepted as given.

This volume then, which is the first part of a larger projected work, lays the groundwork for a phenomenological social science. The second volume, soon to be published, will be concerned with concept and theory formation and will, it is hoped, join the issues directly and spell out the implications of Schütz's method with as much rigor and detail as one finds in the present volume.

Several problems come to mind in reflecting upon Schütz's work and in attempting to assess its relevance for contemporary social science. As Natanson and Luckmann make clear in their respective essays in *Phenomenology and the Social Sciences,* Schütz's work is to be seen as a direct outgrowth of Husserl's examination of the crisis of the European sciences. Husserl's concern is the gradual separation of science from philosophy and the emergence of a nonphilosophically grounded social science based on the mathematical foundations of the successful physical sciences. Schütz's attempt to recover the prescientific life world is preparatory to reflection about human consciousness and human essence. Within the context of an emerging technocracy, in which theoretical social science is seen as a legitimate instrument in the social reconstruction of reality, Schütz's project may be viewed as a revolutionary departure from established orthodoxy. Were Schütz's program to become a paradigm for the social sciences, it would effectively destroy the legitimacy of the ameliorative function of the social sciences and confine their practitioners to the tasks of ethnographic description of the social life world. Whether one views such a development as radical or conservative will depend on one's conception of the proper role of social knowledge in fostering social change. Peter Berger's *Pyramids of Sacrifice* (Garden City, N.Y.: Doubleday, 1976), for example, explicitly spells out some of these implications for one area of social policy from the perspective of a close student of Schütz's. Yet to agree that "technocratic domination" is the central issue of our time does not necessarily imply agreement with Schütz's program, as can readily be seen from the writings of Jürgen Habermas. Here, for example, we find a conception of a "critical social science" committed to the idea of emancipation and performing a "steering" function in the transformation of society. Whatever the outcome, it cannot be denied that Schütz's work will continue to be a provocative challenge to social science orthodoxy and an important contribution to the epistemological debate over a new paradigm for social inquiry.

What Are Norms? A Study of Beliefs and Action in a Maya Community. By Francesca M. Cancian. New York: Cambridge University Press, 1975. Pp. x+212. $14.95.

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The author informs us right off that "the history of this study is also the history of my change from a Parsonian to a 'social construction of reality' sociologist" (p. vii). It is important that Cancian had no stake in the verification of her initial hypotheses and that she was able to reject not only the hypotheses but the theoretical orientation from which they were derived. This is not something all social scientists can do. In a session on the attitude-behavior controversy during the ASA meetings this past September, the chair announced that over the past decade it had become increasingly clear that there is a direct relationship between attitudes and behaviors. This historical distortion, stated without regard to serious arguments and massive evidence to the contrary, was blandly supported by his panel (one of the psychologists on the panel announced that he is able to reject all evidence that he doesn't like by subjecting it to a rigorous methodological critique and then discarding it). Such is the way of science.

One member of that panel sagely observed that "words and deeds" are not the same as "attitudes and behaviors." Cancian deals with "norms and actions," and that too is a different pair. But there is a common assumption threading through all such dichotomies, and Cancian, without a stake in the outcome, provides one more bit of evidence that the assumption of consistency in human orientations toward the same social object is untenable. She demonstrates that generally shared norms in a small society bear little relationship to the actions of individual members of that society.

The community Cancian studied is a collection of Mexican hamlets called Zinacantan, a site exploited by the Harvard-Chiapas project since the late 1950s. The people speak Trotzil, a Mayan language. But, regardless of the site and of her familiarity with the culture and the language, Cancian's work is not primarily ethnographic. Furthermore, it could have been undertaken in any relatively homogeneous community: a black or Jewish ghetto, a residential college campus, a mental hospital, a penitentiary, or a midwestern town. It would, in fact, be a useful contribution to knowledge to verify Cancian's findings in a setting different from the one she studied.

The author cleverly adapts the technique of ethnoscience as a method for identifying community norms. This involves analyzing the subjects' linguistic distinctions in order to isolate normative categories both from one another and from the nonnormative arena. All of this is described in detail in the second chapter (following her introductory discussion of the study of norms and preceding her description of the research setting). Her general description of ethnoscience in chapter 2 is applied to her specific problem in chapter 4, where she describes a "frame-sorting method for describing norms." This begins with a kind of sentence completion test (e.g., "He is

respected because . . .") and proceeds through several sorting procedures with the help of local informants. Chapter 5 describes the hierarchy of clusters of "good" and "bad" among Zinacantecos as they emerge from the sorting procedure, and in chapter 6, verification and reliability procedures are reported.

The second part of the volume begins in chapter 7 with a discussion of the theory of the socialized actor who, having successfully internalized the norms and values of the society, behaves in an appropriately socialized manner. Cancian blames this endemic sociological perspective on Talcott Parsons. (Consistent as it is with his views, it was well entrenched before him and would, I suspect, dominate contemporary textbooks had he never existed.) In good deductive fashion, the investigator formulates three hypotheses, positing three kinds of consistency between norms and action. She then develops measures and conducts a survey in order to provide a test. All three hypotheses are falsified with the disconfirming evidence reported in chapter 8. Painfully recognizing the failure of her Parsonian orientation to fit the facts as she has recorded them, Cancian seeks an alternative explanatory framework which is in accord with her empirical observations. What she patches together in the final chapter is a bit of "symbolic interaction," a touch of "social construction of reality," and a pinch of "reference group." This new perspective she calls the "social identity approach"—an awkward term for a situational sociology. The book closes with a discussion of evidence—both from the literature and from Cancian's own field work—in support of the "social identity" approach.

I had assumed when I started this volume that, with the wealth of observable action available to the investigator, Cancian would provide a comparison between the normative structure and observed behavior in its natural context. This is not what she does. Rather than observed behavior, she analyzes the relationship between self-reported behavior and norms. Richard LaPiere conducted a study of French hotelkeepers prior to his famous "Attitudes vs. Actions" (*Social Forces* 13 [1934]: 230–37) and was criticized for comparing two sets of verbal responses. It was this criticism which led him to undertake the 1934 study in which verbal statements could be compared with observed behaviors. It is ironic that Cancian should make the same mistake that LaPiere recognized in his own research so many years earlier.

Cancian has achieved much of what she claims and perhaps more. Her book is well organized and her writing reflects the clarity of her thinking. The theoretical contrast between the socialized actor, compelled by socialization, and the situation-defining actor, constructing lines of action in an improvisational mode, is well done and illuminating. Her methodology is imaginative and credible and her measurement techniques are worth borrowing. Finally, the author's ability to see the conceptual relevance of research in esoteric substantive areas provides a model of synthesis which is rare in literature reviews. Cancian has contributed considerably more than another piece of evidence regarding the tenuous relationship between words and deeds.

The Form of Sociology: Paradigms and Crises. By S. N. Eisenstadt, with M. Curelaru. New York: John Wiley & Sons, 1976. Pp. xvii+386. $17.95.

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The Form of Sociology should afford sociologists a heightened sense of the theoretical problematics which have crystallized throughout the history of their discipline. Writing in an analytic style reminiscent of Joseph Ben David and Robert Merton, S. N. Eisenstadt and M. Curelaru emphasize the effect of the institutionalization of sociology on the development of its theoretical constructions. Their book is informed with the erudition and authority required by the scope of its argument, and those who have not had the opportunity of being among Eisenstadt's students owe the authors a debt for making his thoughts available to a larger audience.

The development of sociology is divided into four periods: the forerunners, about whom little is said; the founding fathers, with whom institutionalization begins; sociology between the wars; and development since World War II. Within this last period, analysis of which takes up more than half the book, the authors discuss first the structural-functional model, then the "countermodels," including conflict and exchange theories, symbolic interactionism and ethnomethodology, cybernetic and neo-Marxist models, and the "symbolic structuralist model" (e.g., Claude Lévi-Strauss). The book's most original chapters follow, wherein the "metascientific controversies" in contemporary sociology are evaluated.

This evaluation parallels the development of sociological theory itself. The substantive breakthrough presented as "[t]he most crucial phase of development of modern sociology" (p. 101) is the transition from closed to open system models. The open system approach abandons the search for "fixed relationships" among "concrete entities" and focuses instead on interrelationships and feedback processes. In the open system model "each element, although relatively independent or autonomous, also constitute[s] a basic systemic referent of the other components" (p. 91).

If a single key to Eisenstadt's thinking is available, it may be the usage of "openings." On the one hand, the term refers to this transition from closed to open models; on the other, paradigmatic groups themselves may be relatively open or closed, and it is with respect to this sort of openness that the relative "productivity" of metascientific controversies is judged.

The closed paradigmatic groups, to which the authors object, have fostered ". . . the transformation of sociological 'schools' into metaphysical and ideological sects, each with its own combined physical, political, ideological, and analytic paradigms, and all developing strong symbolic closure and patterns of esoteric personal or sectarian discourse" (p. 326). The identity of particular groups is not specified, but a footnote refers to articles discussing contemporary theorizing in Germany and the Netherlands. Open groups, on the other hand, are distinguished by ". . . some orientation to broader 'external' trends—philosophic, ideological, or intellectual—that is,

mediated nevertheless by a very strong *internal* core of analysis focused on some aspect of the sociological *Problemstellung*" (p. 329).

The content of this sociological *Problemstellung* is the subject of the entire book, but the following statement may not be an entirely inadequate summary: ". . . the key problem of social order, the original Hobbesian problem, was rooted [by sociology, as it emerged] not in a presocial 'state of nature' but in the very construction of organized social and cultural life and the division of labor; and above all in the confrontation between the organizational givens of the social division of labor and multiple individual goals" (p. 348). Thus the book's influences must also include Talcott Parsons; *The Form of Sociology* may be read as an attempt to demonstrate a sort of latter-day "middle range" convergence theory, the basis of which is presented in the book's final chapter. This theory, in keeping with the concept of openness, is less a version of some underlying essence of social reality than a systematically stated set of issues, derived from the traditional *Problemstellung,* to which any sociological theorizing must be attendant.

As a systematization of traditional problematics, the book makes a clear contribution. This recommendation, however, must be tempered. *The Form of Sociology* seems to deserve a comment once made to me about the utility of IBM user manuals: "If you know enough to be able to understand them you don't need to read them." The authors intentionally offer only cursory explications of the contents of the theoretical models discussed, but for readers well enough informed not to require such explications, the newsworthiness of some of the book's summary observations may be questionable. Lacking the space required for the extended substantiation such a criticism requires, and choosing not to quote brief passages out of context I leave this issue to readers' evaluations.

More specific criticism can be made of the use of brief quotations from "outsider" secondary sources to summarize certain paradigms. While such quotations may address the letter of the theories in question, "insiders" may feel that they sacrifice the radical distinctiveness of the alternatives being proposed. If a shortcoming along these lines could be demonstrated—and an extended argument would be necessary to do so—the argument of the book would be undercut. The authors' present interpretations allow them to reduce the various models to either "productive" openings in, or counterproductive distractions from, the more or less unified *Problemstellung.* There are models and countermodels, variations on a single theme, rather than independent paradigms. Were the authors to allow greater analytic discrepancy between paradigms—were the paradigms understood as alternative conceptualizations of what would now be a multifaceted *Problemstellung*—the concept of openings would require redefinition, and the criteria on which the paradigms are evaluated, criteria whose validity requires the postulation of a unified *Problemstellung,* would require revision.

Thus by using "outsider" statements to describe, and possibly to neutralize, various paradigms and then by using Merton's work as a metacritique to deny the validity of "insider-outsider" distinctions (see pp. 311

327), the authors have effectively negated the potential of various para digms as alternatives, in any strong sense of that word. Although the book title suggests a variety of paradigms, the authors seem to employ a mor strictly Kuhnian concept of a single paradigm dominating the disciplin The task of the book is to delineate—critics might say to legitimate—th paradigm, the traditional sociological *Problemstellung*.

Finally, the book lacks what might have been desirable in terms of em pirical specificity concerning two of its main themes, the shaping of theor by both the institutionalization process and the conduct of research. O both themes the authors are more suggestive than illustrative; they rarel describe particular cases of institutional arrangements or research project affecting specified theories in ways which they enumerate. Although th book is heavily footnoted, there is a certain lack of citations at points o criticism with regard to controversy, and this silence creates an unfortunat analytic looseness; for example, who are the sociologists within the "closed" paradigms to which the authors object? Greater specificity, however, would have obligated the authors to engage certain theoretical positions, particu larly the countermodels, at a level of interpretation for which they are no at present responsible, as their argument stands.

Sociologists thus have much for which to be grateful to Eisenstadt and Curelaru, and much to expect.

Klassiker des soziologischen Denkens. Vol. 1: *Von Comte bis Durkheim*. Edited by Dirk Käsler. Munich: Verlag C. H. Beck, 1976. Pp. 532. DM 54.

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This volume of essays on the classics of sociological thought begins with a series of reflections on the possibility of a history of sociology. For the sociologist, we learn, the fundamental aim of such a history is self-knowledge. Without a clear grasp of the historical underpinnings of his discipline, the sociologist cannot possibly understand its contemporary problematic. If the gaps in his historical understanding are profound and extensive, then he cannot even comprehend what it means to be a sociologist. It follows that there is an important sense in which he does not know what he is doing: He has no knowledge of the sense in which his own work qualifies as sociology. The historically naive sociologist is suspended between the conflicting claims of conceptual schemes that he does not understand. As the contemporary vogue of ethnomethodology suggests, he is easily seduced by the cheapest forms of skepticism and relativism or by an equally mindless form of hysterical dogmatism. The ultimate purpose of a history of sociology is *Aufklärung:* emancipation from this radical ignorance by the development of a historically grounded sociological consciousness. Put another way, the basic aim of a history of sociology is to discover the conditions under which sociology as a vocation is possible.

The sociologist, however, must always regard such a history as a perplexing and suspicious undertaking. Consider the history of sociology as a pantheon of gods and the timeless truths they represent, a mausoleum in which names and doctrines acquire the status of idols. History as a theogony or a mythology can be expected to generate a witless and stultifying enthusiasm but not an enlightened sociological consciousness. Consider the history of sociology as *Geistesgeschichte,* the self-contained career of ideas that transcend social reality. History as the quasi-hermetic evolution of ideas reifies the conceptual apparatus of sociology. Detached from the conditions under which their development is possible and intelligible, sociological concepts acquire a spurious and pseudo-metaphysical autonomy that obscures their real foundation.

The present volume proposes to avoid these sources of error by constructing the history of sociology as discourse. In this view, the history of sociological thought is created when the sociologist engages his predecessors in a dialogue that takes the form of an immanent analysis of texts. When the problems, concepts, and themes of these texts are subjected to sociological questioning, the result is sociological discourse. Such a history, therefore, is an artifact produced by the contemporary sociologist. It is a product of his problems, methods, and concepts.

The relationship between a sociological text and social reality is complex. Although there is some sense in which the text is a product of its context it cannot be analyzed as an image or reflection of social reality. This is because sociology, in part, constitutes or defines the context in which it is implicated. In addition, the text transcends its context and addresses an anonymous, transhistorical, hermeneutic community: the sociological profession. It follows that there are two senses in which a sociological text qualifies as discourse. It is a dialogue between the sociologist and his social world. And it is also a dialogue between the text and its interpretation, a dialogue between the author of the text and other sociologists. The texts analyzed in this volume established standards for all subsequent work in the field. Such works are classical in the sense that it is impossible to do sociology without coming to terms with them. The classic demonstrates in a paradigmatic fashion what it means to do sociology.

This is the reasoning with which Dirk Käsler, the editor, defends the conception of the history of sociology to which the volume is committed. In its meticulous attention to the subtleties of primary sources, the quality of its scholarship, and the sophistication of its conceptual apparatus, this book obviously surpasses any comparable volume in the Anglo-American literature. Indeed, it is not clear that a comparable volume in English exists. The essay on Comte by Otwin Massing and the contribution on Marx by Helmut Dahmer and Helmut Fleischer offer careful immanent analyses of the primary texts. Especially valuable is Dahmer's discussion of Marx's ideas on the historical types of class societies and the classes of bourgeois society. Paul Kellermann provides a more superficial account of the prin-

cipal ideas and methods of Spencer's sociology, his contemporary and his influence upon subsequent sociology. Although Piet Tom paper on Pareto is one of the shorter essays in the volume, it is t ambitious piece of scholarship. It includes a sketch of Pareto's int development, a reconstruction of his main sociological ideas, and re on his conception of sociological method. Alfred Bellebaum's paper a the main theses of the sociology of Tönnies and traces their conte and subsequent influence. In his essay on Simmel, Peter-Ernst S provides a critique of the historical response to Simmel's sociolo ploying as his foil the hackneyed conception of Simmel as "the fo formal sociology." He follows this critique, which focuses upon the and the American literature, with his view of a more accurate acc Simmel's basic sociological intentions. Rene König argues that the crisis is Durkheim's central sociological concept. In opposition to t mentaries that stress Durkheim's dogmatism, he emphasizes the o ture and the differential structure of Durkheim's sociological thoug

Each essay provides a careful analysis of all the relevant primary and a discussion of the recent secondary literature. Most useful complete bibliographies of primary sources and the selected bibliog of the secondary literature. The publisher promises a second vol the later classics of sociological thought: Mead, Weber, Scheler, I Geiger, Mannheim, and Schütz.

Sociology and Public Affairs: The Chicago School. By James T. Beverly Hills, Calif.: Sage Publications, 1977. Pp. iv+205. $12.00 ($7.00 (paper).

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It is generally recognized that the department of sociology at the Un of Chicago was, in the years before the First World War and the I sion of the 1930s, the foremost department of sociology in the world Carey here presents the results of his search for an explanation of the flowering of the new science at that particular time and place.

His central thesis (p. 7) is that the explanation must include a "r to social and technological trends which seemed to call for sociolog pertise in the 1920s; the intellectual power of their explanatory s the academic setting which encouraged teaching, research, and pub vice; and the unusual and supportive relationships existing between ogists and other professional groups with whom they had contact this period." He acknowledges, however, that these were "men of i well as flesh and blood, not simply products of the social forces of the nor disembodied intellects."

The above statements are supported by numerous statistical tab

inspection of the published output of the members of the department under study, and by interviews with a number of the generation of graduate students in the 1920s.

The tables indicate some relationships of interest, such as a moderate overrepresentation of urban midwesterners who had somewhat less than the average degree of religiosity, although a number of them came from families in which Protestant Christianity was strong. In general, any reform interest on the part of the Chicago sociologists took the shape of a conviction that an eventual mature and objective science of sociology would eventually be applicable, in ways not then foreseeable, for the good of mankind. Thus impatient reformers either were not attracted to the department or soon withdrew to other fields. Social work, for example, originally within the department, early withdrew to become a department of social service administration, and its relationship to sociology became distant.

Possibly the most effective creative force in the sociology department was the enthusiasm for the sociological enterprise itself on the part of the early faculty and graduate students. There was a stimulating atmosphere, testified to by Leonard S. Cottrell, Ernest R. Mowrer, Edgar T. Thompson, and others (quoted from personal interviews, pp. 154–55). As Thompson puts it, "We talked sociology from morning till night. We interrupted a conversation to go to class, and then came back to continue it after class. . . . We went to coffee, we went to lunch, full of the stuff, the excitement of the place . . ."

Carey recognizes that the atmosphere at large in the new university encouraged this spirit. Founded in 1892, the university took its place in the first rank of universities in its opening year. Albion Small established the first department of sociology in the world and charged it with the task of finding good methods and building a science, not merely a theory, of sociology. Small himself could not do this, but he found men who could—men such as William I. Thomas, Ernest W. Burgess, Robert E. Park, and others. These men pursued a goal, not merely responded to an environment. For the title of their famous text, Park and Burgess chose *Introduction to the Science of Sociology*.

It has been suggested, though not by Carey, that Rockefeller money was the principal factor in the success of the university. But as far as the sociologists were concerned, high salaries were not the attraction, as they did not receive them. Park taught during his first year for no salary at all. Wirth's salary was $1,800 for years. Burgess was offered an attractive salary only in his middle years, when another university made a strong effort to attract him. The administration offered to meet the salary item, then after Burgess agreed to stay, the president asked, "In view of my obligation to conserve the resources of the University, would you accept $1,000 less?" Burgess agreed. Clearly, money was neither their motivation nor the explanation for their success.

The term "Chicago School" has become familiar, and Carey examines its

applicability. If it is meant to refer conveniently only to these men and their style of activity, there could be no objection. But elsewhere the "schools of thought" had authoritarian leaders who formulated doctrines which junior colleagues and students were expected to embrace and defend. Such was the style at Yale in the Sumner-Keller tradition and at Columbia, where Giddings was certain that he had the key to social behavior in his "consciousness of kind" principle. Each of the Chicago scholars, however, set his own course, guided by such methods of discovery as he could find or devise, and specifically renounced any purpose to form a "school."

Professor Carey has performed a valuable service in adding his new data to the literature on early American sociology.

Religion and the American Revolution. Edited by Jerald C. Brauer. Philadelphia: Fortress Press, 1976. Pp. xi+73. $2.95.

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The War for American Independence became a revolution through the fusion of colonial experience with radical ideas made especially relevant to the New World context by the political emergency and social tensions of the years 1774–83. The conflict between American Whigs and the British crown and Parliament demanded a clear statement of the meaning of English institutions as they had been transformed during expansion and affected by the development of an American culture. The three lectures in this book attempt to demonstrate the significance of religion in the cultural context within which the revolution was fought as well as in the historical context of the revolution as that event and myth which defines what we as a nation must become.

Jerald C. Brauer's "Puritanism, Revivalism and the Revolution" is an excellent essay on how revivalistic emphasis on new birth, new man, and new age undermined traditional authority and intensified the impetus for revolution proceeding out of the Puritan ethos. Revivalists' emphasis on the spirit elevated the individual believer's conception of and commitment to communally shared norms and values above static orthodoxy, and demanded in the process that institutions be renewed, indeed, that the whole society be rejuvenated by recapturing the Puritans' view of the holy people covenanted with God and consenting to his will through the conversion experience. Revivalism was a movement not only to revitalize colonial society but also to elevate the status of the ordinary person. The revolutionary implications were clear when revivalists sought to achieve in the acts of converted individuals the vitality of traditional ideology by shucking traditional elites and establishing the holy people as judges and prophets of their own destiny.

Brauer's essay is nicely complemented by Robert Bellah's, which argues

that the memory of the revolution as preserved in our civil religion should include the dynamic tension between what John Winthrop would have called the situation of fallen man and the "truly Christian community where all are one body in mutual love and concern" (p. 67), the tension between "virtue" as the "organizing center of the initial Revolution" and the utilitarian principle of self-interest, so often identified with that event. This tension is necessary is order to prevent the religious elements of the American national experience from erupting in mindless chauvinism. Bellah argues that an understanding of the revolution and a renewed commitment to its great declaration will maintain it. This creative tension, Bellah suggests, could become the source of revitalization for a national faith in our own day. After reading Brauer's essay, one begins to sense how the revivalists' role in making the revolution possible becomes a metaphor for a renewed assault on irresponsible authority.

The middle essay of the trilogy is by Sidney Mead. Partially a rhetorical slap at professional theologians, the lecture tries to explain the interaction among "Christendom, Enlightenment, and the Revolution." Despite his list of definitions meant to say that religion is the "soul" of a nation, or patterns of meaning which express the essence of culture, Mead does not really address the issue of Enlightenment ideas and experiences in the American Revolution. He is so intent upon denouncing sectarianism, denominationalism, and social conflict over the meaning of both religion and revolution that he cannot see—as Bellah and Brauer do—the creative tension of intellectual-religious debate. Somehow Mead has become convinced that the rationalist religion of Franklin and Jefferson is the normative theology for an American religion that transcends all the ruckus created by revivalists. Indeed, the revivalists become Mead's serpent in the Edenic garden of the American Enlightenment. He cannot see the rationalists as one sect among others, preferring them to be the measure of silly, parochial, antiintellectual Evangelicals. The people Brauer finds so important in making the revolution Mead sees as detrimental to the shaping of an American religion. The reason is that Brauer sees historical process moving through group interaction—even conflict—and social change; Mead disdains group conflict as petty and he greatly exaggerates—as he always has—the rancor, pervasiveness, and dysfunctional aspects of theological controversy.

Students of American society will find Mead's refusal to root ideas in identifiable groups and his personal dislike for group conflict as major flaws. They will find much more interesting the essays of Brauer and Bellah, who employ insights of sociologists to pose engaging interpretations of religion and revolution in both past and present. The place of public discourse about values is an inviting intellectual problem for sociologists and historians alike, but it can be studied properly only as we understand the social context of that discourse as well as the meaning of meaning for understanding that context.

The Bolsheviks Come to Power. Vol. 2: *The Revolution of 1917 in Petrograd*. By Alexander Rabinowitch. New York: W. W. Norton & Co., 1976. Pp. xxxiii+393. $14.95.

The Road to Bloody Sunday: Father Gapon and the St. Petersburg Massacre of 1905. By Walter Sablinsky. Princeton, N.J.: Princeton University Press, 1976. Pp. xi+414. $18.50.

Sergei Zubatov and Revolutionary Marxism: The Struggle for the Working Class in Tsarist Russia. By Jeremiah Schneiderman. Ithaca, N.Y.: Cornell University Press, 1976. Pp. 401. $18.50.

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In comparison with the upheavals of 17th-century England and 18th-century France, the revolutions of early 20th-century Russia remain a terra incognita for the student of radical movements and collective violence. Whereas Soviet scholarship depicts the Russian worker as a prime mover in the revolutions of 1905 and 1917, Western literature has, with few exceptions (see W. H. Chamberlin, *The Russian Revolution, 1917–1921* [New York: Macmillan, 1935]), generally ignored the role of the working class, and until recently, very little empirical research has been accomplished in the field of Russian labor history. Insofar as Western scholars have dealt with the subject at all, they have focused either on strikes and demonstrations or on the underground revolutionary parties. Impressionistic surveys of worker protest have predictably revealed the "spontaneous" aspects of labor unrest (see Sidney Harcave, *First Blood: The Revolution of 1905* [London: Macmillan, 1964] and Adam B. Ulam, *The Unfinished Revolution* [New York: Vintage, 1964]) while studies of revolutionary parties show a small contentious elite pursuing its objectives in nearly total detachment from the working class (see Bertram D. Wolfe, *Three Who Made a Revolution* [New York: Dell, 1964] and Leonard Schapiro, *The Communist Party of the Soviet Union* [New York: Vintage, 1971]). All this has added up to a composite picture of Russian workers as an inchoate and anarchic mass driven by elemental rebelliousness (*buntarstvo*) but lacking firm links to the revolutionary parties or other organizations.

This view has been challenged by a new generation of American scholars whose empirical research into various aspects of Russia's social and political history began to appear in the 1960s (e.g., Leopold Haimson, "The Problem of Social Stability in Urban Russia, 1905–1917," *Slavic Review* 23, no. 4 [1964]: 619–42 and 24, no. 1 [1965]: 1–22; Allen K. Wildman, *The Making of a Workers' Revolution: Russian Social Democracy, 1891–1903* [Chicago: University of Chicago Press, 1967]; Alexander Rabinowitz, *Prelude to Revolution: The Petrograd Bolsheviks and the July 1917 Uprising* [Bloomington: Indiana University Press, 1968]; and Reginald E. Zelnik, *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870* [Stanford, Calif.: Stanford University Press, 1971]). Their

studies, including the works by Schneiderman, Sablinsky, and Rabinowitch reviewed here, demonstrate that while Russian workers exhibited a great deal of "spontaneous activity, they also displayed a high degree of political awareness and organizational capability as early as the turn of the century. Moreover, the links between revolutionaries (mainly Social Democrats) and workers were by no means so tenuous as the conventional wisdom would have us believe. Even when party cadres were small in number and opportunities for reaching rank-and-file workers exceedingly limited, revolutionary Marxism still had a way of penetrating the workers' milieu and influencing political attitudes to a greater extent than any competing ideology. The studies by Schneiderman, Sablinsky, and Rabinowitch help us to understand why this was the case. The sociologist whose interests lie in the fields of revolution, social movements, and labor policy will find these works highly valuable even though all three of them tend, in varying degrees, toward a descriptive rather than an analytical approach.

Schneiderman's *Sergei Zubatov and Revolutionary Marxism* and Sablinsky's *The Road to Bloody Sunday* examine the early phases of the labor movement on the eve of the 1905 revolution. Their respective subjects are two phases in the experiment in government-supervised labor associations launched after the turn of the century in an effort to channel worker unrest into peaceful and politically loyal organizations. This brief but significant experiment helped to precipitate the revolution of 1905 and, indirectly, to lay a foundation for the subsequent development of trade unionism. Both studies include material for analyzing the dynamics of cooptive labor policies in a repressive autocratic state and the impact of a cooptive strategy on the political orientation of workers.

In the 1890s, against the background of social upheaval and political radicalism, Sergei Zubatov, chief of the Moscow Okhrana (secret police), devised a program to cope with the formidable problems posed by a nascent labor movement. His was the first—possibly the only—comprehensive strategy for labor developed within the Tsarist autocracy, and Schneiderman has written a definitive account of the genesis and implementation of the Zubatov movement.

Like his contemporary revolutionary opponent Lenin, Zubatov believed that workers possessed an inherently economist outlook, aspiring to little more than improvements in their material condition. Only when their quest for material improvement was thwarted by intransigent employers and unsympathetic government authorities did workers become receptive to the appeals of revolutionary Marxism. Political parties, Zubatov argued, took advantage of the situation to "thrust upon the worker [their] own political views and aspirations, disregarding his psyche, affections and aspirations" (p. 243). Consequently, he advocated a drastic revision of government policy toward labor, abandoning repressive tactics for a new approach that would enable workers to form mutual aid societies under direct police supervision for the purpose of promoting their economic interests. Zubatov believed that this policy would instill confidence in legal methods of struggle and reinforce workers' allegiance to the autocracy, thus creating "social

peace through a just, impartial, supraclass reconciliation and equilibrium of social forces" under autocratic auspices (p. 57).

Between 1900 and 1903, Zubatov societies were established among workers in Moscow, St. Petersburg, Vilna, Minsk, and Odessa. In 1902, Zubatov initiated a young Orthodox priest, Georgii Gapon, into the labor movement, and a year later Gapon launched his own experiment in St. Petersburg with funding from the police and assurances of close coordination with the authorities. Sablinsky's study traces the evolution of the Gapon Assembly and its role in organizing the January 9, 1905, demonstration which unleashed the revolution.

At their height, the Zubatov and Gapon associations attracted into their ranks tens of thousands of workers who took advantage of the first opportunity to organize openly and legally in defense of their economic interests. The studies by Schneiderman and Sablinsky conclusively dispell the image of Russian workers as an undifferentiated and chaotic mass capable only of anarchic rebelliousness. By the turn of the century, many Russian workers were eager to follow their Western counterparts in erecting collective organizations as a shield against the uncertainties of the industrial age.

Equally significant is the fact that both Zubatov and Gapon took as their model the Western European trade union movement (particularly English and German) and actively propagated its virtues among government officials, employers, and the workers themselves. Gapon, for one, advocated that Russia follow the example of Germany "where workers were granted the opportunity to provide for their own development and security 'through their own efforts in cooperation with, and guided by, the government'" (Sablinsky, p. 114). Zubatov, Gapon, and their supporters promoted a cooptive strategy toward labor which, if fully implemented over the long term, might have altered the course of Russian history.

Why, then, did the experiment fail in Russia? The accounts by Schneiderman and Sablinsky make it clear that the Zubatov and Gapon associations encountered persistent opposition from many employers and from influential government circles. Moreover, this opposition was fortified by the progress of the experiment itself, which rapidly revealed that police supervision could not contain workers' activities and demands within an acceptable framework. In the words of a former Zubatovite, "Recalling now the speeches of Krasivskii [a Zubatov labor leader] about the position of workers in Western Europe, about the organization of trade unions and so on, it must be recognized that these speeches . . . had quite an impact on our young and feverish brains, and in our consciousness was emerging an awareness of the necessity for struggle to improve our condition" (*Professional 'noe dvizhenie Moskovskikh pishchevikov* [Moscow, 1927], 1:184). Once the Zubatov and Gapon associations gained a following and momentum of their own, workers progressed to increasingly militant forms of struggle, such as prolonged strikes, and finally they mounted a massive demonstration at the Winter Palace which marked the beginning of the 1905 revolution and the end of the experiment in labor cooptation.

Describing the impact of "Bloody Sunday" on St. Petersburg workers,

Sablinsky writes: "What generations of revolutionaries had not been able to achieve in years of propaganda and agitation was brought home to the working mass in a few hours by the bullets of His Majesty's élite troops. The working class of St. Petersburg was revolutionized overnight . . ." (p. 271). While this change in political attitude may not have been as widespread or precipitous as Sablinsky implies, nevertheless there was a pattern of radicalization in the capital city and elsewhere from January 9 onward which bears close resemblance to Zubatov's predications concerning the labor movement. Revolutionary Marxism gained many adherents in the months following "Bloody Sunday," and by the end of 1905, the Soviets and most trade unions were under the influence of Social Democrats.

The experiment in police-supervised labor organizations did not achieve peaceful integration of workers into the autocratic system, as its promoters had envisioned. Unlike Germany, where repeal of the antisocialist law in 1890 allowed mass membership unions under Social Democratic auspices to enjoy a legal existence, the Tsarist autocracy remained wedded to a policy of repression rather than a strategy of integration. The Zubatov and Gapon experiment served to awaken in workers an aspiration for truly independent labor organizations, but the autocratic system, so deeply and traditionally rooted in mistrust for public initiative and autonomous activity, could not tolerate even a government-supervised organization of workers—not to speak of an independent labor movement with Social Democratic leadership. Whereas in Germany, government policy toward labor produced what Guenther Roth, in his book *The Social Democrats in Imperial Germany* (Totowa, N.J.: Bedminster, 1963), described as "negative integration" into society (cultural and economic inclusion but social and political exclusion), in Russia not even the "negative integration" of workers proved possible under the Tsarist autocracy—a circumstance which contributed to the explosiveness of Russian labor in 1905 and 1917.

Rabinowitch's book, *The Bolsheviks Come to Power,* depicts the long-term consequences of earlier failures to achieve a peaceful resolution of Russia's labor problems. In this second of two volumes (the earlier volume appeared in 1968), he examines the leftward thrust of the revolution in Petrograd between July and October, 1917. Rabinowitch's major contribution in this study (and in his earlier one as well) is to document with greater thoroughness than ever before the connections between the urban lower classes in the capital city and the Bolshevik party. He disposes once and for all of the conventional view portraying the October Revolution as a coup d'état by an isolated and monolithic band of fanatical revolutionaries ruthlessly bent on seizing power. Carrying forward the approach of W. H. Chamberlin, Rabinowitch pays close attention to the role of social forces in the interrevolutionary period.

The Bolsheviks Come to Power documents the rapid growth in popular support enjoyed by Lenin's party on the eve of October. Although the Bolsheviks were shrewd propagandists and organizers, they could not have succeeded without a reservoir of popular sentiment among workers and soldiers favoring radical solutions.

Rabinowitch offers a number of explanations for the increasingly pro-Bolshevik orientation of Petrograd workers and soldiers on the eve of October, such as the ineptness of the Provisional Government and the left parties (Mensheviks and Socialist Revolutionaries) and the growing threat of counterrevolution. Although these factors are well known, Rabinowitch chronicles the events with exceptional precision and extensive documentation drawn from primary sources.

Nevertheless, many questions concerning the political attitudes of the lower classes remain unanswered in this study. One wants to know, for example, how workers actually perceived the Bolshevik platform and political objectives when they voted for Bolshevik candidates and finally acclaimed the October seizure of power. Unless we can recreate, at least in part, the so-called view from below, the intricate mechanism of the Russian revolution will continue to be uncomfortably obscure. What we need are investigations of grassroots activity in factory committees, trade unions, and other organizations; studies of workers' preferences and outlook as manifested in voting patterns, resolutions, monetary contributions, and the like; and finally, research into economic trends and living and working conditions. Some of this work is already under way by students of history and sociology (see, e.g., Robert James Devlin, "Petrograd Workers and Workers' Factory Committees in 1917: An Aspect of the Social History of the Russian Revolution" [Ph.D. diss., SUNY, Binghamton, 1976]; Diane Koenker, "Moscow Workers in 1917" [Ph.D. diss., University of Michigan, 1976]; and David Mandel, "Petrograd Workers in 1917" [Ph.D. diss., Columbia University, 1977]), whose contributions to the study of social movements in revolutionary Russia will no doubt necessitate further revision of many older conceptions.

Darwin in America: The Intellectual Response, 1865–1912. By Cynthia Eagle Russett. San Francisco, Calif.: W. H. Freeman & Co., 1976. Pp. ix+228. $4.95.

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In recent years we have witnessed considerable resistance to the idea that our altruistic behavior and our IQ and school performance are determined by our genetic constitutions. These are only the most recent additions to the biological understanding of human society initiated by evolutionary thought. If recent consternation associated with these views has been significant, the initial shock wave following Darwin's announcement was all the more unsettling. Russett details some of those initial reactions to Darwinism in various fields of American social and political thought.

In philosophy, evolutionary ideas received a lively interpretation by members of the "metaphysical club" at Cambridge, Massachusetts: John Fiske, Chauncey Wright, C. S. Peirce, and William James. Thanks to the efforts of

Fiske, evolution was presented to Americans chiefly from the *Synthetic Philosophy* of Spencer, whose popularity soon outstripped that of Darwin. In Spencer's view, evolution was none other than the biological expression of the 19th-century idea of social progress. This was reflected in Fiske's later *Cosmic Philosophy*—"a new form of natural theology based on evolution" (p. 54). Sociologists will be struck by the parallels to the career of Comte, whose scientific positivism, like Fiske's, ultimately gave way to a utopian synthesis of science and religion.

Evolutionary theory, as represented by Herbert Spencer, gave rise to a body of conservative thought whose central tenet was that the stratification of society was arrived at by the same laws which operate in nature. We might speak here not of Social Darwinism but of Social Spencerism. The Spencerians opposed the "artificial" interventions of the state into these "natural" events. Organized charity, the reformist protection of the poor, and the state's maintenance of the unhealthy and the insane were scorned. Sumner remarked that the function of the state should be limited to the protection of "the property of men and the rights of women" (p. 99). Also, whereas Lamarckian evolution lent support to the ideals of education, Social Spencerism did not. Noted Sumner, "Education is the superstition of the age." Modern variants of this position have reappeared recently with the Banfield and the Jensen arguments regarding the inefficacy of inner-city Head Start programs and the hereditary limitations of IQ.

Russett also traces the counterarguments of Lester Frank Ward regarding the naturalistic interpretation of society. Nature never checks population at the source by diminishing fertility, but rather sacrifices countless neonates to ensure the survival of a single individual—hardly an ideal model for society. Also, artificial intervention in agriculture has produced breeds superior to those found in nature. Both items challenge the inherent superiority of the biological model.

The argument took more positive shape with the contributions of James Mark Baldwin and C. H. Cooley. Whereas the Spencerian account of society completely overlooked the naturally occurring sense of ethics and altruism in society, Baldwin concluded, "Morality has arisen because it is socially useful; this is the Darwinian account" (p. 117). Hence this early theory of normative action which anticipated social psychology was weaned expressly on Darwin. Also, drawing on Hegel, Baldwin argued that the individual was not an isolated Hobbesian unit fighting its own struggle for existence, but that the individual was the outcome of a dialectical process in which the self co-emerged with the individual's identification with others. This notion was refined in Cooley's theory of the reference group and the "looking glass self." It is highly ironic that the ground for Mead's social psychology, which followed the tradition of Baldwin, Cooley, and Thomas, was laid in part on a foundation of "hegelian darwinism"—which at least in its inception would not have been inconsistent with sociobiology.

Russett's book is well written and supplemented with period photographs and illustrations. However, it raises more problems than it solves.

Russett's overall contention is that Darwin's ideas initiated a new ethos

in American social and political thought after the Civil War and that this ethos passed with the emergence of modern physics circa 1912. Sounding vaguely like Leslie White, Russett suggests that "America, in the years after the Civil War, was clearly ripe for innovative thought" (p. 215). If Darwinism was an idea "whose time had come," then this surely means no more than that, the war being over, people could once again turn their attention to the pursuit of knowledge, commerce, and family life. Russett suggests, however, that Darwinism represented a basic structural shift in American thought, a shift which was later to be superseded by another great discovery: relativity. Certainly there was a shift "away from supernaturalism, ultimate reality, and final causes," but these were more the effects of Victorian science than conditions for its reception.

In spite of the fact that numerous naturalistic writers like London, Norris, and Dreiser and theorists like Dewey, Veblen, and Cooley continued to be popular well after 1912, Russett dates the end of the age of Darwin with the advance of physics. "Following closely upon the heels of the Darwinian revolution, the revolution in physics cut across the influence of Darwinism in a rather fundamental way" (p. 207). The hallmark of the new ethos was "modern uncertainty" (pp. 19–22) and the "demise of certitude" (pp. 203–7) associated with Heisenberg's uncertainty principle and Einstein's relativity.

Though such theories reorganized modern physics, they did not diminish or displace the influence of Darwin. In our times genetic theories continue to plague economic and educational reform (see Banfield and Jensen). Pious taxpayers continue to lobby for "equal time" instruction in special creation at state-supported colleges. And such ethological works as Wilson's *Sociobiology* seek to subsume all social and psychological thought under an evolutionary zoology. There is no comparable effect on social and political thought from physics. We've learned to stop worrying and love the bomb.

Finally, it is not clear that the ideas that gained prevalence in America were really Darwinian at all. Very few quarters were concerned with the theory of speciation per se. As Russett notes, the naturalistic writers took as much from their common knowledge of urban conditions as from the evolutionary catchwords. It is of note in this regard that the terms "struggle for existence" and "survival of the fittest" were garnered by Darwin from the descriptions of human populations completed by Spencer and Malthus prior to *The Origin of Species*. Hence, the original referents of these terms were conditions which social critics knew first hand. In this sense the reorientation of social thought might be seen to derive primarily from the critique of robber-baron society in the fortuitous language of evolution.

On the same note, it is instructive to contrast Darwin's view of the competition for survival *within* species with sociobiology's emphasis on intraspecific altruism. The model for the former is surely the economic struggle of 19th-century society. On the other hand, only time will tell whether altruism in sociobiology is liberal pragmatism generalized to the zoological world or whether altruism based on genes is liberalism "gone to seed."

Victorian Murderesses. By Mary S. Hartman. New York: Schocken Books, 1977. Pp. x+318. $15.00.

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The Victorian era was the first period of great mobilization for western women. Victorian morality idealized female sexuality as a commodity with which women maximized their ability to exercise some power over marriage decisions and family life. The Victorian ideal of sexual loyalty of both husbands and wives existed alongside persistent legal and cultural double standards. Conflicts along these contradictory lines are illustrated in *Victorian Murderesses*.

Mary Hartman's book is concerned with all that its title implies: violence, Victorian asceticism, and the contradictions of a culture that idealized "pure" female sexuality. But *Victorian Murderesses* is not so much a record of specific female criminals as a rich documentation of some nuances of Victorian life. The evidence recorded in the investigations and trials of 13 English and French women is used to reassemble pieces of a still-puzzling aspect of Victorian social history. Murder is not so much the central issue of the book as a fortuitous source of accounts of the lives of a few select, yet not atypical, women.

Hartman's method is not an unfamiliar one; deviant cases have provided social scientists with samples, though extreme ones, of pervasive cultural trends. Like many others, Hartman is interested in the vicissitudes of 19th-century middle-class culture, and she has tapped a rich source in the legal and public deliberations over these "murderesses."

The circumstances surrounding these criminal cases give us glimpses into the lives of a few members of the growing middle class. The subjects are a diverse variety of women, ranging from a child-beating governess who had failed in a competitive marriage market to respectably married ladies. Hartman's contention is that the tensions visible in these women's lives are to be understood as social tensions which pervaded Victorian society. She argues that the women in this sample are extreme only in that they have been accused of murder—not in the kinds of predicaments they faced. With detailed descriptions of these personal dilemmas, Hartman makes an important contribution to a still small body of literature on the social history of women. More broadly, the book provides us with glimpses into Victorian "everyday life"; that is, the historical accounts illustrate the existence of popular images of men and women alongside the realities of people's lives.

Hartman shows how conflicting ideologies about middle-class womanhood came glaringly into view during these highly publicized criminal cases. In some cases public sentiment favored the defendant at first, but was reversed when evidence of adultery was presented. Indeed, occasionally it appears that adulterous wives were seen as more evil than a betrayed wife who had poisoned her husband. Hartman suggests that in a few cases the charge of adultery influenced a jury's verdict of "guilty" more than the actual evi-

dence of murder. These deliberations occurred during a period when sentiment against adulterous hubands and arranged marriages was increasing and women found themselves in a position where they could demand romantic ties to the men with whom they spent their lives. Most important, then this book offers a social history of shifting opinions about what kinds o roles, both public and private, were acceptable and proper for middle-class women.

But good social history does not always make good sociology. *Victorian Murderesses* offers evidence which supports theories of sexual stratification in Victorian times, but it does not systematically delineate its implications Most of the theoretical discussion is aimed at drawing connections between individual historical examples and general social conditions. For example "In the Reymond affair Sassimone's evident restraint had a purely personal aspect in his . . . obligation to his wife's family, but it also reflected the fact that the new bureaucratic structures to which middle-class men belonged placed a premium on the prevention of public scandal . . . rather than on the preservation of honor in the older sense" (pp. 245–46).

Hartman does make an attempt to contrast the crimes of passion committed by English and French women. She suggests that "the English woman's greater exposure to educational, legal, and social ideals" allowed "aggression to be channeled into political rather than personal violence" (pp. 244–45). Crimes of personal violence, she argues, occurred more often in France; in England "the militant, even violent suffragist activities can be seen as a counterpoint to the French crime of passion" (p. 245). Unfortunately Hartman relies on the concept of "relative deprivation" to explain the French wave of female crimes of passion. Conditions of middle-class women in both England and France had improved, but Englishwomen had legitimate ways to vent their frustration and Frenchwomen did not, or so the theory goes. A less psychologically reductionist and more accurate explanation might suggest that English middle-class women had more material and cultural resources with which to challenge double standards, both in the home and in the courtroom, than did their French counterparts. To explain the deviant behavior of French women by relative deprivation is to overemphasize the emotion of disappointment and underestimate structural factors, neglecting the importance of the different resources—both economic and ideological—available to French and English women.

Although Hartman's attempt at comparative theory is the book's weakest point, sociologists who study Victorian culture in particular, and the position of women in society in general, cannot afford to pass this book by. In spite of its theoretical weakness, its rich illustrations of conflicting ideologies operating side by side during a period of economic and cultural transition in the lives of middle-class women are not to be missed.

The investigations and deliberations over these Victorian murderesses clearly illustrate the sexual bargaining operating both in the press and in the bedroom. What was a reasonable demand for women to make in ex-

change for becoming their husband's legal sexual property? This question, of course, is central to all studies of sexual stratification. Although Mary Hartman's book does not provide the links to larger analytic issues, it furnishes us with detailed historical evidence of Victorian solutions, upon which we can build more comprehensive theoretical discussion.

Women, Crime and Criminology: A Feminist Critique. By Carol Smart. London: Routledge & Kegan Paul, 1976. Pp. xv+196. $10.50.

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Upon paging through Carol Smart's book prior to a thorough reading, any criminologist or student of deviance and social control who has even fleetingly considered the issue of the distribution of criminal offenses by sex would have his/her curiosity piqued. The work is clearly an attempt to add to the slowly growing literature on the female offender by offering a feminist critique of classical and contemporary theories of the nature and etiology of deviance among females, exploring the differential treatment of males and females by the criminal justice system, delving into the oft-made argument that mental illness is a functional equivalent or alternative form of behavior to criminality for women, and proposing ways in which new conceptual frameworks of a nonsexist sort might be developed within which the deviant activities of both men and women might be more fruitfully and accurately analyzed. In addition, although the empirical focus of the book is the deviance of women on the author's own turf, the United Kingdom, she draws frequently on data from the United States. This lends the work a certain comparative perspective which would appear to be a definite strength.

Smart's attempt at comparative analysis is especially evident in the first chapter, which deals with the nature of female criminality. A quick perusal of this chapter is guaranteed to excite even the criminologist who has never felt the deviance of women worthy of either empirical investigation or theoretical consideration. Here the author emphasizes the necessity of knowing which crimes women engage in and to what extent. She states: "As the assumptions and opinions inherent in studies of female criminality are generally dependent upon, or derived from, some kind of statistical evidence, usually extracted from official statistics, the statistical picture of female crime acquires additional significance in any analysis of the theories and practice of female criminality" (p. 5). In an attempt to paint such a "statistical picture," Smart presents and explicates several tables which describe aspects of the criminal behavior of men and women in the United States and the United Kingdom. It is at this point that the casual peruser finds cause for excitement. The author appears to have taken her data from the usual sources, the Federal Bureau of Investigation's *Uniform Crime Re-*

ports (*UCR*) and the *Home Office Criminal Statistics for England and Wales*. Yet a number of the tables present and discuss comparatively a category that Smart indicates represents the "number of persons found guilty." Criminologists and students of deviance in the United States have for years decried the fact that national data of this sort are unavailable. Has Smart found a way to cull from the *UCR*, the only source of national data on crime in the United States, estimates of conviction rates? The *UCR* are, after all, only rough counts of crimes known to the police. They are, in short, arrest statistics. Smart has not only not found a way of estimating conviction rates from arrest rates, she has not even bothered to look at the *UCR*. Instead she uses the data extracted from that source by Dale Hoffman-Bustamonte ("The Nature of Female Criminality," *Issues in Criminology* 8 [Fall 1973]: 117–32). She does so even though Hoffman-Bustamonte repeatedly mentions that her discussion of the nature of female criminality is based upon "female arrest rates" (e.g., p. 118). The effect of this error on the generalizations Smart makes may in fact be minimal. Even so, the data from the United States and the United Kingdom are as a result not comparable without serious reservation. More important, the error seems inexcusable in the context of the author's statement about the importance of such statistics in this area.

Upon a closer reading, the remainder of the work is perhaps less disappointing, but it is disappointing nevertheless. There are valuable elements. For example, Smart's criticisms of the sexist bias of the classical analyses of deviance among women—*The Female Offender* by Cesare Lombroso and William Ferraro (London: T. F. Unwin, 1895), *The Unadjusted Girl* by W. I. Thomas (Boston: Little, Brown & Co., 1923), and *The Criminality of Women* by Otto Pollak (Philadelphia: University of Pennsylvania Press, 1950)—are well taken. Her argument that criminological theorizing about the deviance of men has become ever more sophisticated, progressing from early positivistic theorems to the New Criminology, whereas much of the theorizing about the deviance of women is still of a positivistic and deterministic sort, is also most convincing. Similarly, the author's critical appraisals of John Cowie, Valerie Cowie, and Eliot Slater's *Delinquency in Girls* (London: Heinemann, 1968) and Gisela Konopka's *The Adolescent Girl in Conflict* (Englewood Cliffs, N.J.: Prentice-Hall, 1966) are careful and insightful. However, to present these works as "representative of studies of female criminality carried out in recent decades" (p. 54) is perhaps to overstate the sexist bias of contemporary works. Although Smart's book was not available until late 1976, she never mentions three more balanced treatments published in 1975: Freda Adler's *Sisters in Crime* (New York: McGraw-Hill), Rita James Simon's *Women and Crime* (Lexington, Mass.: D. C. Heath), and Marcia Millman's "She Did It All for Love" (pp. 251–79 in *Another Voice*, edited by Marcia Millman and Rosabeth Moss Kanter [Garden City, N.Y.: Anchor]).

Smart's discussion of the differential treatment of men and women by the criminal justice system focuses on the processing of offenders in the United Kingdom. Her analysis is solid and valuable for those interested in the American case as well, even though there are some significant differences. She appears relatively unfamiliar with the work done in this area in the United States, however. Leo Kanowitz's *Women and the Law* (Albuquerque: University of New Mexico Press, 1969), for instance, could have provided a nice source of comparative data. The emphasis throughout the book, but in this section particularly, is on the tendency of criminological and commonsense theories of female criminality to stress the inherent biological predispositions and/or psychological instabilities of women as explanatory variables rather than as variables of a structural sort which would reflect the relative powerlessness and sexual subordination of women as a group. These theories—and among them Smart includes those that see mental illness as a functional equivalent to criminal behavior for women—then form the basis for public policy, especially in the area of the treatment of women by agents of social control. In response to this situation, she calls for an end to the "sexualizing" of crimes committed by women and an undermining of the assumptions that sexual stratification is the natural order of things and that criminal acts committed by women are somehow more irrational or emotionally motivated than those committed by men.

Last, Smart touches upon the problem of redressing the balance. Although she leaves the reader without a clue as to what a "feminist criminology" might look like, she is able to suggest what one would not look like and what the prerequisities for one would be. It would not be a completely distinct area of study. If it were, it would risk segregation from the mainstream of the social sciences and possible classification as "women's work," with the low status and lack of support that that category usually receives. Smart describes what she has in mind in this way:

> Ultimately a non-separatist social science must be the desired goal because women and men do not act separately in the social world, they are not independent of each other and their inter-relationship is a vital fact of life. But before this goal can be achieved changes in ideology and social practices must take place, and woman must no longer be treated as the "inessential"; the negative to man's positive. In order to achieve such a transformation basic changes in consciousness are necessary and it is here, through consciousness raising that separate studies of women and the concerns of women by feminists, are vital. The aim must be not only to make visible the invisible, to restore women in their own right to social science, but to find alternative modes of conceptualizing the social world so that the interests and concerns of women are addressed and included rather than subsumed or ignored. [Pp. 179–80]

Methinks this has not been so well argued before, and even if it were the only valuable insight contained in *Women, Crime and Criminology: A Feminist Perspective*—and for all the book's shortcomings it is not—it would give the work a certain merit that could not be denied.

Seven American Utopias: The Architecture of Communitarian Socialism, 1790–1975. By Delores Hayden. Cambridge, Mass.: M.I.T. Press, 1976. Pp. 401. $16.95.

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Seven American Utopias has the potential for being an important book. The author, Delores Hayden, sets out to describe seven socialist utopian communities in which architectural design played an important part in the distinctive ideology of each group. The historical development of the Shaker movement, the Mormons, the Fourierist North American Phalanx, the Oneida Colonists, the Amana Community, the Union Colony of Greeley, Colorado, and Alice Constance Austin's Llano del Rio Colony is presented in detail. The characteristic architectural styles of each are discussed and related to the social organization of the communities.

This strategy for exploring the relationships between community and physical design has the potential for avoiding pitfalls encountered by other social ecological studies. There have been enough kinds of ideal communities and a large enough number of towns inspired by utopian ideals with architectural implications so that propositions about the interaction of space and social relations could be systematically explored. This was one of the things Hayden had in mind in writing the book. She outlines a number of issues around which such a project could be organized. Unfortunately, from the standpoint of the sociological reader, the author also tries to achieve several other goals which compete with the sociological ones, diluting the result.

Hayden is a landscape architect with a special interest in the modern commune movement. One of her primary concerns is to explain why modern communes might fail in achieving their architectural ambitions. To do so, she has chosen to look at how successful some of the most famous of American utopian communities were in achieving their ends. She is not content, however, to explore parallels between the modern and the 19th-century experiences of the cases she describes. She wishes to provide accurate histories of these important communes and of their contributions to architecture in general. Important as it is to be accurate in any historical presentation, one suspects that the choice of these seven collectives out of the hundreds listed in an appendix was motivated by the author's desire to produce a document which not only was historically accurate but also would cover important topics in architectural history, whether or not this treatment was consistent with the sociological goals of the research.

Hayden deals with the problems of theoretical unification this creates by trying to show that her cases share three common dilemmas: (1) the balance between authority and participation in design, (2) allocation of property to the communal or to the private domain, and (3) balancing of unique and replicable aspects of plans for communities. While each of these dilemmas presents real problems faced by members of the communities discussed,

they fail to work as unifying focuses of the book. This is partly because in her eclecticism the author does not really focus her case discussions on the dilemmas. More important, it is because the dilemmas do not emerge organically as consequences of issues the author finds central to each case presentation.

More than anything else, this is a book about architecture. It treats the way spatial arrangements may be manipulated to reinforce certain aspects of social life. Yet the thread is lost again and again as interesting side issues —the feminist aspects of each community, for example—are explored. In some of the case studies, particularly those of the Union Colony and the Llano del Rio Colony, architecture scarcely plays a role. The three dilemmas are discussed with respect to each, but no new problems are introduced to justify the inclusion of these cases. We learn little from them. This is in contrast to the other discussions, in which we find unique ways of dealing with the problems of socialist living, more architectural character, and important problems.

Some of these latter case discussions are fascinating and thematic. The analysis of Shaker life is best. Hayden describes a meticulousness, a degree of discipline, and a concern with geometric regularity in this group which I find amazing. Why would people feel obliged to go along with such a constraining regime? The answer seems to be that radical limitation of freedoms in routine daily life heightens the possibility for ecstatic religious experience. The orderliness of normal, everyday life is juxtaposed to singing, dancing, and wild fantasies in religious services. This phenomenon might have quite general applications to understanding religious organization. The interdependence of these opposite sorts of conduct are sensitively described and explained.

The Shaker case shows the potential for the approach the author has chosen here. However, what seems like the central explanatory problem concerning Shaker life has little to do with the central dilemmas Hayden uses as an organizing focus. Because she fails to focus on equally stimulating social organizational problems arising from architectural norms in other case discussions, the sense of excitement is lost. The discussion meanders and the argument dissipates.

Economic Integration in New Communities: An Evaluation of Factors Affecting Policies and Implementation. By Helene V. Smookler. Cambridge, Mass.: Ballinger Publishing Co., 1976. Pp. xxxiii+236. $15.50.

Residential Mobility in New Communities: An Analysis of Recent In-Movers and Prospective Out-Movers. By Edward J. Kaiser. Cambridge, Mass.: Ballinger Publishing Co., 1976. Pp. xxxiii+188. $15.00.

Recreation and Leisure in New Communities. By Raymond J. Burby III. Cambridge, Mass.: Ballinger Publishing Co., 1976. Pp. xxxii+366. $17.50.

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During the 1960s a number of "planned communities" were undertaken by private developers in the United States. According to their advocates these communities were distinguished from suburbs by planning, a range of services and amenities, self-sufficiency, and social balance. Enthusiasm for the potential accomplishments of planned communities led to the passage of the Urban Growth and New Community Development Act of 1970, which provided federal loan guarantees and other forms of assistance for approved new community projects. In 1972 the first nationwide study of new community development was initiated, under the direction of Shirley F. Weiss at the Center for Urban and Regional Studies of the University of North Carolina at Chapel Hill, in an attempt to assess new community performance. *Economic Integration in New Communities*, *Residential Mobility in New Communities*, and *Recreation and Leisure in New Communities* are research monographs deriving from the North Carolina new community study.

In terms of the policy implications of new communities, Helene V. Smookler's *Economic Integration in New Communities* is the most important of the three monographs. The 1970 Urban Growth and New Community Development Act justified federal support of planned communities on the grounds that they could open up the suburbs to lower-income and minority residents. Smookler seeks to determine the extent of economic integration in new communities as measured by (1) the proportion of subsidized housing and (2) the demographic traits of residents.

In a sample of 15 new communities (13 of them privately developed and two federally assisted), Smookler found that seven contained subsidized housing and thus were characterized by some economic mix. For the seven communities that were "economically integrated" the subsidized housing population accounted on the average for about 9% of the new community population. However, this average is misleading given the fact that 36% of the housing in Jonathan, Minnesota, is subsidized, a far higher percentage than for the other new communities. As a result, the meaning of "economic integration" in Smookler's study becomes unclear. Is the extent and nature (macro vs. micro level) of economic mix a factor in integration, or is any amount of subsidized housing to be considered indicative of social balance?

As for the second measure of economic integration—the demographic traits of new community residents—Smookler found that most of the communities that had subsidized housing also had some nonwhite residents. On the other hand, since almost all the new communities in the sample contained nonwhites, racial integration did not necessarily occur as a result of the provision of subsidized housing. However, the developer's public commitment to integration did facilitate the inclusion of subsidized housing and the accessibility of new towns to minorities.

For Smookler the developer's marketing strategy becomes *the* crucial factor in achieving economic integration. Developers who planned for socioeconomic integration early in the development process and made those plans public (in advertising brochures, for example) achieved the highest levels of economic and racial integration as well as the most support for that policy among residents surveyed. Moreover, developers were able to do this without creating a negative impact on the new community housing market. Smookler thus defends new communities as a feasible and desirable method of integrating suburban areas. She concludes that "social objectives can be pursued without damaging the housing market and with positive social impact" (p. 125).

Edward Kaiser's book, *Residential Mobility in New Communities*, examines the traits of new community migrants versus conventional community migrants as well as the reasons for mobility. Kaiser finds that new community "in-movers" are not representative of the mobile population as a whole. Instead, they are more likely to be upper-middle-income whites. Nor are those blacks who do move to new communities representative of the black metropolitan population. New community blacks in general had higher educational, occupational, and income levels than the black metropolitan population as a whole.

As for their reasons for moving to new communities, in-movers gave answers similar to those of conventional community in-movers. Housing and its traits were of greater concern than community attributes for both groups of in-movers—a surprising finding given the emphasis placed on community amenities by new community developers. Black new community residents, however, did mention the importance of neighborhood and community facilities more often than whites. Kaiser explains this racial difference by noting that blacks are more residentially stable than other elements of the new community population and therefore have an important stake in the community and its institutions.

Prospective job change was the most common reason for planning a move in both new and conventional communities. Consequently, the ability of new community developers to influence mobility is sharply limited. However, according to Kaiser the market appeal of new communities to potential in-movers can be strengthened by the development of outstanding shopping, educational, and recreational services.

How well new communities have done in the last-mentioned area, recreation, is the subject of the third monograph, by Raymond J. Burby III. Burby's *Recreation and Leisure in New Communities* evaluates the experiences of 15 new communities in developing recreational service systems. It provides information not only about the extent and variety of recreational resources and user satisfaction but also about the particular traits of the political and institutional context which influenced the systems.

Recreational facilities and open space were, on the average, more plentiful and more accessible in new communities than in conventional communities. Participation rates in various recreational activities, however, were only slightly higher than in conventional communities. Moreover, new commu-

nity recreational systems performed best for whites in late middle age who were affluent and whose children had left home. Young adults, elderly persons, and blacks were less likely to evaluate recreational systems positively. The most dissatisfied group in new communities was young adults; they expressed a desire for more cultural, entertainment, and shopping facilities in their communities. In part, they were expressing a preference for the qualities of life associated with larger, more heterogeneous urban communities. To some extent, however, this complaint resulted from the narrow conception of "recreation" embedded in new community recreational planning. Nonathletic forms of leisure, for example, received much less attention from developers than did indoor activities designed to facilitate sociability.

Taken together, these three monographs offer many previously unavailable data concerning new communities in the United States. Each book is also concerned with using its analysis to provide new community developers and policymakers with information about how to meet their goals more effectively. To some extent the nature of their mission leads the authors to be more concerned with generating data than with analyzing and critically assessing their implications.

Smookler, for example, concludes that social and profit-oriented goals can be achieved simultaneously in new communities—a conclusion somewhat more sanguine than her data suggest. Her positive assessment of the new community's potential to achieve economic integration rests on the assumption that middle-income and upper-middle-income buyers and renters will be willing to live in economically integrated communities, and she does in fact find that residents of economically integrated communities are positively disposed toward economic integration. Less easy to determine, however, is whether those residents who favor economic integration consciously chose an integrated community or whether they in fact underwent a conversion process once in the community. If the former is the case, Smookler's optimism concerning the new community's ability to integrate the suburbs appears somewhat overstated. While Smookler leans toward the latter explanation, as a result of finding that individuals living closer to subsidized housing were more favorable to it, she does not supply enough evidence to offer it as more than a hypothesis.

Despite this criticism, the three monographs can be useful to social scientists interested in urban policy and community as well as to developers and planners. The data they provide offer partial answers to the two major questions posed by new communities: (1) Are new communities substantially different from conventional communities, and (2) are new communities fulfilling the criteria that led to federal interest in and funding for new community development? Burby and Smookler would answer both questions positively. Kaiser's analysis, in contrast, seems to indicate that whatever the objective traits of new communities as opposed to conventional ones may be, they are not (or at least, not yet) important factors in determining why individuals move in and out of new communities. Kaiser notes that new communities are not perceived as qualitatively different from—

only perhaps marginally better than—conventional communities. For most of their residents new communities are not "the next America."

Inequality in American Communities. By Richard F. Curtis and Elton F. Jackson. New York: Academic Press, 1977. Pp. xii+354. $19.50.

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This monograph is not a study of how and why communities differ in their distribution of goods, influence, and social honor. Rather, it focuses on how rank characteristics (education, occupation, income, race, parental SES, migration) of male heads of families in three communities (a metropolis, a small city, and a small town) in each of two states (Indiana and Arizona) affect their perceptions of the class structure, their class self-placement, their views of national public issues, and their perception of the legitimacy of success. The authors ask whether the rank characteristics of individuals have an additive and linear relationship on stratification-linked phenomena such as satisfaction with life, informal and formal participation, political ideology, party identification, anomia, and intolerance. They also ask whether community characteristics (size and region) and individual characteristics (length of residence, rural or urban origin, occupation, race, education, income, etc.) affect the same phenomena. In short, this research follows the well-known path of the attainment model in the study of achievement and related stratification phenomena in six communities.

Curtis and Jackson present their research design succinctly in an excellent chapter on models of rank effects. They find that rank effects combine additively to explain individual attitudes and behavior, correspond to a linear model, fit a multidimensional model of stratification better than a unidimensional model, and vary minimally by community. Rank effects of education and occupation are more important than those of other variables which may operate for some communities and for some dependent variables. The evidence presented for these findings is convincing.

Not surprisingly the substantive findings of the research support generalizations associated with attainment model studies. Curtis and Jackson conclude that the stratification system is national in scope and that community systems, like the national one, are not rigid because rates of social mobility and status inconsistency are universally high. Education is crucial in explaining people's place in the ranking system and, apart from race, ethnic status has little effect. Expectedly, the class self-placements of men reflect their present status and not their status background. Moreover, most men share a consensual view of domestic politics and a belief that most men (apparently none of the questions referred to women) earn their success legitimately. Moreover, the successful and the unsuccessful do not differ sharply in their life satisfactions. High-status men visit friends and work

associates more than low-status men. High rank also encourages men's organizational and church membership which, in turn, promotes their political activities. Since the several rank dimensions independently affect political liberalism and party identification, stratal polarization in the community is limited; further, variance in ideology and class identification is not affected by social rank. While high rank but not social background reduce anomia, mediating variables such as social isolation do not. Finally, increasing education serves to reduce all forms of intolerance.

Most of the social rank characteristics were slightly or moderately related to the stratification phenomena studied; and intervening variables added little to the explanatory power of social rank. But very few community characteristics were significantly and systematically related to the stratification variables. In short, most of the statistical results of the research conform to the image of community and national stratification arrangements as being somewhat mushy and unstructured.

This monograph will please or irritate sociologists depending on their views of the proper study of stratification. However, few will deny that Curtis and Jackson have written a superb monograph within that paradigm which equates the distribution of rank characteristics of individuals with social stratification. The authors present alternative hypotheses lucidly; they propose fair tests of them; they bring the appropriate literature to bear on their problems; they apply the appropriate statistical procedures knowledgeably; they interpret their results in a balanced manner; and most important, they relate their findings to important theoretical problems. Yet even within their framework, two important limitations should be noted. First, the exclusion of women heads of family (24% of all heads) in the samples is lamentable because the views of lower-status people get slighted. Second, assigning mean scores to the 20%–34% of the respondents who did not answer various questions may emphasize the consensual view of the stratification system held by middle-status people.

Sociologists of the structural persuasion who believe that communities have distinct features which affect stratification will conclude that this study missed the boat. The 10 pages devoted to community descriptions mostly portray the attributes of the individuals in the samples and not the attributes of the communities such as institutional control patterns, the ecological segregation of classes, and present and past political cleavages. Further, the authors ignore the community power literature and make no attempt to identify local issues common to all or some of the communities (e.g., busing, the location of government housing, school bond issues) in order to ascertain whether different strata see local issues in stratification terms. Nor do they attempt to determine whether strata in different communities have differential access to local positions of power, prestige, and affluence. The systematic study of community characteristics which affect inequality remains an unfinished task for sociology. Until we do that job, we cannot accept the findings of Curtis and Jackson as conclusive.

Man and Work. By David Meakin. New York: Holmes & Meier, 1976. Pp. viii+215. $10.00.

Economic Means for Human Needs. Edited by Burkhard Strumpel. Ann Arbor: Institute for Social Research, 1976. Pp. ix+303. $14.00.

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These books are concerned with a common topic, personal well-being in modern industrial society. However, in perspective, in goals, and in data they treat the problem in entirely different and potentially complementary ways. David Meakin is concerned with the classic ideas about the importance of creative work; the contributors to Burkhard Strumpel's volume are concerned with empirical facts. Meakin treats the views of intellectuals; Strumpel and his colleagues treat the perceptions of workers themselves. In the end, Meakin presents a standard view of the topic while the Strumpel volume provides new insights on the complex issues connected with well-being and its relation to economic conditions.

Meakin argues that work should provide a means for self-realization and an individual's integration with society. It should be shaped by an ethic of joy and fulfillment in creativity. However, in modern society work appears to isolate individuals, to demoralize, and "to replace hope for any other kind of satisfaction by resignation to a sort of ersatz fulfillment in the acceptance of consumer goods. This transference of emphasis from creation to consumption is one of the most characteristic features of our civilization and culture" (p. 3).

Meakin's argument is a synthesis of the writings of a number of European literary figures. He treats writers as diverse as Tolstoy, D. H. Lawrence, Camus, E. M. Morris, Simone Weil, Marx, Carlyle, Jünger, Péguy. By tracing a major preoccupation in literature, by calling attention to an important social problem, Meakin accomplishes the goals he set for himself. As an introduction to the topic, the book can be valuable. But on another level, this narrow treatment of the problem oversimplifies issues of work and personal well-being.

First, many of the writers are novelists and romantic figures who do not observe the world in systematic ways. Often modern conditions are interpreted in a framework rooted in the past. For example, Morris and Weil believe that given the opportunity, workers will move away from the machine to do things by hand. It is impossible to restructure industry along the lines they suggest; even if it were possible, there is no reason to believe that workers would want it that way. The continued popularity of big cars and household gadgets in the face of rising energy costs points to the "love of the machine." Or to take another example, Meakin provides an entire chapter entitled "Towards an Organic Society." It treats two main ideas: (1) Work and personal life should be inseparable; (2) this unity can occur only in an organic community. There is, however, no discussion of Durkheim's warnings about the negative consequences of such communities even

though an earlier chapter treats Durkheim's ideas about "modern" alienation. Meakin repeatedly questions the "wordy optimism" and the romantic ideas of the writers he treats, but does not devote sufficient attention to the consequences these might have for the whole argument.

Second, the importance of leisure is underrated. Defining work as ideally a creative and varied activity means that there is less need to treat leisure and implies that any problem with the quality of modern leisure can be treated as the spilling over of tedious, repetitive work into all other daily activities. Leisure, however, is important in its own right. In modern society it is destroyed by factors other than the quality of work. Materialism and consumerism also shape leisure. True, such forces may be regarded as part of the same environment which damages work, but it oversimplifies matters to ignore the independent effects the various forces have on leisure.

Third, Meakin presents quite convincingly the case that modern industrial society has changed the meaning of work and the nature of contact between workers and the environment. But the more sociologically interesting problems bear on variation of the quality of the work experience through the social structure, the relation between work quality and personal well-being, and in turn the effect of these phenomena on the social system. Precisely such issues as these are treated in the volume edited by Strumpel.

The papers in *Economic Means for Human Needs* focus on individuals' economic well-being, that is, "how successfully they manage their affairs, or 'make ends meet,' and provide for emergencies, and how frequently they encounter and seize opportunities either for getting ahead and increasing their income or for getting out of an unsatisfactory work situation" (p. 3). The research reported in the book is based on a single project, "Economic Incentives, Values, and Subjective Well-Being." Data were gathered between 1971 and 1973 from samples in the Detroit and Baltimore metropolitan areas as well as from national cross-sections of the adult population. The common data base and the collaboration of the contributors during various stages of research, as well as the overview sections written by Strumpel, give this volume a coherence which is not common in edited books.

The following objectives directed this research: (1) the measurement of economic well-being, (2) the analysis of its various sources, both objective (e.g., socioeconomic status) and subjective (e.g., notions of equity and feelings of personal control), and (3) a consideration of the consequences of economic well-being for the larger system, that is, sources of societal malfunction and discontent. These issues of theory and method were linked to pressing social problems. For example, the potential effects of a long term decline in rates of economic growth are considered. As Strumpel points out, the field of psychological economics has developed during times of prosperity, and very little is known about people's reactions to an accumulation of adverse income, price, and employment experience. In sum, the problems tackled in this book are important and by and large the results are successful.

One of the major themes of the volume is the important and complex

effects of socioeconomic status on economic well-being. For example, Strumpel finds that measures of economic welfare have entirely different meanings in the different status groups. What people look for in a job and in a standard of living, the links among their feelings of satisfaction with income, with job, and with standard of living—these differ for whites and blacks and for the various occupational categories. At one extreme, blacks (mainly blue-collar workers) tend to have low expectations about their jobs. A good job is one which pays well and offers steady employment. However, a good standard of living, described by blacks as a "comfortable life," means a radically different way of life, and this is not linked to current or expected job. Professional workers are at the other extreme. They express the most satisfaction with their jobs, standard of living, and education. Professionals also place the greatest importance on the intrinsic rewards of a job. As it turns out, professional workers are the one group who reflect the classic ideal about the quality of work and personal well-being.

Further, Richard Curtin finds that incentives to make sacrifices and to take risks in the pursuit of economic improvement vary with socioeconomic status. The more limited resources and opportunities of lower-status workers encourage them to choose security rather than engage in behavior which might lead to advancement. He argues that personal values reinforce status position. Not only do the range of opportunities differ by status but so do the values and goals that might promote advancement.

This volume succeeds in impressing one with the intricacies of understanding and measuring economic well-being. For example, according to Gerald and Patricia Gurin, models treating feelings of personal efficacy have traditionally been too simple. The belief in personal control over one's life situation needs to be distinguished from a more generalized "belief in internal control, that success follows hard work and that failure is an individual responsibility" (p. 132). George Katona also calls attention to the need to distinguish between attitudes about one's personal economic situation and reactions to the economy generally. Beliefs about personal financial progress are not identical to attitudes about the economy in general. Over the years the former have been quite stable while shifts have occurred in the latter. The picture provided by these papers is further complicated by apparently contradictory findings which derive from the way concepts are measured, from the particular sample, or from an emphasis on one variable rather than another. What emerges from this book is scarcely a simple and complete view. Instead it represents a good beginning.

Organizational Effectiveness. Edited by S. Lee Spray. Kent, Ohio: Kent State University Press, 1976. Pp. 192. $12.50.

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This is an uneven collection of articles which, in toto, makes only a limited contribution to the literature on organizational effectiveness. Still, a few of

the selections are interesting and worthwhile. The sociologist or organizatic theorist concerned with effectiveness might desire to add this work to h collection, its overall problematic nature notwithstanding.

The papers were originally presented at a Kent State University coı ference. Conference organizers and editors of proceedings normally confroı a Herculean task, attempting to control the consistency and quality ‹ papers, make good use of their inevitable variety and uniqueness, and iı vent ingenious outlines and plans to make the whole package fit togethe Another typical problem is that of trying to meld the diverse interests (academicians and practitioners. It is obvious that Spray faced all of thes problems. In my opinion, he did a decent editorial job, considering the basi flaws in the material he was forced to integrate.

The four sections in the book deal with theory, research, utilization, an relevance. The articles in the section on theory are by far the best. Robeı Dubin discusses a fundamental dilemma when considering the question (effectiveness, specifically, "effective for whom?" The internal-external (efficiency–social utility differences are basic to an understanding of organ zational effectiveness, and Dubin presents the dilemma well.

A paper by William Evan takes an open-systems perspective and focuse on inputs, transformation processes, outputs, and feedback effects. To mea sure organizational effectiveness, Evan argues, one must measure organiza tional performance ". . . with respect to all four systemic processes as we as their interrelationships" (p. 19). Thus, effectiveness is a multidimer sional concept, which militates against a simple response to the questio of how well the organization is performing. Evan's nine effectiveness ratio —"systemic process variables"—suggest support for the criterion-choic dilemma raised by Dubin.

John Campbell concludes the theory section by examining the issue o effectiveness ". . . largely from the perspective of a differential, measure ment-oriented psychologist" (p. 29). In essence, his point is that the de velopment of criterion measures of effectiveness depends on the point o view or assumptions one brings to that undertaking. For example,

> The goal-oriented analyst would immediately seek out the principal powe centers of decision makers and ask them to state their objectives. He migh also employ techniques to reveal the actual operative goals of the organiza tion, as well as the publicly stated ones. For better or worse, once the re searcher had goals defined, he would try to develop criterion variables tha would measure how well the objectives (of either kind) were being met
>
> A natural systems oriented researcher would not immediately ask whaı the organization was trying to accomplish. Rather, he would explore anc ask questions, perhaps about the degree of conflict among work groups, the nature of communications, the level of racial tension, the percentage of jobs that were filled by people with the appropriate level of training, the job satisfaction of the organization members, and the like. . . . His concern would be with the overall viability and strength of the system. He would have some a priori notions of what the characteristics of a strong system are, and he would center his questioning around those. [Pp. 31–32]

The message rings clear. Decisions regarding effective performance ar not simple and straightforward. There is no scientific algorithm that ca specify "the" criteria of organizational effectiveness independent of th values and predispositions of the people involved in that judgmental process Effectiveness is a rich, multidimensional concept, a point that comes throug well in this section of the book.

The book goes downhill from here on. The section on research is dis jointed. Two papers focus on autonomy and turnover with little real justifi cation for assuming that autonomy will lead to better organizational per formance or that turnover is negatively related to organizational effective ness. An article on employee performance in two Japanese firms adds littl to an understanding of effectiveness. The final selection—on goal effective ness in an organization of priests—is more consistent with the theoretica issues raised in the previous group of papers. However, it is not enough t save the research section.

The section on utilization is confusing. The two papers focus, respec tively, on the effectiveness of organizational development (OD) program and on action research applied to OD. While the articles are fairly inter esting and well written, the link between OD effectiveness and organiza tional effectiveness is missing. In fact, I wondered whether the utilizatio papers were not originally intended for another conference, and not one o organizational effectiveness. That position is surely much too extreme, bu it does reflect my disappointment in the section.

The section on relevance highlights the inevitable—that practitioners se issues and problems (including those related to effectiveness) differentl from academicians. The participants seeking solutions to real questions ar "too busy with day-to-day problems" (p. 170) to get involved in basi theoretical issues. They point out, critically, that the conference materia "... has a long way to go in terms of being translatable into something w can use" (p. 171). And so on.

Some question of relevance is bound to arise at a mixed conference. cannot help wondering, however, how much the uneven nature of this col lection of articles may have added to the relevance problem.

Blue Collar Aristocrats: Life Styles at a Working Class Tavern. By E. E LeMasters. Madison: University of Wisconsin Press, 1977. Pp. ix+218 $3.95 (paper).

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The central problem investigated by LeMasters is the homogenization o American life-styles. In particular, he raises the question, Are the life-style of affluent manual workers converging with those of white-collar worker of the same income level? In another vocabulary, LeMasters is testing th cultural part of the embourgeoisement thesis common in the stratificatio

literature. His work can be viewed, in part, as providing an update of Bennett Berger's classic study of affluent auto workers (*Working-Class Suburb* [Berkeley: University of California Press, 1959]).

LeMasters utilizes the participant observation technique in his study of 50 highly paid skilled workers (mostly in the construction industry) and some of their wives. The observations on life-style were made over a three-year period and are restricted to the setting of a working-class tavern in a suburb of Madison, Wisconsin. An underlying, somewhat questionable assumption is that the spontaneous conversation in a tavern will reveal more about life-style than observation or interviews at work, at home, or in some other setting.

The book is well organized and analytic in its approach, with separate chapters on such relevant components of culture as the meaning of work, marital structure, the importance of relatives, attitudes on political and racial issues, and so on. In general, there is little support for the embourgeoisement thesis. The general culture and life-style of the taverngoer depicted here resemble those found in prior research on the working class by such writers as Gans, Kamarovsky, and Berger and in current research by Lillian Rubin (*Worlds of Pain: Life in the Working Class Family* [New York: Basic, 1976]).

The second chapter on the meaning of work, however, differs significantly from some prior research. The evidence from this study may militate against the conclusion of the Task Force on Work in America and others that most blue-collar workers are alienated from their work. Instead of being simply a means to a paycheck, work is of considerable importance. This is in large part a function of such factors as the rich peer-group life both on the job and after hours in the tavern, high wages, low supervision, considerable job security, and a sense of power and dignity obtained from a strong union. This sense of job satisfaction may be in large part a function of the differential importance of peer groups at work and the fact that these work-centered peer groups remain intact for a while after the end of labor. This pattern was not, for example, commonly found either in the impressionistic studies of such writers as Gans or in the survey research by Blauner.

The next four and one-half chapters (out of a total of 11) deal with aspects of family organization. Traditional family structure is marked by sharp sex-role stereotypes, rather alienated relationships between husband and wife involving such things as sex-segregated leisure activities and lack of in-depth sharing of emotions, a relatively high level of stress and strife, feelings of marital incompleteness, a mother-centered pattern of child rearing, an adult-centered orientation, and so on. For example, adult centeredness is indicated by the working class's voting down the construction of a community swimming pool for children. Adults argued, for example, that they swam in the local lakes when they were small and so could their children.

The importance of relatives in the working class is reconfirmed in the chapter on kin. The distrust of the institutions of the outside world, a category central in Gans's extensive work on working-class subcultures, is re-

demonstrated in the realms of politics, race relations, and the social welfare institution. Two chapters on the social organization of the tavern offer considerable insight into an area marked by little research.

The chief weaknesses of LeMasters's work lie in the methodological area. There is no control group of white-collar workers with which to draw life-style comparisons. More important, the sample, consisting of over 90% construction workers, is too selective. The comparability of these subjects to skilled workers in general or even to other taverngoing skilled workers is not demonstrated. The life-style findings are for a group of highly paid manual workers, born largely in rural Wisconsin, who work in open-air settings and engage in after-hours drinking behavior with their work-centered peer groups. This is a rather specific group, and the findings might well be different for other groups of highly skilled workers such as those in an urban factory setting. While LeMasters gets close to the reality of the actor, in keeping with the symbolic interactionist–ethnomethodologist technique, his actors are such a limited group that one hesitates to generalize the findings. Those seeking a more sound work based on alternative methodologies might select MacKenzie's *The Aristocracy of Labor: The Position of the Skilled Craftsman in the American Class Structure* (New York: Cambridge University Press, 1973).

The Dynamics of Sex and Gender: A Sociological Perspective. By Laurel Richardson Walum. Chicago: Rand McNally College Publishing Co., 1977. Pp. xv+246. $6.95 (paper).

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As a college-level women's studies text, *The Dynamics of Sex and Gender* is admirable on many counts. First, it is sociological—that is, Laurel Walum's analysis depends more on social facts than on psychological or biological facts to explain gender. This emphasis on the sociological is very hard to find in women's studies texts. Second, the writing strikes a good balance—it does not popularize but does offer clear and well-integrated synopses of research and internal summaries scattered throughout the chapters. And third, the author presents a consistent critical analysis of who benefits and who pays for the current pattern of gender inequality.

Walum's theme throughout is that social values and social structures in America mainly benefit achievement-oriented middle- and upper-class white males and penalize lower-middle-class and blue-collar males, particularly blacks, who are job dependent because they are the primary breadwinners, but who do not do prestigious work. Women of all classes are disadvantaged by the current system of gender stratification. They are doomed, if they are upper middle or upper class and white, to be emotionally supportive helpmeets for their career-committed husbands. If they are lower middle class or working class, they become the reserve labor force the economy summons

when cheap labor is necessary and dismisses when it is not, without in any way reducing their burden of wifely tasks. Black women are the most disadvantaged group because although, like men, they must work, as women they do not cash in on the benefits of the males' primary breadwinning status.

This political analysis runs as a thread throughout the book. In the first section, "Learning the Culture," Walum shows how language, early childhood socialization, and educational experiences separate the various groups and begin the process of class and sex stratification. She is particularly good on gender differences in black language and on the powerful effects of nonverbal communication (an area she explored previously in an article on the door ceremony). She is weakest when trying to integrate social-psychological and biological material with social-structural analysis, as in her précis of Garfinkel's account of the gender development of the transsexual, Agnes. Walum's version does not do justice to the subtle interplay of variables evident in the original.

The second section of the book, "Maintaining the Culture: Ideational Components," relies too much on outdated and unpublished material but is generally well written and informative at the student level. The areas of discussion are biology and society, psychology, law, and religion. Walum has some interesting and surprising things to say on the feminization of religion, which should cause controversy in and out of the classroom.

The third section, "The Structure of Sexual Inequality," not surprisingly turns out to be the strongest section, and the one that will be of most interest to nonstudent readers. In the chapter "Sexual Stratification," Walum marshalls her evidence for the social sources and supports for white male domination in our society. The chapter on the work world spells out its impact on the work lives of both men and women. This chapter badly needs updating, in that it fails to use Judith Long Laws's analysis of tokenism and Rosabeth Moss Kanter's work on sexism in large-scale organizations, both of which had been published in 1975, before this book went to press. There is an excellent chapter entitled "The Politics of House Life," in which Walum comes up with the original notion that since men are dominant in our society, in a truly equalitarian household the woman should be superior in education, occupation, or income.

The book ends with a chapter on power and one on men's, women's, and black liberation. Although there is the obligatory up-beat final note, these last two chapters are pessimistic in that Walum says feminist women can expect little from white middle-class men's liberation groups, since these men have little to gain and much to lose from a disappearance of sexual inequality. She also analyzes quite sensitively the impact of the race and gender hierarchies on black women's and men's roles and relationships.

All in all, this is a book I would recommend highly as a basic text in a women's studies course or in introductory sociology. It should be revised, however: Use of the most recent research on sexual inequality at work and

on the interpenetration of race, class, and gender in the American stratification system would do much to support Walum's thoughtful arguments.

The Longest War: Sex Differences in Perspective. By Carol Tavris and Carole Offir. New York: Harcourt Brace Jovanovich, 1977. Pp. xiv+333. $6.95 (paper).

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Subtitled *Sex Differences in Perspective,* this book is presented as a comprehensive text for women's studies courses, offering biological, psychoanalytic, learning-theory, sociological, and "evolutionary" (anthropological) approaches to the subject of gender. The authors, a psychologist (and former editor of *Psychology Today*) and a social psychologist, describe their main topic as "the chasm between men and women of all classes, cultures and races" (p. viii) and, appropriately, devote their first chapter to themes of misogyny in literature, philosophy, and history and to patterns of ascribed female inferiority in various cultures, especially that of the United States.

Since the book is intended as an undergraduate text, it would be unfair to fault it for the lack of integration that necessarily follows from its organization in terms of various perspectives. The authors provide an admirably thorough coverage of a great diversity of topics, ranging from a lucid account of prenatal development to a useful description of life in contemporary Israeli kibbutzim. The references, listed separately for each chapter but unfortunately at the end of the book, are useful too, being drawn from a variety of recent sources. Above all, the book is well written, it has dash and humor, and it is not superficial. This is far more than can be said of most textbooks currently available for courses in the sociology (or psychology) of women.

For courses in the sociology (or psychology) of *gender,* however, the book has certain drawbacks. Having begun with the "misogyny" theme in chapter 1, the authors are committed to a women's viewpoint, which is evident throughout the book. In chapter 2, sex differences are described primarily in terms of traits attributed (correctly or otherwise) to women, and in chapter 3, on sexuality, women's sexual problems, practices, and fantasies are given much more space than men's. Chapter 4, on genes, hormones, and instincts, is a little more even-handed, but even here, menstruation, menopause, and maternity occupy 15 pages, compared with two for "testosterone and temperament." Similarly, penis envy is discussed at rather extravagant length in the authors' sophisticated critique of psychoanalytic theory (chap. 5), while the oedipus complex is presented largely in terms of its inapplicability to women. Chapter 6, "The Learning Perspective," is a concise review of research on early socialization, media bias, self-esteem, and achievement

motivation as well as theories of cognitive development and parental identification. But again, the emphasis is on the effects of sex-role socialization on women, on women's self-esteem, and women's motivation. Finally, in chapters 7 and 8, which present the sociological and anthropological perspectives, power is discussed in the context of women's problems as "token" managers and executives; marriage is discussed in terms of "the housewife syndrome" and "the working-wife syndrome" (husbands are discussed in terms of how much housework they do); and cross-cultural comparisons refer mainly to the status of women as a function of subsistence technology and population pressure.

Of course, a book intended for women's studies courses must appeal to a largely female student audience, and it is true that in the not-distant past almost all textbooks in social science were written from a man's viewpoint. Books by and about women are still a welcome novelty in the social science literature as a whole, and they are still vital to the struggle to counteract the negative impact of male-dominated educational institutions on women. But for those who teach, or wish to pursue, a sociology of gender rather than a sociology of women, it is still legitimate to want books that explore the physiology, psychology, cultural history, and/or social roles of men *and* women more or less equally.

Given the authors' academic origins in psychology, they deserve much credit for a generally balanced treatment in which neither a psychological nor a sociological frame of reference is emphasized at the expense of the other. In their conclusion (chap. 9), they argue persuasively that social structure, economic conditions and attitudes, and values must all change if sexual equality is to occur (p. 290), though they implicitly assign causal priority to demographic and economic factors, for example, "Future events such as catastrophic war, economic recession or decreasing population could make it necessary once again to glorify large families and send women back to hearth and home" (p. 295). In fact, the space given to psychological material seems almost disproportionate in view of the tone of the concluding chapter, in which the authors seem to dismiss most biological and psychological sex differences as irrelevant in the light of cross-cultural and historical evidence that women's status depends on the food supply and methods of food production and distribution. Although most sociologists probably share this conclusion, the argument used to support it here is not entirely plausible. The authors' critique of theory and data from ethnographic sources is less sophisticated than their critique of psychoanalytic and learning theory. For instance, in chapter 8, which relies heavily on the work of Marvin Harris, a rather tenuous theory in which male dominance is derived from the relation between population and food supply is embraced uncritically. In contrast, the authors react cautiously to biological and psychological research findings, warning us to beware of experimenter bias and to avoid confusing correlation with causation. But why should correlations based on societal units be taken as evidence of causation when correlations based on individuals are not? On the other hand, chapter 8 also contains a

good critique of functionalist theory and a crisp discussion of the limitations of the analogy between cultural and biological evolution.

Occasionally, one wishes that Tavris and Offir had made more effort than they did to connect or integrate the different parts of their book. But fragmentation of this kind is perhaps impossible to avoid in an interdisciplinary text, and most readers will probably agree that in this case it is not a serious problem. Certainly it is well compensated for by the authors' consistent use of theory to organize empirical findings presented in each chapter. In chapter 7, for instance, they not only describe institutionalized sexual inequality but also explain why changes in institutions alone would not alleviate it. Similarly, in their discussion of efforts to achieve sexual equality in socialist countries (chap. 9), they demonstrate that neither an institutional approach nor a learning-theory approach alone accounts for the persistence of sex-typed roles.

Unlike books organized in terms of a single framework, *The Longest War* demonstrates that no single explanatory model is adequate to account for women's social inferiority in contemporary urban societies (or elsewhere). But having shown *why* sex-linked biological factors can have only a limited effect on human behavior, and *how* sex-role socialization is affected by its specific economic and institutional context, the authors necessarily, and convincingly, conclude that the environmental and institutional context must take priority over personality differences than can be attributed to early socialization. For this reason, and because the book is exceedingly well written, it is certain to be widely adopted for courses in sociology as well as women's studies.

Sixguns and Society: A Structural Study of the Western. By Will Wright. Berkeley and Los Angeles: University of California Press, 1977. Pp. 217. $3.95 (paper).

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University of the Pacific

This fine work (first published in hardcover in 1975) deserves critical attention if for no other reason than that it demonstrates to the American audience the strengths inherent in structural analysis—and does so in an engaging, sophisticated, and original manner. Having read this book, one begins to realize just how serious an oversight the virtual ignoring of structuralism by American sociologists has been.

Wright has chosen for analysis American Western films that have been singled out by the industry as the top money-makers of the year based on distribution receipts of over $4 million per film, for the years 1930 through 1971 (and one film from 1972 which Wright was sure would appear on the list for that year when it was finally compiled). Claiming that "there have been no serious, systematic studies of the Western" (p. 2) (a claim some

would take issue with), he stakes out his territory and describes the purpose of his book to be an explanation of the popularity of this cultural form. Treating the Western film as myth, he views it as standing between individual consciousness and society. To be popular, as are these films, the myth must "appeal to or reinforce the individuals who view it by communicating a symbolic meaning to them" (p. 2). So far, Wright is speaking pure Claude Lévi-Strauss, but it is here that they part company. Wright posits that this symbolic meaning must reflect the particular social institutions and attitudes that have created and continue to nourish the myth. Thus, the structure of myth can and does change as the social world in which it is embedded changes. "My argument, then, is that within each period the structure of the myth corresponds to the conceptual needs of social and self understanding required by the dominant social institutions of that period; the historical changes in the structure of myth correspond to the changes in the structure of those dominant institutions" (p. 14). Building from the more psychologically oriented base provided by Lévi-Strauss, Wright has created a structural analysis that is completely sociological in character. It is for this reason the book is so important.

By synthesizing the work of Lévi-Strauss on the conceptual structure of tribal myths (and carefully explaining where and why he takes issue with him in a "Methodological Epilogue"), that of Kenneth Burke on the social structure of literature, and that of Vladimir Propp on the narrative structure of Russian folktales, Wright creates a methodology by which he identifies four basic plot structures in Western films, which he labels classical, vengeance, transitional, and professional. Films of each type seem to have appeared in a rough evolutionary sequence that Wright believes to correspond with changes in major American institutions (mainly economic) from 1930 to 1972.

In order to relate the four plots to their social context, Wright provides "an analysis of the social institutions of America" (p. 130) and attempts a demonstration of the correlation between the structures of the Western plots and the structure of these institutions. He further attempts to show that the structure of the social institutions covaries with changes in Western plot and that "this institutional change predates a little but approximately corresponds in time to the change in structure of the myth" (p. 130). He begs the question of influence: "My purpose is not to show how institutions create a myth, nor to show how myths create institutions, but to show that the structure of any social myth . . . must symbolically reflect the structure of social actions as those actions are patterned and constrained by the central institutions of society" (p. 131). Even so, his structural analysis of the meaning of these films and the correspondence of this meaning to the attitudes, values, and contradictions arising from changes in American economic institutions from 1930 to 1972 is both highly original and fascinating.

For each of the four types of Western, Wright offers short plot synopses of representative examples, then proceeds carefully and in detail to a structural interpretation of films of each type. According to his analysis, the classical plot (found in such films as *Shane*) reveals that the way to achieve friend-

ship, respect, and dignity is to separate oneself from others and to use one's strength as an autonomous individual to aid society. This plot exists in the context of what Wright describes as a restricted but active market economy (pre-World War II). The vengeance plot—actually a variation on the classical and seen in films such as *One-eyed Jacks*—weakens the compatibility of the individual and society by showing that the path to love and respect is to separate from others, to struggle against strong enemies, yet to strive to remember and return to the values of marriage and humility. This plot exists in the context of a tentative planned economy. The transitional plot (*High Noon*), anticipating new social values of changing economic institutions, argues that love and companionship are available to the individual who stands firm against the intolerance and ignorance of society—but at the cost of becoming an outcast. Finally, the professional plot—in the context of a post-World War II corporate economy and seen in such films as *Butch Cassidy and the Sundance Kid*—argues that companionship and respect are to be won only by becoming a skilled technician, joining an elite group of professionals, accepting any job that is offered, and being loyal only to the integrity of the team, not to any competing social or community values. Wright effectively underscores the applicability of this plot to American society in the 1970s by quoting John Mitchell and others speaking in their own defense during the congressional hearings on Watergate and related activities.

As the author sums it up, "In the sixties and seventies, the traditional conceptual conflict between the idea of society and the idea of the individual has been transformed into a conflict between society and an elite group. This is perhaps one of the most significant consequences of the emergence of capitalist technology as a social and ideological force" (p. 184).

Wright seems to expect to raise academic hackles with this book. "I have entered this new, untamed territory armed with a theory, a methodology, and a set of data; with these weapons, together with Truth and Justice, I intend to drive out the evil ranchers, railroad men and professors in order to show there is redeeming social value in this seemingly barren intellectual wasteland" (p. 2). He will probably offend pop culturists, many of whom have produced the studies of Westerns he dismisses ("There have been very few analyses of the Western; those that exist are mostly superficial and precious" [p. 200]); he will offend literary and social critics who discuss such concepts as "garden and desert," the affirmation of violence, and the gun as phallic symbol—all of which (and more) Wright pointedly ignores; he will offend the Lévi-Strauss school of structuralism, as he takes issue with the master (carefully detailing his position in the "Methodological Epilogue"); he will offend positivists with his "soft" methodology; he will offend those social scientists who feel popular culture is not appropriate as subject matter for serious social science; he will offend those who feel the most important variable in the popular culture equation is the culture industry itself, as he dismisses the impact of stars such as John Wayne on the popularity of the films they appear in and does not discuss the role of the film industry in shaping media content or the structure of the Western plot;

he will probably offend many others (especially Marxists)—but he never offends without carefully detailing the reason why he chose to construct the study as he did, and, in most cases, he defends his approach admirably. One can almost see the Guys in the Black Hats blustering as they pack to leave town.

Wright's sketchy analysis of American institutions, drawing on the works of Karl Polanyi, Jürgen Habermas, and—most heavily—John Kenneth Galbraith, is the least satisfying portion of this work (especially for those who take issue with Galbraith—and I suspect there are many of us), even as it is a most crucial portion. This is a serious problem. To the extent that one does not accept Wright's interpretation of economic institutional change, one may be tempted to call his whole thesis into question. This situation is unfortunate, in that it tends to obscure the brilliance of his adaptation of structural analysis as a sociological tool of inquiry. Make no mistake—this is a most important book for anyone interested in seriously studying culture, popular or otherwise.

Six Essays in Comparative Sociology. By André Béteille. New York: Oxford University Press, 1976. Pp. 133. $4.00.

Ronald Frankenberg

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The first three chapters in this book are the national sociology lectures delivered by Béteille at three Indian universities during 1972 and 1973. They are entitled "Sociology and Social Anthropology," "Peasant Studies and Their Significance," and "The Concept of Peasant Society." In clear and lucid prose reminiscent of Radcliffe-Brown's, Béteille argues that the sharp division between sociology and social anthropology is meaningless and administrative, not only in Europe but also in India, where its meaninglessness is especially pernicious because of colonial overtones. He also argues that European anthropologists turned to peasant studies when tribes seemed to be disappearing and exported their illusions about European peasantry to India where, with sociologists, they turned a loose category into a concept of peasant society which is unconvincing, inept, and unreal.

After reading these three chapters, I began to think of Indian social science in Roy Campbell's terms: I could see the snaffle and the bit all right, but where was the bloody horse? In a constructive effort to reorient Indian studies, Béteille has added three more chapters—"Tribe and Peasantry," "Peasants and Workers," and "Marxism and Modern Sociology." In his preface, he suggests that the second is the more important part of the book and may be more lasting in its interest than the first. The whole provides a stimulating introduction to Indian studies (and, by extension, Third World sociology) and, at least for me, puts them in context, albeit a context I wanted to look at rather critically. The comparison between social anthropology and sociology suffers from an unduly dichotomous approach; in

fact, kinship imposes itself on anthropologists only because tribal societies were neither prestige stratified (like those studied by U.S. sociologists) nor riven by class division (like those studied by European sociologists). The philosophical and practical problem of what anthropologists and sociologists respectively imposed on the societies they studied is rather glossed over. Recent arguments and counterarguments radiating from Paris to the effect that kinship is the class of precapitalist society arrived too late. Béteille stresses that the two disciplines have a common ancestry in Durkheim and the *Anneé sociologique* group but does not point out that it had two branches: sociology derives from *Suicide* and *The Rules of Sociological Method*, social anthropology from *The Elementary Forms of Religious Life*. Aron and Lévi-Strauss as the twin suns of French sociology seem to have had less longevity than Béteille suggests. At least in Britain they have been eclipsed by Althusser, Lacan, and the like.

The peasant-studies chapter focuses on villages which combine the important principles of kinship and stratification and a comparison of the findings of Indian and Chinese studies fertilized by the anthropological determination to understand the life of peasants by living it. Redfield gets much credit for initiating this approach both in China (through Fei Shao-Tung) and in India. Mao Tse-tung had, of course, done it first, and while Fei may not have known this in the 1940s, certainly Béteille knows it now but does not say so. He is less coy about the relationship between Tagore and Gandhi and the pioneering Indian anthropologist N. K. Bose. Béteille states that village studies (apart from C. K. Yang's) stop dead in China in 1949. Presumably, he consciously rejects Hinton's *Fanshen*, Myrdal's two books, the two studies by David and Isabel Crook, etc. Like Western pro-Soviet Marxists, Béteille seems to regard the ways of Communist China as a total mystery. Without taking up the late Maurice Freedman's devastating attack on building up a view of total societies by using communities as building blocks, he recognizes the limitations of the attempt and the uselessness both of the "vast and amorphous sociological category" peasant society and of attempts to divide India into an urban society, side by side with tribes, and a peasant society for anything that does not fit too neatly into the first two categories. He recognizes how "functional" caste has often been used as an excuse to avoid the analysis of conflict between interest groups in the Indian countryside. His anxiety to avoid overgeneralization about peasant society leads him into often interesting and challenging detail. One example of this is the observation that upwardly mobile rural men in India withdraw their wives from agricultural labor and then follow them into a life of cultured idleness later, whereas in pre-Communist China the process was the reverse.

What Béteille could get from modern Marxism is the methodological impulse to move in the opposite direction from the types of studies he criticizes. The best refuge from inadequately abstract concepts may well be adequate concepts rather than empiricism. "Peasant society" may be too general, or it may not be general enough. His last two chapters, which are both essentially about the relationship of Marxism to sociology, deserve more detailed comment than I have room for. In hindsight it is ironic that Lukács and

Goldman should be his examples of Marxists unwilling to compromise with sociology, since one recent British book, as well as many recent French ones, dismisses both as non-Marxist Hegelians fit only to associate with sociologists. He is less wrong than most British and American writers about Marx on peasants since he seems to have read the *18th Brumaire* instead of emulating Lévi-Strauss and reading only a few pages occasionally. He understands the importance of a narrow definition of peasantry.

He is correctly impatient with Indian Marxists who have not taken to heart Lenin's definition of Marxism as the "concrete analysis of concrete situations" and are too theoretically (or dogmatically) proud to learn from non-Marxist sociology. I do not, however, think he has come to terms with classless society as a dynamic process (any more than Soviet theorists have, as he correctly points out, in their evasive concept of strata). It may be and probably is true that some Indian Marxists are looking as blindly to China as their pre-1956 predecessors did to the USSR. Some, however, may understand that it is a different society, with a differently understood theory, in a different historical situation. No sociologist or Marxist, anywhere and certainly not in India, can afford to turn a blind eye in that direction.

The Outlook Tower: Essays on Urbanization in Memory of Patrick Geddes. Edited by J. V. Ferreira and S. S. Jha. Bombay: Popular Prakashan, 1976. Pp. xxxiii+475. Rs 120.00.

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Inasmuch as the 24 essays and articles in this volume are addressed to different groups of readers, it seems impossible to evaluate them as a collection. They fall into four classes, however.

1. The first five selections concern the life and works of Patrick Geddes, early town planner and urban sociologist and the founder of the University of Bombay Department of Sociology. During the 1890s in Edinburgh, Geddes established the Outlook Tower, an eight-story edifice, as the first "sociological laboratory": "Geddes intended that the Outlook Tower would mould a comprehensive or synthetic [view-point] . . . : that of the citizen who strives to understand all aspects of the place where he lives and weighs its evils of ugliness, poverty and crime against its heritage of scenic beauty, natural resources and human culture" (Boardman, p. 5). While in Scotland, Geddes began to develop his concepts of town planning: "diagnosis before treatment" (the regional survey) and "conservative surgery" (rather than obliteration of slum areas). Despite advancing age, Geddes spent most of 1914–24 in India; there he conducted extensive research, including surveys of some 50 Indian urban areas. Indeed, several of the contributors find Geddes as remarkable for his single-minded devotion to empirical research as for the timeliness of his proposals for urban restoration.

2. Five articles use aggregate data to describe U.S. urban areas and to

identify changes therein. For example, in a 30-page article Irene B. Taeuber traces migration and urbanization in the United States from 1607 to 1960. Several of these pieces have been written by well-known scholars (e.g., Amos Hawley and Leo F. Schnore) and provide excellent short introductions to urban studies. They would be appropriate for introductory courses surveying the social sciences were it not for a defect to be discussed below.

3. Three other articles are highly specialized and useful to a select group of scholars. Edgar F. Borgatta and Jeffrey Hadden, for instance, contribute a detailed, 64-page critical review of "social area analysis," an empirical approach to the study of urban characteristics associated with Shevky and Williams.

4. The balance of the articles are studies of non-Western, chiefly Indian, cities and city dwellers. Topics range from planning and physical structure to patterns of interaction and the problems of the Anglo-Indian minority. Those who infrequently peruse the area journals will find this last set of readings exotic.

The second and third groups of articles suffer from a flaw: They are out of date. Several attempt to describe contemporary demographic trends and characteristics, yet not one of them (apparently) uses a data base more recent than the 1960 census. Despite the lag between the time of writing and the 1976 publishing date, only one article has been updated—and only through one footnote. The time element is particularly irritating when several writers urge further research in a particular area. The reader wonders exactly what *has* occurred in the seven or more years since the article was written. Even the most provocative piece in this collection (by Joel Smith), a query about possible sampling error in the widespread use of individual communities as research settings, suffers from this defect. Do such studies predominate *today*?

In short, *The Outlook Tower* is more an aggregate than a collection of works that complement each other. Its greatest virtue—albeit one with limited appeal—is its collection of essays on Patrick Geddes. The Indian case studies can be recommended, but other selections may be of most interest to historians.

The Corrosion of the Self: Society's Effects on People. By Thomas Kreilkamp. New York: New York University Press, 1976. Pp. xvi+235. $12.50.

R. S. Perinbanayagam

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Many have lamented the deleterious effects of society on the individual, the limitations and injuries it does to free and autonomous beings. Such a Rousseauistic view has long been passé in circles whose thought has been touched, however slightly, by the ideas of Durkheim, Mead, or Marx. Such a view is possible only if one insists on an artificial separation of society and the individual, ignoring the dialectical nature of the relationship between

self and society. Thomas Kreilkamp comes to develop anew the thesis of an oppressive society that leads to the "corrosion of the self."

"The primary aim of this book is to provide a conceptual framework within which certain general issues can be considered" (p. xi), observes the author, who proceeds to list some of these issues. He wishes to clarify "the relation of self to society" (p. xi) and "to inform the reader of the conceptual side of contemporary social psychology, which he claims "people" do not seem to appreciate—at least judging by public debate on such issues as family and school and their effects on the formation of self" (p. xii).

To achieve these aims the author discusses the work of Herbert Marcuse, Ronald Laing, Erving Goffman, Carl Rogers, Kenneth Burke, and Thomas Schelling, in the hope that "the reader may be persuaded that these authors are fascinating and original" (p. xiii). Before he treats them, however, Kreilkamp lays low a number of others: Kenneth Keniston for *The Uncommitted,* Elliot Aronson for *The Social Animal,* and B. F. Skinner for his *Walden Two,* of all things. Various criticisms are leveled against the psychoanalytic biases of Keniston and the inadequacy of the experimental method. Skinner's ahistorical and astructural views too are properly castigated. But these are tired points, and though they have often fallen on suspicious and unresponsive ears, I do not see that they bear repetition.

Marcuse is next taken up for discussion, and at times it is not very clear whether the author approves of him or not. He passes various summary judgments along the way while wading through *One-dimensional Man* and *Eros and Civilization.* Sometimes the author, as if unsure of his own judgment, leans on Lionel Trilling's essay on Matthew Arnold to further discount Marcuse's influence. While Kreilkamp has not systematically misinterpreted Marcuse, he does not appear to have anything but a superficial understanding of his work. Further, he is not sure what to do with Marcuse. After listing the latter's inadequacies, he still thinks Marcuse will "help us see the importance of taking ideas seriously," no less.

When the author moves into a review of Laing, he develops a surer touch and discusses his work with insight and understanding, and he is able to conclude that "we will need to move beyond a simple-minded distinction between conformity on the one hand and autonomy on the other" (p. 115). Yet this idea is not confronted squarely in the rest of the book; mere passing stabs at it are made now and then. This constitutes the central weakness of the book. The same can be said of the treatment of Goffman. Very lucidly Kreilkamp summarizes Goffman's ideas and vision of society and self and then lets his own theme slip out of his hands, gently. However, it must be asserted that his examination of Goffman is deft and insightful—something that cannot be said of many who have written on him. The manner in which he connects the various essays, books, and tales of Goffman to extract their common and extremely profound theme makes this chapter on Goffman one of the best written on him. The chapters that follow deal with the work of Carl Rogers, who properly elicits a number of sharp comments for his simple-minded claim of the possibility of a "self that is unhooked from all determination by external contingency" (p. 155), from all the effects of

social structure and the distribution of power and resource in a given society.

The last two authors dealt with are Burke and Schelling, in a chapter whose title, "Toward a Relevant Conceptual Structure," implies that these two authors are somehow capable of solving the problems raised earlier in the book. Yet a short extrapolation of the work of Burke and of Schelling is undertaken without facing the resolution of the issue that is the ostensible theme of the book. If the chosen metaphor for the relationship between humans and the society they live in is "corrosion" the author has a responsibility to present a discussion of how a self can even begin to arise outside a society. If, however, the thesis is that *particular* forms of society are corrosive, some attempt should be made to describe a society where such corrosion is minimal. The author makes no such attempt. Discussing the oppressive aspects of a society or of societies in general and developing a particular thesis about it or them cannot be efficiently undertaken by extensive examination of authors selected because they are "fascinating and original" (p. xiii). Instead one must cultivate an intense personal vision and then use other authors to buttress it.

Medicine under Capitalism. By Vicente Navarro. New York: Prodist, 1977. Pp. xvi+230. $14.95 (cloth); $5.95 (paper).

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In one of the most important critical analyses of the field of health and service in the past decade, Vicente Navarro observes that much of the previous research has been done "by experts on trees who neither analyze nor question the forest but accept it as given" (p. 135). His basic position, presented in the introduction and throughout this collection of essays, is that "the composition and distribution of health resources are determined by the same forces that determine the distribution of economic and political power in our society. Indeed, I would postulate that the former cannot be understood without an understanding of the latter" (p. 135).

Most of the clearly written essays that this book comprises have been published before, but in places which make their gathering together here convenient and useful. Navarro has avowedly and deliberately attempted, in these studies, to engage the "mainstream" of liberal medical sociology in a debate. The theoretical framework that ties all of these essays together, to the point where they do constitute a book, is that of interdisciplinary, modern Marxian theory, especially as it has been developed by such researchers as Offe in Germany, Poulantzas in France, Miliband in England, and O'Connor in the United States. I must stress at the outset that this is a work of extremely serious scholarship, by a public health physician with wide training in political sociology and detailed familiarity with the health and socioeconomic data of Western Europe, the United States, and Latin America. Navarro chides the rhetorical enthusiasts on the Left for having

in the past discredited the real and practical usefulness of their theory by refusing to submit it to a test; he himself tests it repeatedly.

It is Navarro's contention, proved to the point that the majority of medical sociologists ignore it at the peril of their own ignorance, that the sociopolitical and economic context of advanced capitalism strongly affects the level of health in both "developed" and "underdeveloped" sectors of Latin America and the United States. At the same time, this political-economic arrangement constrains or prevents any significant solution to health problems which threatens the interests of capital.

The first part of the book, "The Underdevelopment of Health in Capitalist Societies," begins with a general critique of the liberal economists' notion of "underdevelopment" and contrasts it with the Marxian model of deliberate economic colonialization. This is followed by an excellent study of the Chilean health system before, during, and after Allende. The grim details are conveyed dispassionately by an author who grew up in Fascist Spain and who can read the dispatches in the original—surely a test of restraint if one needs to be found. In a very useful juxtaposition, the next two essays note the contextual reasons why health is so poor in Appalachia and in America's factories. Comparative statistics are presented, along with the basic political-economic reasoning, to explain why the coal barons of West Virginia and the chemical industry stand to lose more money than they gain from the health of workers.

In part 2, "The Meaning of Health and Medicine," Navarro provides a critique of Illich from the classical Marxian left, noting that Illich's refusal to specify the profit motive in his analysis, and his recommendations of self-cure and cutback of services, play into the hands of a corporate sector that is headed this way itself as a cost-saving measure. Illich's Luddite analysis does not impress me either, and for similar reasons, though overtechnologization is a problem.

Navarro looks at the demography of health manpower in part 3, "The Political Economy of Health Care." He finds the social class hierarchy of the health occupations reflective of, and only explainable by, the class system of the wider society. In a special analysis of the position of women, he notes that the majority of women in general and of women in health-related occupations are exploited and on the lower rungs of the occupational ladder. Women physicians are not an exception in the class analysis: Replacing the sons of doctors with the daughters of doctors constitutes neither structural reform nor true progress.

In part 4, "Class, Political Power and Health Care," the author surveys briefly the recent developments on the theory of the role of the state in advanced capitalism. He then shows some of the complex ways relationships are formed among capitalist sectors, state mechanisms, fiscal crises resulting from attempts to solve problems unsolvable within the capitalist arrangement, and the consequences all of these have for health care and the health service professions, as well as the consumer. The Marxian concept of class struggle is operationalized here and shown to have real usefulness.

A few minor criticisms are in order. First, because the book is a collection,

there is some repetition, but with the one exception of too many tables on the socioeconomic composition of the American work force, it is not a problem. Second, the broader the canvas, the less complete the analyses become. Thus the details on Chile, Illich, and health manpower are stronger than the broad strokes on the state in advanced capitalism. In the last section (though not elsewhere) some unjustice is done to Poulantzas, who is stereotyped as both an economic determinist and an abstract structuralist, and whose concept of the relative autonomy of the state is then discussed without footnoting him. Dedicatory statements at the outset of each chapter prepare one for rhetoric, but instead, far more often one finds committed scholarship and challenging hypotheses.

The years to come will, I think, find us all in Navarro's debt. His analyses, and the program of research they imply, should be required reading for all students of health and service.

The New Charismatics: The Origins, Development, and Significance of Neo-Pentecostalism. By Richard Quebedeaux. Garden City, N.Y.: Doubleday & Co., 1976. Pp. xii+252. $7.95.

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A revised version of Richard Quebedeaux's 1975 Ph.D. thesis in history at Oxford University, *The New Charismatics* is very well researched, as might be expected. The writing, however, has none of the scholastic cumbersomeness that theses often reflect. This is a highly readable book that weaves together several perspectives in a very smooth fashion. The author's knowledge and use of these perspectives appears balanced and competent. The discussion of the Neo-Pentecostal movement's leading personalities makes truly interesting reading.

Quebedeaux summarizes his own work as follows: "The scope of the present study embraces a historical, sociological, and ecclesiastical appraisal of Neo-Pentecostalism. I am chiefly concerned with its origins, development, and significance as a contemporary movement within the recent history of Christianity and the history of culture more generally. The strictly theological character of Charismatic Renewal . . . does not play a major role in this presentation. . . . What is particularly original about the present study is its treatment of Neo-Pentecostal distinctives—leadership, faith and practice, causes for the movement's emergence and success, its relationship to trends in the wider society—and its examination of Classical Pentecostalism and Charismatic Renewal contrasted with each other" (pp. 209–10).

Although some references are made to the Charismatic Renewal elsewhere in the world, the book focuses primarily upon the movement within the United States and Great Britain. Within the cultural framework of these two countries, the author describes in the first three chapters the historical background and contemporary emergence of the Charismatic Re-

newal. In chapter 4, he examines the sources and types of leadership within the movement as well as reviewing the influences of prominent figures. Chapter 5, entitled "Faith and Practice," surveys the movement's various activities, especially their intra-, inter-, and nondenominational character. While Quebedeaux does not gloss over conflicts and critical evaluations of Charismatic Renewal, it is quite apparent that the movement does bridge some important historical gaps among various Protestant denominations as well as those between Protestants and Catholics. One of the main reasons for its success in this area is its emphasis upon religious *experience* rather than religious doctrine. On the other hand, some of the movement's sharpest critics are those who reject the emphasis upon religious experience and assert the importance of doctrinal differences, as do many churches in the fundamental-evangelical tradition.

Chapter 6 summarizes one of the major themes of the book—the contrasts between classical Pentecostalism and Neo-Pentecostalism, or Charismatic Renewal. Modern pentecostalism has been very evident throughout the world since the beginning of this century and the Azusa Street revival in 1901. However, its impact has been limited largely to the lower social classes and until less than 20 years ago was external to the Catholic, Eastern Orthodox, and mainline Protestant denominations. The striking feature of Charismatic Renewal is that it is essentially a middle- and upper-middle-class movement that focuses upon religious experience. While persons involved in the movement still represent a very small minority of any one denomination, charismatics are found in almost the entire range of denominations, from conservative to liberal. Persons involved participate in charismatic activities both within their own denomination and in numerous inter- and nondenominational gatherings as well. It is this type of interpersonal linkage that has particularly characterized the movement as church and religious *renewal*.

In contrast to this picture of tolerant diffusion, Quebedeaux correctly characterizes the classical pentecostal churches as being, at least traditionally, sectarian. He does, however, describe some important changes that have taken place in the classical pentecostal churches and make it increasingly difficult to describe them in conventional sectarian terms. The Charismatic Renewal in the mainline churches has been an important factor in changing the classical pentecostal churches, since it has introduced opportunities for communication and participation that did not previously exist.

The author concludes the book with two more chapters reviewing the opposition to and acceptance of the movement and the reasons for its success. Finally, he provides a postscript assessing the future of the movement and an appendix reviewing various scholarly investigations, including those focusing upon glossolalia.

In conclusion, while there are many important sociological implications of this social movement, there is one factor that deserves additional comment. Sociologists, as well as the historian Quebedeaux, frequently impose middle-class biases upon their data. For example, Neo-Pentecostalism is considered important at least in part because it has been adopted and modi-

fied by the middle class. The author frequently refers to classical pentecostal practices as consisting of unnecessary "cultural baggage" such as hand-clapping and shouting. On the other hand, he does not attempt to apply this same criticism to the new middle-class practices that undoubtedly contain excesses of their own, such as undue concern about having a quiet service.

Status Characteristics and Social Interaction: An Expectation-States Approach. By Joseph Berger, H. Famit Fisek, Robert Z. Norman, and Morris Zelditch, Jr. New York: Elsevier Scientific Publishing Co., 1977. Pp. xi+196. $14.95.

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Research programs, undertaken by theory groups working within some sort of paradigm, are our present-day response to the continuing problem of producing cumulative knowledge in sociology. When Nicholas Mullins surveyed the field in *Theories and Theory Groups in Contemporary American Sociology* (New York: Harper & Row, 1973), he applied a model of scientific theory group transformations to sociology. The model included such components as intellectual leadership, paradigm content, research and training centers, "successes," program statements, and critical reactions. Through various stages—from undifferentiated inclusion within a normal state of a field, to network formation, to clustering or "thickening" of ties among group members, to eventual specialty status—a theory group proceeded either to achieve incorporation into the discipline or to die at some point by failing to make the requisite transformation to the next state.

One theory group Mullins did not analyze is represented by the monograph under review. I will make use of the general social and intellectual aspects of theory groups outlined by Mullins. Thus I will try to indicate the overall paradigm within which the present monograph may be located and evaluated. The paradigm may be called Expectation States Theory (EST), and the corresponding network of group members I will call "the EST group." My estimate is that it includes some 20 or so active research members and perhaps a dozen peripheral members. The group's most widely recognized intellectual leaders are Joseph Berger, Bernard P. Cohen, and Morris Zelditch, Jr. Each of the three is a Harvard Ph.D., a product of the Social Relations Department of the 1950s. In fact, an important starting point of the entire development of this group was Berger's Ph.D. thesis concerning theoretical problems abstracted from the Bales setting. Thus, EST has emerged from two of Mullins's "past" theory groups, "Standard American Sociology" and "Small Group Theory." And some of the premises incorporated into EST (e.g., the relational, cognitive, and situational foci) are drawn at least in part from Mullins's third "past" theory group, "Symbolic Interactionism."

Since the early 1960s these three EST intellectual leaders have been at Stanford, where the Laboratory for Social Research has provided an important research and training center. One other research and training center exists, I believe, at Michigan State University. The group includes a very small core of former and present colleagues as well as a relatively large number of productive former students of the intellectual leaders (such as Thomas Conner, Hamit Fisek, Lee Freese, and Murray Webster). The coauthorship network reveals some dense connections not only among the intellectual leaders and other colleagues but between them and some of these former students. Berger and Zelditch, who have collaborated on about a dozen publications, appear to have the highest coauthorship frequency, and Berger is the "sociometric star" in the network of coauthorships.

Through journal articles, chapters in edited books, research monographs, and unpublished papers circulated in the group, a certain paradigm content has emerged over time. I would characterize it as follows: Via abstract and analytical scope-defined formulations, empirical regularities in task-oriented social interaction may be experimentally studied and theoretically explained. This explanation is achieved by explicit and testable theories which have in common their utilization of the theoretical construct, "expectation states."

Let me try to unravel this somewhat cryptic statement. To see where the authors are starting from, one has to recall the early emphasis placed on "verificational studies" by Hans Zetterberg in his *Theory and Verification in Sociology* (Totowa, N.J.: Bedminster, 1965), first published in the 1950s. Verification studies derive their significance from their confirmatory or disconfirmatory relation to a set of explicit premises in a theoretical argument being put to a test. The importance of the situation studied is irrelevant apart from this function for theory, so it may well be a "trivial" situation. Yet it may be of massive importance for the evaluation of a theory covering an unlimited class of situations of which the given instance is one conveniently studied member. In the EST paradigm, as it bears upon the theory of status characteristics and social interaction, the class of situations ("status situations") is rigorously defined and a special experimental instance is devised ("the Berger situation") wherein social interaction can be controlled and monitored. Since verification is concerned with general propositions and not with initial conditions, it is imperative for a good verificational study that the initial conditions be known. A derived prediction which fails can then be more readily attributed to a problem in the general theoretical premises.

The abstract class of situations for which the principles are formulated constitutes the scope of the theory. For instance, in the class of all abstractly defined status situations, those which involve only one "diffuse status characteristic," only one task, and only two actors may be taken as the first subclass for analysis: This is the initial scope formulation, "the elementary status situation." The Berger situation can be controlled so as to provide an observable instance of the class so that the first theory, concerning how power and prestige differences arise within such a situation under varying conditions, can be subjected to a verificational study. If confirmation is

achieved, the scope can be extended. What this means is that the theory is generalized to subsume the confirmed first theory as a special case. For example, one can extend the theory from elementary situations to those in which there are at most two status characteristics. In the wider class of situations, new phenomena occur. A theory which succeeds in explaining the new phenomena and is framed so that it also explains the earlier phenomena has been successfully extended. There are, therefore, two aspects to extension. One is theoretical: The new theory must be exhibited so clearly as to make obvious, or deductively demonstrable, that the old theory is a special case. The second aspect is empirical: The extended theory must be tested for the class of situations that realize the new scope conditions nonvacuously. For example, a Berger situation may be set up so that individuals see two status characteristics as discriminative of self and other, say educational status (college vs. high school) and sex. How do we know there are not three such status characteristics from the standpoint of the actor? In natural situations one could not be sure that this initial condition obtains, but the Berger situation (including the surrounding know-how of experimental manipulations) can be controlled to assure that only two discriminative status characteristics are initially present. The aim of the new book, structured into two parts, is to describe earlier extensions of the theory of how status characteristics organize social interaction (part 1) and to formulate a systematic new extension to a very wide class of situations (part 2).

Further aspects of the EST paradigm content may be found in various publications that contain what we can regard as program statements. Important in this connection are the introductions to the two volumes of *Sociological Theories in Progress* (New York: Houghton Mifflin, 1966, 1972), edited by Berger, Zelditch, and their former Stanford colleague Bo Anderson (now at Michigan State). It should be noted that only a few of the papers in these volumes are contributions by the EST group. In fact, the first volume has been cited by Robert Merton (in *Theoretical Sociology* [New York: Free Press, 1967]) as a good source for numerous examples of systematic middle-range theory. The editors' introduction to that volume cites Zetterberg in regard to the need for abstractly formulated theories of analytically discriminated processes. It also quotes Talcott Parsons on the need for a working theoretical tradition. The EST group certainly fulfills these needs: It simultaneously realizes Zetterberg's precepts concerning verification, Parsons's call for the routinization of commitment to cumulative theoretical knowledge, and Merton's plea for theories of the middle range. It seems to have achieved this by building upon a landmark original paper, a success: "Status Characteristics and Expectation States" by Berger, Cohen, and Zelditch, which appeared in the first of the two edited volumes just mentioned. This paper set off a stream of verificational studies, revisions, extensions, refinements. Part 1 of the present book summarizes this decade of research findings.

The group emerged clearly in the 1960s in its network stage. It passed into a cluster stage, I believe, in the early 1970s. I judge this by the "thickening" of ties among colleagues and coauthors, particularly counting the

role of former students. By 1972, in the second of the two volumes mentioned above, the group's need to differentiate its point of view from other paradigms with which it might be confused became apparent. In particular, an effort was made to discriminate the EST group's type of sociological orientation from the orientation of those other believers in middle-range theory and empirical testing, the group Mullins calls "New Causal Theorists." New causal theory, according to the EST group, is committed to an essentially historical strategy rather than a generalizing one. In the former strategy a single type of effect is explained by appeal to a multitude of variables, measuring the causal contribution of each. In the generalizing strategy, however, by analytical abstraction there is a focus on a single process as such, regarding any natural series of events as compounds of many such "unitary" processes. Not the effects, under particular concrete identifications of the generic concepts, are of concern but the process in all its abstract generality (over the entire unlimited class of empirical cases that may realize the abstractly defined scope conditions). For instance, explaining differences in school achievement in the United States via path analysis is to be distinguished from formulating and confirming a theory of the reference group comparison process that may be only one process in the complex compound of mechanisms that affect school achievement. The path analysis is restricted to the historical circumstances built into its problem, whereas the reference group process has the potential for unlimited realization in diverse circumstances in diverse social and cultural conditions.

A more recent program statement may be found amid the collection of papers in *Expectation States Theory: A Theoretical Research Program* (Cambridge, Mass.: Winthrop, 1974), edited by Berger, Conner, and Fisek. Berger's introductory essay spells out the importance he attributes to scope-defined formulations. In the new book under review here, this idea is once again emphasized; in particular, chapter 3, "Scope and Method of the Investigation," is a useful compact statement of the general methodological position.

One other important statement of the metatheoretical ideas characterizing the paradigm may be found in *Types of Formalization in Small Group Research* (New York: Houghton Mifflin, 1962), written by Berger, Cohen, Zelditch, and a mathematician, J. Laurie Snell. The book offers three extended examples of formal models, each exemplifying a type of formalization according to the purpose of the model builder. In one case the aim is to explicate a concept that occurs in some informal theory. The example is the Cartwright-Harary formalization of the Heider concept of balance. A second type involves the aim of representing some single recurrent process in mathematical form. The example is Cohen's model for conformity in the Asch situation. A third type is given by the aim of mathematically representing the generic process or processes that generate a whole series of regularities within a given domain. This is called a theoretical construct model, and the example is the stimulus-sampling theory originated by William Estes to explain a variety of learning phenomena.

I believe that the idea of a theoretical construct model and the vivid

example provided in learning theory have been extremely important elements in the paradigm of the EST group. I would think that the stimulus element construct as it appears in stimulus-sampling theory in terms of the unobservable states of an underlying Markov chain is the origin of the EST group's effort (in some papers) to formulate expectation states as the state space of an unobservable Markov chain with behavior as a probabilistic function of these states. Yet, there are two methodologically important differences between stimulus-sampling theory and expectation states theory. First, stimulus-sampling theory retains the behavioristic approach of the stimulus-response background. It is silent on the vast internal information processing of the human mind, collapsing all mentality into "state of conditioning of the stimulus elements." The EST paradigm, on the other hand, is based on cognitive psychology. To be sure, it also grants little in the way of mental events, since expectation states are few and simple (at least in the earlier work; the second half of the present book presents a more complex picture of information processing activities). Second, stimulus-sampling theory is a genuine mathematical theory, with a framework of axioms within which an enormous variety of special models may be constructed and applied. It is a unified theory with high deductive capabilities within its own chosen mode of representation. On the other hand, EST still has quite limited deductive schemes: Assumptions and definitions are clearly set out, but the presentation is not mathematical. Again, in part 2 of *Status Characteristics and Social Interaction* an effort is made to push one theory toward a state in which some powerful deductions might be made. (I will omit details, but readers should beware of some very odd mathematics in relation to the estimation of parameters.) But there is some way to go before a satisfactory process-oriented theory is developed that is comparable to the methodological exemplar provided by stimulus-sampling theory. For one thing, stimulus-sampling theory is the matrix from which a variety of learning phenomena are explained. But the theory in this book tends to focus on one "dependent variable," the frequency of staying with a response given an opportunity to change it in a two-stage decision process. This quite limited formal target for prediction indicates a much narrower vision than seems to have guided the founders of modern mathematical psychology in its behavioral side.

It is very important to realize the difference between a theoretical construct model and a representational model. Most important, in my view, is whether or not the model of a process is derived from axioms that, under other abstractly described and formally represented circumstances, will constrain the model to other forms. In representational models (such as most extant Markov mobility models) the basic generator of the process (e.g., a transition matrix) is set up with an effort to provide a rationale for the particular form but without constraints forced upon the setup by a set of general axioms as applied to the given conditions. In the theoretical construct case, on the other hand, the generator may turn out to be very much more complex than one would like: it may be nonlinear, second order, or whatever. More important, certain parameters of the generator may be com-

plex functions of underlying theoretical parameters, as in the learning models in which the state-of-conditioning transition probabilities are derived functions of quantities implicitly defined in the axioms of the stimulus-sampling theory. Nor is this element captured by devices introduced into representational models by Coleman in his book, *Introduction to Mathematical Sociology* (New York: Free Press, 1964), in which parameters of the chain are expressed in regression-type equations as functions of external variables. In a theoretical construct model, these functions are internal to the theoretically derived model, forced upon the particular model by the assumptions made about the circumstances to which the general theory is being applied. This theoretical construct notion, it seems to me, dominates the more formal work of the EST group and also remains as an ideal even when the work fails to achieve the formal coherence and deductive elaboration I would like to see. The theorists know how the theoretical construct "expectation states" should be functioning even if, in their various efforts, reality and ideal fail to match. They are guided by a highly sophisticated methodological conscience. Using this standard, part 2 of the present book is a disappointment. Inexplicably, given their earlier work with Markov processes and the participation of one mathematician (Norman), the authors fail to even attempt to construct a dynamic model, whether of the theoretical construct or representational variety.

A good example of critical work is given in one part of Herbert Costner's review of the first volume of *Sociological Theories in Progress* (*American Sociological Review* 32, no. 1 [February 1967]: 128–29). Costner deplored the tendency, found in the "success" paper mentioned above, to substitute formal notation for formal model. In other words, notation is elaborated to discriminate various conceptual entities as they are introduced, but there is an absence of a mathematical framework to generate deductions. Costner also worries about the elaborate effort to do what he calls "a formalization of the familiar notion of 'halo effects.'" In a sense, Costner is saying that the paper provides merely an explicational model, at best, and not a theoretical construct model. In the present book, the authors seem sensitive to these criticisms. In part 1, they present the theory with virtually *no* notation whatever. In part 2, on the other hand, they attempt not only to extend the theory further but also to work it into connection with a graph-theoretic representation of the actor's cognitive state and to derive consequences. This is very much in the cognitive balance tradition, although the principles of balance theory are not explicitly used. "Structure completion" on signed graphs, however, is a central notion.

But criticisms of the formalization aspect of the EST work touch neither the core metatheoretical element of the paradigm, which is the notion of scope-defined formulations, nor the central working method of the group, which is the experimental verificational study. I turn now to a criticism of these aspects of EST as they are manifested in the new monograph.

First, is it possible that there are latent dysfunctions for theory construction in the employment of an experimental program of testing, refinement, and extension? Consider the scope condition, in relation to the group task,

that members possess a common cognition and shared evaluation of the outcomes. A scope condition of this sort may be satisfied initially in a situation, but then the status-organizing process may alter the initial consensus. A theoretician could hold three different orientations concerning a midsituational upsetting of consensual initial conditions: (1) If it occurs, it has no feedback effect on the status-organizing process and so is not critical to the situation; (2) there may be a feedback effect, but it is negligible; and (3) the feedback effect may not be negligible, but then the particular situation is outside the scope of the theory. With reference to the theory in all generality (i.e., for all situations within its scope), we could not know that (1) holds except by a theoretical analysis. The same is true of (2), even though for the Berger situation both (1) and (2) may be true. In practice, experimenters might resort to (3), simply discarding subjects who fail to meet scope conditions. Thus, only situations satisfying (1) or (2) are in the actual scope of the theory because in the wider class of situations, phenomena set off by the status-organizing process and with return influence on that process cannot be excluded by theoretical fiat. In the example given above, if commitment to group goals varies with position in the power and prestige order, the emergent order can give rise to differential loss of initially uniform commitment, and this differential loss of commitment can give rise to additional aspects of the status-organizing process (e.g., the emergence of a quite overt struggle for power).

Second, is it possible that this is a theory, not of social interaction, but of social cognition? The graphs represent actors' internal representations of the situation. Expectation states are in the representation, and the theorist calculates an "expectation advantage" where the scope conditions appear to allow what I find a perplexing type of deduction. Namely, without any formal representation at all of the back-and-forth ("A says X, then B does Y, then A does Z . . .") nature of interaction in task-oriented groups, the theory implies something about the consequences of interaction. It glides over an ocean of interactive events which it ignores, landing on a far shore of equilibrium in which a power and prestige order exists because of the initial conditions on the starting side. The actor's image of the initial conditions generates a cognitive outcome that then generates his position in the equilibrium power and prestige order: This is the essence of the "general theory" in part 2 of this book. Stated in this manner, is it not clear that *social* process is not under analysis? In fairness to the authors, let me say that I assume that a theory which aims to explain a certain property of interaction should do so by explicit derivation of the interactional sequences that simply display the property. This is extremely difficult. The authors are apparently trying to do it in two stages, beginning with a kind of rough equilibrium model. This seems to be the strategy of the earlier success paper, with its follow-up by process models. But the earlier process models have not been consolidated into the present general framework. (In fact, a detailed criticism of the model in part 2 would have to note the apparent inconsistency with the earlier Markov process models by virtue of its tacit assumption of stationarity in the trial-by-trial influence probabilities.) The

result is that the authors' use of models bears a closer resemblance to repre sentational model building than theoretical construct model building b virtue of their commitment to an essentially verbal theory which is sporadi cally and almost noncommittally coordinated to various models.

The scientific sophistication that exists within the EST group assures tha these criticisms, if they have any merit, will be addressed and perhaps re solved. But what will be the fate of this theory program in sociology? Th precise future of EST depends on whether the group makes its final trans formation from cluster stage to incorporation into a redefined normal stat of sociology. The students are there, some of them very productive, paper continue to be published by a variety of journals, and in recent years w have seen several monographs. Apparently, the group is thriving.

Who Should Get What? Fairness Judgments of the Distribution of Earnings[1]

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Previous traditions of distributive-justice research assume the existence of a normative structure for making judgments about fairness of allocations of social goods, such as earnings. Does a consensual normative framework for judging the fairness of distributions of earnings exist in the U.S. population? What principles underlie popular judgments concerning earnings distributions? Data indicate both that judgments of earnings fairness are not idiosyncratic and that they involve individual and group differences related to considerations of merit and need. Some tolerance for variation in earnings among households is noted, and the same factors accounting for earnings-fairness judgments justify earnings considered fair. Considerable agreement exists concerning what principles are relevant to earnings-fairness judgments, while disagreement concerning how to apply these standards in practice is admitted. Apparently the standards for earnings judgments derive both from conceptions of the empirical distribution of earnings and from underlying values concerning what is fair and just.

"Who gets what?" is the central question in studies of income distribution (Kuznets 1953; Miller 1971). The issue of "why" is addressed in status- and income-attainment studies (Becker 1964; Duncan, Featherman, and Duncan 1972, Sewell and Hauser 1975; Jencks et al. 1972). In contrast, the present study is concerned with who *should* get what and is therefore an exercise in the empirical study of distributive justice. Using empirical data we test for the existence of a normative basis for making such judgments. We provide evidence that such a framework exists in the American

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population and that there are, therefore, general, widely agreed upon principles underlying popular judgments concerning the equity of distributions of earnings.

Distributive-justice issues are treated in two somewhat disparate literatures. On one hand, political, legal, and moral philosophers have contributed insightful essays on the "nature" of justice, providing many useful distinctions and conceptual clarifications (Friedrich and Chapman 1963; Brandt 1962; Rawls 1971). On the other, experimental social psychologists have dealt with feelings of equity, typically experimentally manipulating situations in an effort to understand what produces in subjects feelings of "justice" or "injustice" (Berger et al. 1972; Berkowitz and Walster 1976). The latter tradition has developed a body of propositions called "equity theory" concerning the conditions under which subjects will experience "injustice."

Both the philosophical and the social psychological equity-theory literatures assume the existence of a widely adhered to set of norms that state the principles underlying common judgments concerning what are fair and unfair divisions of resources, as, for example, in judgments of differentials in tax or wage rates. The present article does not take the existence of a normative structure as a given; instead it attempts to examine whether there is any empirical evidence for its existence. It is also an extension of a pilot study (Jasso and Rossi 1976) conducted in Baltimore to the more meaningful universe of the total U.S. adult population.

THE NATURE OF DISTRIBUTIVE JUSTICE

Three common themes run through the philosophical treatments of distributive justice. Most writers agree that a society's conception of justice is normative, entailing approval of what is "just" and stating an obligation to follow just courses of action (see Bird 1967, pp. 10–41). Principles of distributive justice specify appropriate rights and duties in basic social institutions and define the appropriate distributions of the benefits and burdens of social cooperation and hence provide the bases for judgments as to fairness and justice in the unequal distribution of valued goods and benefits (Rawls 1963, p. 99; 1971, p. 5–6).

The essential problem of distributive justice starts from the observation that some degree of inequality in the distribution of social resources is regarded as just and fair in every society and goes on to raise the question of how these inequalities are justified in a society. It is also abundantly clear that the justificatory principles involve the recognition that some individual and group differences should be recognized as justifying differences in shares (Boulding 1958, 1962; Chapman 1963; Rawls 1971; Vlastos 1962).

Since not all dissimilarities can be relevant, the critical questions become, What are the criteria used in justifying inequalities, and how are the different criteria weighted when several of them conflict in a given case?

Boulding (1962) has suggested that two very general principles underlie distributive-justice judgments. On one hand, a "principle of disalienation" states that no person or unit in a society shall be left without claim on resources, meaning that no one should be alienated from the society. On the other, a "principle of desert" asserts that persons should get what they deserve, a principle that recognizes as a major source of inequality the unequal contributions of units to the major concerns of the society. The first principle stresses that the distribution of resources ought to pay attention to some measure of a unit's need; the second stresses criteria of merit. These two criteria can be in conflict; indeed, stress on one or the other principle lies at the bases of the liberal and conservative views of our own society.

Boulding suggests that the potential conflicts between the principles of disalienation and desert are reconciled in the creation of a conception of a "social minimum," a level of resources below which no unit is allowed to fall, while considerations of desert come into play as justification for shares that are larger than the minimum: ". . . Society lays a modest table at which all can sup and a high table at which the deserving can feast" (Boulding 1962, p. 83).

Although most of the writers on distributive justice provide ample justification for a view that such conceptions are fraught with enough ambiguity to allow individuals and groups to develop their own particular views of fairness, most subscribe to the expectation that in any society there is some degree of agreement over what is fair and just.[2] This does not mean that there is no disagreement over how these principles are to be applied in practice. Rather, a minimum criterion of an adequate theory of distributive justice is the existence of consensus with regard to those general principles or precepts: ". . . The fact that people accept the same general rule of distributive justice need not mean that they will always agree on what is a fair distribution of reward between them. Even if they concede that reward should be proportional to investment and contribution, they may still differ in their views of what legitimately constitutes investment, contribution, and reward, and how persons and groups are to be ranked on these dimensions" (Homans 1974, p. 250). What is essential is that some rule of distributive justice exists. It may not be the most noble of such rules, but it is what many will regard as fair (Homans 1974, p. 249).

[2] Only a few draw the inference that such judgments are largely idiosyncratic (Walster Berscheid, and Walster 1976), an inference that we believe renders the conception of distributive justice a trivial one.

METHOD

To pursue these issues on an empirical level we turn to judgments made by a representative sample of American adults concerning the fairness of earnings, as attributed arbitrarily to fictional individuals and households described in brief vignettes in terms of a few key characteristics. The questions we will raise concerning these judgments are: (1) What characteristics of households and individuals enter into the judgments of their earnings as fair or unfair? (2) What weights are given to characteristics in arriving at such judgments? and (3) How much consensus is there about the criteria and the weights to be applied?

The ways in which households differ one from the other are myriad, if not infinite. We cannot be concerned with all such potential criteria, and our choice among criteria should be guided by appropriate models of status and income attainment as well as by considerations from the conceptions of distributive justice outlined above. Thus, we settled upon occupation and education as both measures of "merit" and important features of models of income attainment and used marital status and number of children as measures of "need." Because sex and ethnicity play such an important role in stratification theory, we included these variables as well.

The earnings shown for the "vignettes," or computer-generated descriptions of households, were rated by our sample along a nine-point scale described below. The choice of earnings as the social resource or good to be judged was dictated by the consideration that earnings are the major source of income for most people and by the fact that many discussions of distributive justice center on the distribution of income. In addition, unlike many other social goods, earned income has a desirable property: It is characterized by a common, easily understood metric, the dollar.

The data were collected through personal interviews with a national probability sample of 522 adult respondents residing in private households By setting sex quotas within households, half of the interviews were obtained from adult men and half from adult women. The interview consisted of two tasks: Each respondent rated a sample of 50 vignettes (described in more detail below) and provided standard background and demographic information about himself and his household.[3]

[3] It should be noted that each respondent received a separately drawn sample of vignettes These samples were drawn in an unbiased manner so that each respondent received a sample that differed only within random sampling error limits from that of any other respondent. While there might have been much to gain from standardizing the sample of vignettes, presenting to each respondent the same set, at the same time we would have lost the ability to generalize to the universe of all possible vignettes. Since in this particular instance we are more interested in the ways in which the characteristics of vignette households affect the fairness ratings in combination, we felt that our strategy was a reasonable way to settle a trade-off problem. It should also be noted that certain other benefits follow: First, it was difficult for respondents to

The basic task set before each respondent was to judge how fairly paid were each of the households described in a sample of 50 vignettes, each printed on an IBM card and containing selected information describing a household. Two types of households were described: ones consisting of one person and ones consisting of husbands and wives.

The computer program that wrote out each card picked out a value for each characteristic at random (within certain limits described below). The characteristics were: (1) educational attainment—in years for each adult in household; (2) occupation—51 different occupational titles chosen from among titles for which occupational prestige scores were available and picked with probabilities roughly proportional to appearance of corresponding census category in civilian labor force (score range was 8.2–81.5; 25% of the husband-wife households had wife's occupation designated as "housewife"; shown for each adult in household); (3) sex—used in one-person households only (each person in married-couple households designated as either husband or wife); (4) ethnicity—national and racial origins specified for entire household, using groups for which NORC prestige scores were available; (5) marital status—used in one-person households only (single, married, widowed, divorced); (6) number of children—ranging from zero to six for husband-wife households and for one-person households designated as widowed or divorced; (7) gross annual earnings—designated for each employed individual in a household, ranging from $0 (housewife's earnings) to $40,000 (for a husband-wife household earnings were shown separately and combined); (8) net annual earnings—gross annual earnings taxed at various rates ranging from 0% to 35% (shown separately for each employed person and also combined for husband-wife households).

A small number of constraints were placed on the association among household-member characteristics. Extremely anomalous combinations of education and occupation (e.g., physicians with less than college education) were prohibited, and the differences in occupational prestige scores between husbands and wives were restricted to 50 points or less on the NORC scale. As we shall show, these restrictions produced slight correlations among the household characteristics, but considerably lower ones than ordinarily found in the "real world."

We also fixed randomly wife's occupation in husband-wife households as "housewife" in 25% of such households. In addition, all households had a 10% chance of having their ethnicity listed as "Black-American."

It is important to note that the universe of vignettes from which each

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develop response sets since particular household-member characteristics appeared infrequently in any vignette sample. Second, the samples being drawn in an unbiased fashion had the same statistical characteristics as the universe. Finally, we were able to use more household characteristics and more values within each characteristic than had we used a single sample of vignettes for all respondents

respondent's sample of vignettes was drawn had important statistical characteristics. First, except for occupation each variable was almost rectangularly distributed and hence had larger than ordinary variances. Second, the characteristics were minimally correlated among themselves, reducing problems associated with multicollinearity and allowing better estimates of the effects of household characteristics on the fairness ratings. Indeed, the advantages are those of fully crossed experimental designs.

The rating task given to respondents was to judge the relative fairness of the earnings shown in each vignette in nine degrees ranging from "extreme underpayment" to "extreme overpayment" running through a midpoint of "fairly paid."[4] For purposes of analysis each of the categories was given a score ranging from 1 for the "extremely overpaid" through 5 for "fairly paid" through 9 for "extremely underpaid."

We have chosen to regard these judgments as representing an interval scale, admittedly an almost arbitrary assessment.[5] It is important to note that a positive increment in scale value represents a shift in judgment in the direction of underpayment, while a negative increment indicates the opposite.[6]

FINDINGS

Despite the fact that earnings were randomly associated with household characteristics, respondents' modal ratings were in the "fairly paid" cate-

[4] The wording of the item used was: "Wages and salaries that people get for the jobs they do vary a lot. We have made up descriptions of different kinds of people holding down different kinds of jobs. Here is a box with nine slots in it Here is a card with a description of a family or a person with the jobs and salaries or wages that they earn Please place this card in the slot in the middle labeled 'fairly paid' if you think that the money earned by the person or the family described on that card is *fair payment* Put the card in the slot labeled 'extremely overpaid' if you think that the money is *way too much payment* for the person or the family: that is, if taking everything into account, you think they are very much overpaid. Put the card in the slot at the bottom labeled 'extremely underpaid' if you think that the person or family is *very much underpaid.* Use the other slots in the box for the card if you think that they are being *slightly* or *moderately underpaid* or *slightly* or *moderately overpaid.* There are no right or wrong answers: we want your personal judgment about the wages and salaries earned." The sorting box used by respondents contained nine slots, marked as follows "Extremely overpaid," "much overpaid," "somewhat overpaid," "slightly overpaid," "fairly paid," "slightly underpaid," "somewhat underpaid," "much underpaid," and "extremely underpaid " Each of these labels was also numbered from one to nine. When the rating task was completed by the respondent, interviewers placed each of the stacks of cards sorted into each of the slots into separate envelopes, appropriately marked

[5] Several transformations were tried on the ratings, including squaring the ratings, arbitrary "stretching" of scores toward the two extremes, and logarithmic transformations None of the transformations improved analysis results, and all led to lowered amounts of variance explained.

[6] An alternative treatment would regard high scores and low scores as "unfairness" ratings with midpoint scores regarded as "fairness." Folding over the rating scale in that fashion was not justified in the light of an analysis indicating that no sensible results could be obtained with the scale so transformed.

TABLE 1

INCOME-FAIRNESS RATINGS BY TYPE OF HOUSEHOLD

Income-Fairness Rating	One-Person Households	Full-Family Households	All Households
1. Extremely overpaid (%)	6 8	8 1	8.0
2. Much overpaid (%)	5.2	7.8	7 6
3. Somewhat overpaid (%)	7 4	9 8	9 5
4. Slightly overpaid (%)	9 5	10 9	10.8
5. Fairly paid (%)	27 8	28 4	28.4
6. Slightly underpaid (%)	12 1	10 8	10 9
7. Somewhat underpaid (%)	10 3	9.0	9.1
8. Much underpaid (%)	8 7	7 2	7.3
9. Extremely underpaid (%)	12 2	8 0	8 4
$\bar{X}$	5 40	4.96	5 00
Ratings (N)	2,652	23,429	26,081
% rated	98.53	97 63	97 72

gory, as the marginal distributions of ratings shown in table 1 indicat The American population shows some degree of tolerance for variations i earnings, 28% of the vignettes being rated as "fairly paid." Husband-wi vignettes are slightly more likely to be regarded as "overpaid" ($\bar{X} = 4.96$ than one-person households ($\bar{X} = 5.40$). Of course, since in a majority c the husband-wife households both husband and wife were given earning this result may simply reflect the fact that such households had earnin nearly twice as high, on the average, as one-person households.

The distributions of table 1 are compatible with a variety of normati structures, except those that maintain that any and all earnings values a morally acceptable. Obviously respondents made differentiations in the judgments of underpayment and overpayment. Furthermore, despite t fact that earnings were rectangularly and randomly distributed to vignette the ratings are not rectangularly distributed and, as we will show, are n randomly distributed, either.

In the regression analyses that follow, we have chosen to represent eac of the occupational titles by the NORC prestige score for that title. Pe haps the most compelling reason to do so is that these scores provide a overall "goodness" metric that has been found useful in other studies. should be noted that in the real world occupations are not unconfound with educational attainment and income. However, the correlations wi income and education are also far from perfect, ranging between .4 and . indicating that many persons are off the regression lines implied.[7] Furthe

[7] To test whether the occupational prestige scores capture the effects of occupation, entered each occupation (save one) as a dummy variable and regressed ratings on t resulting dummies. The resulting R^2 was essentially identical to the R^2 resulting fro using the prestige scores as the independent variable Furthermore, the regression c efficients for each occupation treated as a dummy correlate .95 with the prestige sco for the same occupations.

more, by specifying a correlation of approximately .2 between education and occupation in our "vignette world," we have loosened considerably the connection between these two variables. Of course, the specified zero correlation between income and education or occupation is also a loosening of the connection between the variables involved. Similar reasoning applies to the use of NORC prestige scores for ethnicities.

Table 2 presents the regression of fairness ratings on vignette characteristics for one-person vignettes. The zero-order correlations, means, and standard deviations for all variables included in the regression are shown in table 2A. Note that the correlations among the first 10 variables are design outcomes and hence hardly findings. The success of the randomization scheme built into the vignette-writing program is shown in the intercorrelations among the first six variables, all of which are small, except for those variables upon which constraints were placed. Column 11 of table 2A, which presents correlations with the fairness ratings, contains empirical findings, foreshadowing the regression analysis.

Four of the variables (7–10) have been derived from combinations of vignette characteristics. An interaction term was formed involving earnings and occupation, guided by the expectation that special attention would be given to unusual combinations of those two variables. The square of earnings was entered since there are good theoretical reasons to expect diminishing utility of earnings on the margins. Since net earnings were also shown on the vignettes, we computed the ratio of such earnings to gross earnings, expressing the tax rate implicitly attached to each vignette. Other interaction terms among vignette characteristics were also formed and tried out, only those presented here achieved statistical and substantive significance.

Table 2B displays the resulting regression statistics. The reader should note that the immediately following analyses focus on central tendencies in earnings-fairness judgments. In a later section we address the issue of consensus in earnings-fairness judgments and the extent to which there are systematic departures from such central tendencies which are reflected in differences in the structure of fairness judgments for various subgroups of respondents. It is clear that the respondents were paying some attention to almost all of the characteristics that were built into the vignettes: Only sex did not achieve a statistically significant regression coefficient.[8] The

[8] The statistical significance tests applied to this regression analysis assume that we have as many degrees of freedom as there are vignettes that were rated. However, since the vignettes were rated by 522 respondents, they are not independent to the extent that there is some degree of intraperson correlation among ratings. In order to test the effects of the intraperson correlations, three separate samples consisting of one vignette from each respondent were drawn. Regressions were computed over these samples and results compared with those in tables 2 and 3. By and large the intrapersonal correlations yielded regression coefficients larger than those shown in tables 2 and 3, with R^2s that were essentially the same as shown in those tables Furthermore, averaging the

TABLE 2

REGRESSION OF EARNINGS-FAIRNESS RATINGS ON VIGNETTE CHARACTERISTICS OF ONE-PERSON HOUSEHOLDS

A DESCRIPTIVE STATISTICS

	1	2	3	4	5	6	7	8	9	10	11	X	SD
1. Education		− 028	− 003	000	− 034	311	142	004	− 006	− 025	127	6.5	3.3
2. Children (N)[a]			024	017	− 021	− 002	001	021	000	.001	124	2 3	2.8
3. Sex				− 016	059	000	− 008	− 016	− 008	− 015	− 005	1 5	.5
4. Gross earnings					016	− 006	834	975	.074	008	642	156 1	110.2
5. Ethnicity						− 019	012	022	− 017	021	011	43 3	13.4
6 Occupation (NORC)							448	− 003	008	− 337	207	43 0	16 1
7. Earnings-occupation interaction								814	069	− 148	− 427	6,707	5,675
8. Earnings squared									049	004	579	36,511	266,633
9. Tax rate										− 019	− 027	85	12
10. Dummy for welfare status											− 135	024	.152
11 Fairness rating												5 4	2.2

B REGRESSION STATISTICS

	b	b^*	t
1 Education (years)	05349	07897	5 6
2. Children (N)[a].	11152	13638	10 1
3. Sex (M = 1, F = 2)	− 10155	− 02257	−1 7
4. Gross earnings ($ hundreds)	− 03520	−1 72412	−24 3
5. Ethnicity[b]	00458	02729	2 0
6. Occupation (NORC score)[c]	00995	07124	2 9
7. Earnings-occupation interaction	00007	17855	4 2
8. Earnings squared[d]	00452	95158	15 5
9. Tax rate[e].	78535	04032	2 9
10. Dummy for welfare status[f]	−1 07653	− 07290	−5.1

NOTE.—R^2 = 530, F = 293 9, N = 2,613, intercept = 7 6

[a] Number of children scored 0 = single person, 1 = married, no children, 2–7 = number of children

[b] Ethnicity scored in terms of prestige scores derived from NORC unpublished data.

[c] Occupational prestige measured in terms of NORC unpublished scores

[d] Earnings squared are gross earnings squared multiplied by 01

[e] Tax rate is one minus net income shown in vignette as a ratio of gross income

critical aspect of these results is, of course, that the respondents did not pay equal attention to each of the vignette descriptors. Although the standardized regression coefficients (b^*) are useful only as comparisons among variables in this analysis (since they have been standardized by design-imposed variances), we can use them as indicators of the relative importance placed by respondents on the vignette characteristics.

It is clear that the ratings are most sensitive to earnings, indicating that there are strains toward judging the highest earnings as overpaid despite the other qualifications—occupation, education, etc.—of the individual described in the vignette, and similar strains toward judgments of underpayment for persons given very low earnings, regardless of other characteristics of the person. These findings hint that Boulding's (1962) speculations concerning a social minimum are reflected in these judgments. Apparently, low income appears to be unfair underpayment despite whatever levels of merit the individual displays in his occupation or educational attainments. But the findings also hint that there are social maxima, earnings which are so high that they cannot be justified by accomplishments of an educational or occupational variety. We will attempt later in this paper to compute the social minimum and social maximum implied by these results.

There is also some support for the economists' notion of diminishing marginal utility of income. The squared term for earnings is significant and carries a positive sign indicating that equal increments in earnings do not get equal increments of ratings (in the direction of overpayment). Respondents are apparently more sensitive to earnings shifts in the lower ranges studied than in the upper ranges.[9]

Earnings attributed to the vignette persons were presented in two forms, as gross and as net incomes, the difference between the two being generated by an implicit tax rate varying randomly from 5% to 35%. Respondents appear to have been sensitive to the implicit tax rates, as the coefficient for

regression coefficients over the three samples, we found that the averages correlated .99 with the coefficients computed in tables 2 and 3. The t-statistics for the regression statistics computed from the three subsamples tended to be higher than one would expect on the basis of a reduction in sample size from vignettes to persons, indicating that the t-statistics shown in tables 2 and 3 are conservatively stated. In short, the intrapersonal correlations acted to produce a downward bias in the regression coefficients of tables 2 and 3 but did not upset the size orderings of the coefficients or the amount of variance explained. Hence, the analyses shown in the tables tend to be conservative statements of the effects of vignette characteristics on ratings.

[9] One of the reviewers of an earlier version of this article suggested that a more appropriate model to be applied is a multiplicative one in which the log of the ratings would be regressed on the log of the other variables in the equation of tables 2 and 3. We have rejected this approach for two reasons. First, there does not seem to be any theoretical reason for a multiplicative model. Second, a multiplicative model, when tried, was a less close fit (as indexed by a reduction in R^2) than the additive linear models used in tables 2 and 3.

that variable indicates—or perhaps it would be more accurate to say tha they took into account net as well as gross earnings in deciding whether t consider a vignette person as fairly paid.

"Merit" considerations appear to be important in the judgments. Occu pational and educational attainment affect ratings in the expected directio both independently and through the interaction term formed between them All told, the higher the educational and occupational attainments, the mor ratings are moved in the underpayment direction. Furthermore, high occu pation coupled with high education yields extra increments in the sam direction. In short, high occupational and educational attainment is seen a justifying a judgment tending in the direction of underpayment, regardles of the level of earnings involved.[10] Given the ranges of these two variable and their associated coefficients, it is clear that occupation and educatio dominate other characteristics, save earnings, in influencing distributive justice judgments.

The only measures of need included in table 2 are marital status an number of children. Married persons and persons with children tend to b rated more on the underpaid side, compared with persons with similar othe characteristics who are not married or who do not have children. In shor "need" also plays a role in the judgments.

The remaining characteristics are status ones, of which only being o welfare makes a difference in judgments.[11] No matter what earnings ar attributed or what other characteristics are shown for a person, character izing that person as being on welfare leads to a judgment that is on th average more than one point higher in the direction of overpayment. Se and ethnicity, surprisingly, do not get coefficients which approach statis tical significance although the signs of the coefficients are in the expecte direction: Females and persons of lower ethnicity are given scores that ar higher in the direction of overpayment.

Finally, we draw the reader's attention to the relatively high R^2 for th entire equation, .53. About half the variation in ratings from vignette t vignette is accounted for by these vignette characteristics. Some of th residual, unexplained variance represents "error" in the sense that there i respondent variability in rating the same vignette, that is, unreliability i

[10] Because of the inclusion of the interaction term it is difficult to interpret the regre sion coefficients estimated for occupation and education, since by definition, each c the constituent terms is highly correlated with the interaction term. However, whe occupation and education are entered into regressions without the interaction term, th b^*s for these two variables are larger than any others, save earnings, indicating th extent to which merit considerations dominate the judgments.

[11] For about 2% of the vignettes the phrase "being on welfare" was printed instead o an occupation. We can regard "being on welfare" as an occupation or as a status cha acteristic, there being good reasons for either treatment. It was our judgment that was more appropriate to regard this as a status characteristic.

response. Of course, some of the unexplained variance may also represent differences among respondents in rating principles implicitly used. At this point, we are unable to distinguish between the two sources of variance, although later in this paper we make an attempt to explore this issue in greater detail.

Since the one-person vignettes are composed of single and formerly married persons (widowed or divorced) and equally of males and females, we checked the possibility that the structures of regression equations might be different from one type of vignette person to another. Separate regression equations were run for males and females, and for single and formerly married persons. To conserve space the resulting equations will not be shown here; however, regression equations were essentially alike across vignette-person types with only a slight hint of a larger regression weight for educational attainment for women than for men, possibly indicating that a woman's educational attainment is more of a justification for her earnings.

We turn now to consider vignettes describing households consisting of married couples. Husband-wife vignettes were considerably more complicated than one-person vignettes since education, occupation, and earnings were shown separately for husbands and wives along with the combined earnings of the household for a total of 12 items of information describing each vignette household. Accordingly, husband-wife vignettes were more difficult to judge (as we have seen in the results in table 1). More "error" in judgment, as reflected in a lower amount of variance explained, can be expected since respondents are likely to find it more difficult to "take into account" 12 items of information than eight. We can also anticipate that there will be more interaction among vignette characteristics, especially between husband's and wife's attributes.

The regression of earnings-fairness ratings on vignette characteristics is shown in table 3. As expected, R^2 is somewhat lower than in table 2 (.45 as compared to .52). The vignette characteristics that helped to explain the ratings of one-person households were also useful in explaining the judgments of husband-wife households. Earnings, of course, are the major explainer of fairness judgments, but husband's and wife's earnings do not contribute equally. Respondents are apparently more sensitive to husbands' earnings than to those of wives. The regression coefficients for husbands' earnings and for the square of husbands' earnings are 1.4 and 1.9 times greater, respectively, than the corresponding coefficients for wives. The same differential sensitivity is shown with respect to educational attainment: The ratio of the coefficient for husbands' educational attainment to wives' is 1.6. Thus a shift in husband's income and educational attainment

TABLE 3

REGRESSION OF EARNINGS-FAIRNESS RATINGS ON CHARACTERISTICS OF HUSBAND-WIFE VIGNETTES

Independent Variable[a]	*b*	*b**	*t*
1. Husband's education (years)	02858	.04339	8 3
2. Wife's education (years)...	.01724	02563	5.0
3 No. children (0–6)	07564	.07812	15.9
4. Husband's gross earnings ($ hundreds)	− 02176	−.08325	−41 4
5. Wife's gross earnings ($ hundreds)	− 01558	− 82385	−41 5
6. Ethnicity	.00033	00199	.4
7. Husband's occupation	01092	07991	9 0
8. Wife's occupation	.01622	.11142	12.8
9 Husband's earnings squared	00209	.44714	20 1
10. Wife's earnings squared...	00110	21047	11.9
11. Husband and wife's earnings interaction	00003	29711	30 1
12. Husband earnings-occupation interaction	00002	.04858	3 1
13 Wife earnings-occupation interaction	− 00001	− 03064	−1 8
14. Dummy for welfare status	− 69582	− 04281	−8 3

NOTE —Intercept = 6.85, R^2 = 453, N = 22,874, F = 1,350 3 Zero-order correlations and distributional statistics have not been shown here to conserve space The same patterning of correlation coefficients as in table 2 obtains for this analysis, since most of the correlations involved are products of design characteristics

[a] See note to table 2 for explanation of some variables

of a given size and direction makes more of a difference in the judgment ratings than corresponding shifts for wives.[12]

Although many possible interaction terms can be formed between husbands' and wives' characteristics, only two survived statistical tests. The husband and wife earnings interaction indicates that, as husband's and wife's earnings both increase, there is a decelerated movement in the direction of overpayment. In short, the combined earnings of husband and wife show the same tendency toward diminishing marginal utility. The two interaction terms involving husbands' and wives' earnings and occupations are small and substantively unimportant, with the wives' interaction term not quite reaching statistical significance.

The findings concerning husband and wife vignettes suggest several general processes at work. First, although respondents were sensitive to both husbands' and wives' characteristics, they were more sensitive to the former than the latter, indicating that husbands' "meritorious" attainments justify a given level of household income more readily than those of wives. Second, double-earning households are apparently judged entitled to somewhat less than twice the earnings of single-person households. Third, we find the same suggestions of both social minima and social maxima at work for family households as for one-person households. Finally, we may note again

[12] Note that the sum of coefficients for husband's and wife's education does not equal the coefficient for education for one-person households, indicating that husband's and wife's educations are being weighted separately.

that status characteristics such as ethnicity have a negligible impact. Earnings-fairness ratings are sensitive mainly to income, indicators of "merit" (occupational and educational attainments), and indicators of "need" (number of children).

The Dollar Value of Equity

A more dramatic presentation of the findings can be obtained by regressing the earnings shown in the vignettes on the characteristics presented in the vignettes, holding fairness ratings constant. Once again we point out that the present analysis (and the one that will follow regarding a fair distribution of earnings) deals with central tendencies in our data, and we reserve consideration of respondent subgroup differences to a later section This form of analysis, shown in table 4, yields the dollar increment (or decrement) *seen as equally fair* for each unit of the characteristic in question. Thus a college graduate (16 years of education) compared with a high school graduate (12 years of education) in one-person households that are alike in other respects can earn \$779 more and achieve the same earnings-fairness ratings since each year of education "entitles" a person to receive \$194.64 in earnings. The *b*-coefficients of table 4 are interpretable

TABLE 4

REGRESSION OF TOTAL EARNINGS IN DOLLARS ON VIGNETTE CHARACTERISTICS

	b	*b**	*t*
Husband-wife households:			
Husband's NORC score	62 28	062	11 6
Wife's NORC score	52 42	049	8 7
Husband's education (years)	138 78	029	5 7
Wife's education (years)	62 73	013	2 5
Children (*N*)	325 44	046	9 6
Ethnicity score	−5 78	− 005	−1 0
Housewife dummy	−9,954 39	− 266	−47 4
Welfare dummy	−2,376 80	− 020	−3 9
Vignette rating	−4,148 25	− 569	−113 06
Intercept	43,192 00		
One-person households:			
NORC score	70 01	102	6 2
Education (years)	194 64	056	3 8
Children (*N*)	423.37	106	7 2
Ethnicity score	27 79	034	2 3
Sex	−541 10	−.025	−1 7
Welfare dummy	−3,551 59	−.049	−3 1
Tax rate	51 51	053	3 7
Vignette rating	−3,376 90	− 689	−45 7
Intercept	27,563.00		

NOTE —For husband-wife households, R^2 = 471, F = 2,259.84, and N = 22,864; for one-person households, R^2 = .448, F = 264.6, and N = 2,613.

as the earnings sensitivity of each of the characteristics, holding other characteristics and the fairness ratings constant.

Since the husband-wife vignettes and the one-person vignettes yield dollar estimates that are quite similar, we will discuss in the text only those coefficients that have been computed for the husband-wife vignettes. First, it is interesting to note that each unit of the fairness rating implies $4,148: With everything else held constant, an increment of that amount in the household's earnings shifts the fairness rating one point in the direction of being overpaid.[13] Since the fairness ratings have a range of nine points, the difference between vignettes (holding everything else constant) that are rated as extremely overpaid and extremely underpaid is about $37,000.

Each unit of the prestige scores for husbands' and wives' occupations justifies, respectively, $62 and $52. Given that the occupations used in the vignettes had a range of about 90 prestige-score points, the justified earnings differential for the occupational range is about $5,600 for husbands and about $4,700 for wives, indicating that merit for males in the form of occupational attainment is given greater recognition in equity judgments. A similar differential holds for educational attainments, each year of education being worth $139 and $63, respectively, for husbands and wives, or $1,390 and $630 over the range of 10 years of educational attainment differentials shown in the vignettes.

Each child that a couple has justifies an additional $325 of income or $1,950 over the range of zero to six children. Note that while this finding indicates that respondents take into account this indicator of "need," they are not as generous as the Internal Revenue Service, which currently allows an income exemption of $1,500 for each dependent.

The coefficient for housewife indicates that, holding everything else constant, a family in which the wife is a housewife loses about $9,954 of justified income.[14] Or, put the other way around, a couple in which the wife is working can earn almost $10,000 more than one in which the wife is a housewife and still get the same income-fairness rating. In short, respondents recognize as "just" that households with two earners are entitled to have a combined income that is much more than fair for a single-earner household. In this sense, merit in the form of occupational achievements of both earners in a household is being recognized in the earnings-fairness ratings.

[13] Actually, the coefficient puts things the other way around, i.e., a shift in the fairness rating in the direction of underpayment implies a shift downward in income of $4,148

[14] Housewives were given an occupational prestige rating of 28.6, a value derived from a previous study (Rossi et al 1974) Wives designated as housewives were also coded as a dummy variable Hence, the dollar value (b-coefficient) for housewife is independent of the prestige ranking as an occupation

A Fair Distribution of Earnings

Justification of inequality in the distribution of social goods on the basis of competing claims for distributive shares is the essential problem of distributive-justice judgments. Thus far we have seen that the normative structure of earnings-fairness judgments in the United States involves balancing competing claims of merit and need. Efforts to avoid conflict when simultaneously applying both principles result in the establishment of "social minimum" earnings: some amount of earnings under which no household or person should fall. Implicit in the concept of social minimum earnings is the idea that no matter how "badly" one fares in life, especially in regard to those criteria deemed relevant for allocation of social goods, society should make provisions to assure a minimal subsistence. As a corollary, it seems reasonable to us to ask whether there might also be a "social maximum," some amount of earnings over which no household or person should earn. It is clear, though perhaps not intuitively obvious, that both social minimum and social maximum earnings are limiting cases: They represent the amounts of earnings the least qualified and the least needful individual or household (social minimum) and the most qualified and most needful individual or household (social maximum) can earn and be judged "fairly paid."

Estimates of fair earnings were derived by computing the expected gross earnings for households with various characteristics using essentially the equations presented in tables 2 and 3.[15] We set the equations equal to 5 (a judgment of fair payment) and specify all characteristics except earnings for persons and households of given characteristics who are judged to be fairly paid, solving for earnings as the unknown.

In table 5 we present a series of estimates that are designed to illustrate the implicit social minima and social maxima contained in the findings presented so far. We have computed the fairly paid earnings for persons and households on welfare, for the "working poor," and for "average" persons or households in our vignette universe and the fairly paid earnings for the persons and households with the "best" occupations, educational attainment levels, and so on. It should be noted that these earnings are estimates implied by the analyses performed on the vignettes and are not generated by asking directly what is an acceptable minimum and maximum set of earnings.

The computed lower and upper boundaries of fair earnings for single persons are $2,538 and $34,485, respectively, and for husband-wife households $7,211 and $44,466 when we use the welfare households as the lower

[15] The actual equations used omitted the squared and interaction terms in order to facilitate computation.

TABLE 5

ESTIMATED EARNINGS FOR FAIRLY-PAID WELFARE POOR, WORKING POOR, AVERAGE HOUSEHOLDS, AND BEST POSSIBLE HOUSEHOLDS*
($)

	Welfare Poor†	Working Poor‡	Average§	Best Off‖
One-person households	2,538	11,346	19,900	34,485
Husband-wife households	7,211	13,655	26,122	44,466

* Estimates computed by setting completely linear versions of the equations shown in tables 2 and 3 equal to 5 (or a rating of "fairly paid"), setting vignette characteristics as indicated, and solving for the earnings implied

† Persons or households living on welfare with no children, lowest level of education, and lowest rated ethnicity (black). Wives shown as "housewife" in husband-wife vignettes

‡ Same characteristics as for welfare poor except occupation shown as "garbage collector for males and laundry worker for females" (except for wives who are shown to be "housewives")

§ Characteristics shown as the mean values for each of the vignette characteristics Note that the "average" person or household here is average in the sense of this study rather than the universe of American households Averages for values of these variables are shown in the margins of table 2 for one-person households, they are substantially identical to those for two-person households

‖ Vignette characteristics set at their highest values occupations as "physicians," educational attainment as "college graduates," and ethnicity as "British-Americans."

bounds. Using the working poor, the lower bounds become $11,346 and $13,655 for one-person and two-person households.

The computed social maxima are clearly below maximum earnings in contemporary times. While very few households currently earn incomes of the magnitudes shown as maxima, the highest earnings are clearly many magnitudes larger. The social minima, in contrast, appear somewhat closer to median earnings. For example, the median earnings of a husband and wife household in 1975 (when the wife was not in the labor force) were $12,757, slightly below the fair payment for the working poor household and considerably below the fair earnings for our "average" households (U.S. Bureau of the Census 1977). The 1975 poverty levels for one-person households and two-person households were $2,724 and $3,506, figures that were about the minima computed for the one-person welfare poor and well below the computed minima for husbands and wives.

In sum, respondents were both more generous and more democratic than public policy and the empirical income distribution. The ceilings on earnings represented by the computed social maxima are well below the earnings of the affluent. At the same time fair earnings for persons on the bottom of the occupational and educational pyramids are set higher than poverty-level definitions Implicitly respondents define an equitable earnings distribution that has a higher mean and a lower variance.

Consensus and Earnings-Fairness Judgments

The results reported thus far in this article suggest that judgments concerning fairness in the distribution of earnings are not random, and indeed

reflect the application of some set of rules which balance off considerations of merit and need. Some amount of agreement in regard to relevant criteria is a necessary empirical constraint for any empirical theory of distributive justice. In other words, while individuals may disagree on exactly how various rules are to be applied in practice, they must at least agree on what the rules or principles for judging the fairness of allocations of distributive shares are to be (Homans 1974). As noted earlier, this amounts to the question whether interrespondent variation is large enough to render the empirical definition of fairness useless. This does not mean, of course, that respondents do not systematically vary in their judgments of earnings fairness, only that this variation does not overshadow the findings reported earlier in this article.

The fact that about half of the variance in fairness ratings goes unexplained in the analyses of tables 2 and 3 hints strongly that there may be some lack of consensus over the principles that underlie earnings-fairness judgments. Since some of the unexplained variance can be attributed to "error" in the sense of lack of individual consistency in making ratings,[16] it is clearly incorrect to regard the amount of unexplained variance as a pure indicator of lack of consensus. Our approach to the question of consensus has been to look for subgroups among our respondents who differ systematically in the way in which their members handled the fairness ratings. This approach equates dissensus with lack of agreement among significant subgroups of respondents, a tactic that rests heavily upon our ability to identify significant subgroups. To the extent that the data collected do not allow us to identify other, possibly more important, respondent subgroups, our tests must be regarded as only partial tests of the issues raised by the question of consensus over ratings.

Our approach requires us to examine our data for significant respondent-vignette characteristics interactions rather than simply focus on main effects of respondent characteristics which really only indicate overall differences in level of fairness ratings. As a heuristic guide we began by regressing fairness ratings on vignette and respondent characteristics, as well as various respondent-vignette interactions. Results of these early analyses pointed to potential differences across subgroups of respondents in the metric coefficients reported in tables 2 and 3, as well as differences in

[16] For example, an individual may use adjacent ratings to express the same (subjective) degree of fairness or unfairness, sometimes using one and sometimes the other. Or some individuals may center their ratings on higher means or use larger ratings variances than others, while retaining the same relative judgments concerning particular vignettes. Both tendencies would increase the amount of unexplained variance in tables 2 and 3 without indicating a different underlying structure of judgments concerning the fairness of earnings.

minima-maxima levels implicit in table 4 and reported in table 5.[17] Differences appeared to be between high- and low-status respondents as measured by income, occupation, education, and race, which is not surprising. Considering that these variables are ordinarily about as powerful subgroup indicators as are usually found in sociological studies, the tests for consensus that we will employ are nontrivial.

On the basis of the earlier analyses noted above, we divided our sample into subgroups which would capture significant differences while at the same time containing enough cases to render results reliable and meaningful. Tables 6 and 7 present the results of regression of earnings-fairness ratings, for one-person and husband-wife households, respectively, on both vignette and respondent characteristics for various subgroups of respondents. Only results for high- and low-income and high- and low-household-prestige respondents are presented. Little in the way of subgroup differences was evident when we partitioned our sample into high- and low-education subgroups, and there were not enough blacks in our sample to make race comparisons possible. Since the results for one-person household earnings-fairness ratings are substantively similar to those for husband-wife households, we will focus our discussion on the latter. Examination of table 7 reveals that high-status respondents place greater weight on merit considerations, while low-status respondents give need considerations more weight. Coefficients for husbands' and wives' education and occupation are larger in the high-status respondent regressions, while the coefficients for number of children are larger in the low-status respondent regressions. The differences in coefficients reported are typically four times as large as the standard errors of any of the individual coefficients presented in table 7. Further, the equations appear to have somewhat better fit for high-status respondents than for low-status ones as reflected in the considerable differences in R^2s reported in tables 6 and 7. Of course, the usual caveats regarding comparison of R^2s across various equations must be considered in interpreting these findings.

Clearly there is systematic variation in the structure of judgments of earnings fairness due to respondent characteristics. But this finding does not nullify the results and interpretations made earlier regarding central tendencies in the structure of earnings-fairness judgments. This is reflected in the fact that the R^2 statistic increased by only .011 (for one-person households) and .012 (for husband-wife households) when respondent characteristics and respondent-vignette characteristics interactions were entered

[17] We omit reporting coefficients for vignette characteristics here since they were assigned at random to respondents and hence are orthogonal to respondent characteristics, and resulting coefficients are identical to those of tables 2 and 3.

TABLE 6

REGRESSIONS OF ONE-PERSON HOUSEHOLD EARNINGS-FAIRNESS RATINGS ON VIGNETTE AND RESPONDENT CHARACTERISTICS FOR VARIOUS RESPONDENT SUBGROUPS

	HIGH-INCOME* RESPONDENTS		LOW-INCOME RESPONDENTS		HIGH-HOUSEHOLD PRESTIGE† RESPONDENTS		LOW-HOUSEHOLD PRESTIGE RESPONDENTS	
	b	*t*	*b*	*t*	*b*	*t*	*b*	*t*
Vignette characteristics:								
1. Education (years)	04399	3 2	06457	4 6	05467	3 8	05668	4.2
2. No. of children (0–6)	10733	4 0	12489	4 6	10187	3 7	12460	4 8
3. Sex (M = 1, F = 2)	− 23508	−2 7	03123	3	− 02675	− 3	− 13793	−1 6
4. Gross earnings ($ hundreds)	− 03284	−18 6	− 03147	−16 8	− 03246	−17 4	− 03185	−17.9
5. Ethnicity	00692	2 1	00108	3	00672	1 9	00260	.8
6. Occupation (NORC)	03214	11 4	01919	6 5	03151	10 6	02054	7.3
7 Gross earnings squared	00004	10 7	00005	10 4	.00004	9 9	00005	11.1
8. Marital status (0 = previously married, 1 = single)	02103	1	16246	1 1	− 04400	− 3	20159	1.4
Respondent characteristics:								
9. Sex (M = 1, F = 2)	07622	9	15768	1 7	− 04467	− 5	19012	2.2
10. Race (1 = white, 2 = black)	34704	2 7	19562	1 5	28451	1 9	21871	1.8
11. Education (years)	− 01518	− 7	− 01274	− 8	− 03028	−1.3	− 01452	− 9
12. Income ($ thousands)	− 00907	− 8	00606	4	− 00236	− 3	01808	2.2
13 Household prestige	− 00806	−2 3	− 00517	−1 3	− 04400	− 3	20159	1.4
Intercept	7 388		6 802		6 699		7 098	
R^2	619		473		619		486	
N	1,081		1,341		960		1,462	

* Income divided at $14,000 or more per year

† Household prestige as computed according to Rossi et al (1974) and divided at 55 0 or above.

TABLE 7

REGRESSIONS OF HUSBAND-WIFE HOUSEHOLD EARNINGS-FAIRNESS RATINGS ON VIGNETTE AND RESPONDENT CHARACTERISTICS FOR VARIOUS RESPONDENT SUBGROUPS

	High-Income* Respondents		Low-Income Respondents		High-Household Prestige† Respondents		Low-Household Prestige Respondents	
	b	*t*	*b*	*t*	*b*	*t*	*b*	*t*
Vignette characteristics·								
1. Husband's education (years)	03993	7 8	01963	3 9	04399	8 2	.01939	4.0
2. Wife's education (years).	02184	4 2	01173	2 3	02577	4 8	01023	2.1
3. No. children (0–6)	04712	6 6	09845	14 1	05855	7 8	08587	12.8
4. Husband's gross earnings ($ hundreds)	− 01956	−29 1	− 01674	−25 4	− 02010	−28 7	− 01673	−26.3
5 Wife's gross earnings ($ hundreds)	− 01166	−24 1	−.01188	−25 4	− 01181	−23 5	−.01176	−25.9
6. Ethnicity	00214	1 8	− 00049	− 4	.00240	1 9	− 00077	−.7
7. Husband's occupation (NORC)	01908	17 9	01389	13 2	01876	16 8	01450	14 3
8. Wife's occupation (NORC)	01844	15 1	01285	10 8	01923	15 2	.01265	10.9
9 Husband's earnings squared	00002	14 6	00002	12 6	00002	14 9	00002	12.8
10. Wife's earnings squared	00001	5 8	00001	9 6	00001	5.9	00001	9.5
Respondent characteristics·								
11. Sex (M = 1, F = 2)	− 01183	− 4	14966	4 6	07163	2 1	.08001	2 6
12. Race (1 = white, 2 = black)	32182	6 8	33891	7 3	38637	7 1	27322	6.4
13. Age (years)	00226	1 9	− 00454	−5 2	00149	1 3	−.00355	−3 9
14. Education (years)	− 03369	−4 2	− 00351	−.6	−.03914	−4 3	−.00269	−.5
15. Income ($ thousands)	− 00694	−1 6	01664	2 8	.00422	1 4	.01467	4.7
16. Household prestige	− 00583	−4 2	− 01001	−7 1	− 01004	−4 4	−.01284	−6.9
Intercept	6 557		6 132		6.462		6.447	
R^2	503		396		509		.398	
N	9,411		11,418		8,448		12,381	

* Income divided at $14,000 or more per year.

† Household prestige as computed according to Rossi et al. (1974) and divided at 55.0 or above.

into the earnings-fairness regressions. One way of looking at this result is that very little in the way of unique variation is accounted for by differences among respondents or by interactions between respondent and vignette characteristics. We do, however, gain increased insight into how principles for judging the allocation of earnings are applied in actual practice. As the household prestige of respondents increases there is a slight tendency to judge vignettes overpaid; yet when the household is characterized by high occupational and educational attainment, higher-prestige respondents tend to judge the household underpaid to a greater extent than do lower-prestige respondents. On the other hand, lower-prestige respondents are more sensitive to the number of dependents in the households than are higher-prestige respondents. Respondents appear to be favoring their cognates in the vignettes, while tempering this tendency when attainments seem too large. On the whole, it is important to remember that subgroup differences in rating tendencies, while systematic, add little to the explained variance, contributing relatively small changes in the ratings that are influenced by where the rater stands in the American social structure.

The results of similar subgroup analyses focusing on questions regarding the dollar value of equity and a fair distribution of earnings pointed to somewhat the same departures from central tendencies noted for subgroup analyses of earnings-fairness ratings. Higher-status respondents allowed more earnings for additional increments in education and occupational attainment of vignette household members, while low-status respondents gave considerably more earnings for each additional child. With regard to minima-maxima levels, two findings emerged. Both high- and low-status respondents appear to adhere to roughly the same minima as reflected in the intercept terms across respective equations remaining roughly the same.[18] However, the implied maxima for high-status respondents tend to be greater than those for low-status respondents. By allowing for higher maximal fair earnings, high-status respondents "stretch out" the inequality in a fair distribution of earnings, while low-status respondents are "shrinking" that inequality.

CONCLUSION: IMPLICATIONS FOR DISTRIBUTIVE-JUSTICE RESEARCH

We set out to find whether a normative structure governing judgments of fairness in the distribution of household earnings exists in the United States and, if so, to flesh out the detail of that structure in light of presumably relevant characteristics. Previous research traditions (Berger et al 1972; Berkowitz and Walster 1976; Brandt 1962; Rawls 1971) have pre-

[18] It should be noted that the analyses represented in tables 6 and 7 detect both additive and interactive respondent-characteristic effects

sumed the existence of a consensual normative framework for making distributive-justice judgments; they may proceed assured that such a framework does exist, as amply demonstrated by the research reported in this paper.

In general, judgments concerning the extent to which a household is overpaid, fairly paid, or underpaid involve balancing both merit or performance considerations and those of need.

Not all characteristics nor all individuals' characteristics play equal roles in justifying earnings. Occupational overshadow educational attainments and husbands' characteristics count more than wives'. Because only two measures of need, marital status and number of children, were used in the construction of vignettes, it is not possible to judge the relative sensitivity of judgments to need as compared with merit; the best we can say is that both are taken into account in the judgments. Further, without nullifying findings describing central tendencies in earnings-fairness judgments, respondents' positions in the American social structure systematically influence their balancing of merit or performance considerations against those of need.

Our study has by no means exhausted all that can be studied about distributive-justice norms. The vignettes constructed simulate only crudely the empirical world and hence the judgments we have elicited are blunted to that extent. There are undoubtedly additional individual and household characteristics that play important roles in such judgments. We do believe, however, that we have contributed both a demonstration of a fruitful approach to studying such judgments and some evidence that such judgments are lawful and normative.

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Ascription versus Achievement in the Attribution of Family Social Status[1]

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The social standing of families in our society is the result of collective evaluations of certain characteristics of family members. The most important characteristics are known to be the occupational and educational attainments of both the husband and wife. Beyond such achieved characteristics, there are many ascribed characteristics which have been presumed to have importance in the determination of a family's social standing. This paper presents the results of research on the relative importance of a number of achieved and ascribed characteristics in the evaluation of family units. Social origins as indicated by parental social status and individual achievements as indicated by educational and occupational attainments are studied for their relative importance to the process of status attribution. We show that achievement is more important than ascription. However, the latter is quite important in the status evaluation of families in our society. Further, it is apparent that families are evaluated on the basis of well-understood and agreed-upon principles.

Sociologists have long asserted that families are the basic units of our social stratification system The reason given is that all members of a family are seen as sharing equal status in society's eyes (see Parsons 1949; Barber 1957; Turner 1970). Despite this, we have only recently begun empirical studies aimed at discovering the determinants of family social standing or developing methods for measuring the social status of families. The status or prestige attributed to a household has been shown to be strongly affected by the occupational and educational attainment levels (Rossi et al. 1974; Sampson 1973; Sampson and Rossi 1975) of household members. All members contribute unequally, so it is impossible to assign to any family a posi-

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tion based on one member's characteristics. Further, it is clear that occup tion and education dominate ethnicity and race. Of course, neither ethnici nor race begins to exhaust the catalog of ascriptive statuses. Particular important are characteristics of the families of origin of household mei bers, to which this article will devote major attention.

The family of origin is one of the few ascriptive groups to which mei bers of modern society belong, and as such it is believed to have cons quences throughout one's life. It has become abundantly clear from tl status-attainment studies of the past decade that the social origins of pe sons have both direct and indirect effects on their status attainment (Bl: and Duncan 1967; Sewell and Hauser 1975; Jencks et al. 1972). Occupatio al achievement levels of adults are strongly conditioned by the characteristi of families of origin. Such achievements are indirectly affected by parent characteristics (particularly father's education) in that they condition one own educational attainment, which is then the principal determinant one's occupational achievement. We are more concerned in this paper wi the direct, independent effect of an individual's social origins. Thus tl issue is, What role do families of origin play in the attribution of soci status standing to households *independent* of the individual achievemen of the family members?

In the general social stratification literature we find considerable suppo for the position that the prestige standing of a family unit is affected vei directly by the standing of the members' own families of orientation, i: dependent of the individual achievements of family members. The liter ture is silent on how ascription and achievement elements are combined arrive at an overall current social standing, but there is considerable agre ment that some significant contribution is made by each set of elemen (Gerth and Mills in Stub 1972; Shils 1970; Goode in Bendix and Lips 1966; Parsons 1949).

We use the terms "status," "prestige," and "social standing" inte changeably and define them for our purposes as the collectively shared a sessment or evaluation of a societal unit on the basis of certain objecti characteristics of the unit. We presume that people do make status distin tions and that these distinctions are important in guiding and directir various formal and informal encounters. Such a presumption is based o substantial empirical evidence that people can make shared distinctions o social objects in terms of their social status (see Centers 1949; Hodg Siegel, and Rossi 1964). Further, such subjective evaluations have bee shown to be important in face-to-face interaction. The perceived status o the "other" is an important determinant of the type of interaction in whic one engages (see Berger, Cohen, and Zelditch 1972). In task-oriente groups the perceived status of another will affect the amount of credenc given that person's evaluation of one's work and is thus an important el

ment in the formation of the self-concept (see Berger and Fisek 1970; Berger et al. 1972; Moore 1968; Webster and Sobieszek 1974).

Highlighting the importance of family social status is a body of literature concerning the exchange of help, friendship, money, etc., and describing kin networks, associational patterns, and marriage partners (Young and Willmott 1971; Bott 1957). Farber has shown that social status can serve as an organizing principle around which kinship may be structured. Prestige can serve as a "symbolic estate" in families, being transmitted, protected, and valued by family members (Farber 1971).

In sum, a family's social status is important in many respects. The mechanism of social evaluation is one which acts to expose the family to and integrate it into the larger societal milieu.

Our thesis rests on the premise that families or households are meaningful units of social stratification, a position that has considerable support in the stratification literature (Parsons 1949; Barber 1957; Turner 1970). That households are empirically meaningful as units of stratification has been shown in studies (Rossi et al. 1974; Sampson and Rossi 1975) in which respondents rate the social standings of households and place them in a status hierarchy on the basis of minimal descriptions of household members' occupational and educational attainments.

The present study can be seen as an extension of the status-attribution research undertaken by Rossi and his colleagues. In their studies individuals rated short vignettes which described simple nuclear families (husband and wife) in terms of their education and occupation along a nine-point scale of "social standing." These ratings were then regressed on the vignette-household characteristics to discern the determinants of household social standing ratings. Later studies enriched the descriptions of vignette families, extending them to include more complex families, race, age of members, number of children, ethnicity, and several other characteristics.

In this study the ascriptive elements of the family (i.e., social origins) are indicated by specifying the educational and occupational attainments of each family member's father as well as designating an ethnic or racial category for the family. The achievement elements are represented by the occupational and educational attainment levels of each family member and a statement concerning the migration history of the family.[2]

The selection of these characteristics was directed in part by past research as well as the methodological requirements of this study. Both education and occupation have consistently been found to be important determinants of family social status (see Rossi et al. 1974; Nock and Rossi, in press). For every type of household studied (single, divorced, widowed, married with minor children, married with adult children) these two char-

[2] Migration status refers to statements concerning the residential history of the vignette families, e.g., "The couple moved from the country to a large city."

acteristics outweigh all others considered (e.g., age, number of children, sex). In addition to these two, Sampson and Rossi (1975) found that race is important in the attribution of family social status. Complementing such a finding are the early research by Bogardus (1925) and the more recent work by Triandis and Triandis (1965) which indicate that ethnic titles are relevant in status judgments. Thus the families studied in the research on which this paper is based have their social origins described by characteristics which have been demonstrated to have strong and consistent effects in the determination of family status.

An equal amount of information describes the achievements of the family members. In addition to indicating occupational and educational achievements of each member, we have described the family's migration status (migration history and type of area lived in). Whereas parental achievement is included to indicate the social origins of each person (i.e., the social context within which his own achievement occurred), migration status is included to indicate some sort of spatial context within which the individual and joint achievements of the couple occurred. Empirical studies have found considerable differences in individual occupational status according to the size of areas moved to and from. Blau and Duncan noted that ". . . regardless of the size of the community where [a man] now works, the larger the community where he grew up, the better are his chances to achieve occupational success and to move up from the status of his father" (Blau and Duncan in Abrahamson, Mizruchi, and Hornung 1976, p. 287). This superiority is independent of other effects, and Blau and Duncan only speculate that it may be due to "sophistication about the labor market and the occupational life generally that boys growing up in large cities tend to acquire" (ibid.). Thus we know that there is some enhanced achievement value associated with spatial origins. Whether or not there is also status value is the question we are asking.

We know that occupations often differ according to the context in which they are found. Being a doctor in a small town is not the same as being one in a large metropolitan area. Thus it may be hypothesized that migration to certain areas (particularly the large city where one finds the best occupational opportunities) not only might be seen instrumentally but also may be status enhancing in itself. As such, migration would be properly seen as an aspect of achievement. In all, 25 statements describe residence or migration from or to various types of areas which differ in size (e.g., suburb of a large city, large city, etc.).

Note that we are not attempting to study famous lineages here, such as the Rockefellers or Mellons. The intergenerational transmission of great estates or fortunes or privileges of upper-class membership is another topic altogether. The families described in the vignettes used in this study have

no lineage descriptors or names assigned to them. This is a study of persons who hold the positions in our society that are presumably neither at the very pinnacle nor in the lowest depths. In this respect, our study is a fit companion to the status-attainment studies.

METHOD

The method used centers on the evaluative or subjective aspects of social status. Status judgments were solicited concerning households described by systematically rotating various characteristics in approximately random combinations. The ratings given to these hypothetical households are taken to mirror the ways in which persons in our society actually evaluate households. It is this evaluation process we want to understand. It should be noted that we are not attempting to describe the process of status attainment of either members of the families of origin or of the nuclear unit.

It would have been desirable to obtain ratings of a large sample of actual families known by a large sample of adults. This, of course, is practically unfeasible. Very few (if any) families are well known enough to be rated by a national sample of respondents. Our method is analogous to that used to obtain prestige values for occupations, ratings of occupations being obtained without reference to actual incumbents.

A national area probability sample of 536 adult noninstitutionalized respondents rated on a nine-point scale the social standing of a set of 50 vignette descriptions of hypothetical households. Fieldwork was conducted by Audits and Surveys, Inc., a New York–based research organization.[3] A sex quota of 50% was imposed. The hypothetical households which respondents rated were described in short vignettes, containing the following information.

I. Nuclear family
   A. Occupation of husband (one of 50 occupations including living on welfare)
   B. Educational attainment of husband (fifth grade through college)
   C. Occupation of wife (one of 50 occupations including that of housewife)
   D. Educational attainment of wife (fifth grade through college)
   E. Migration status—Area of residence and migration history (permanent residence in, or migration from and to, one of five types of areas: large city, suburb of large city, small town, small city, country)

[3] We wish to thank Sol Dutkas of Audits and Surveys for his help in monitoring and managing this project.

II. Family of origin
  A. Husband's father's educational attainment (fifth grade through college)
  B. Husband's father's occupation (one of 50 occupations including living on welfare)
  C. Wife's father's educational attainment (fifth grade through college)
  D. Wife's father's occupation (one of 50 occupations including living on welfare)
  E. Ethnicity, described by the national origin of the nuclear family (one of 20 ethnicities including Black-American)

Occupation is measured in terms of occupational prestige scores reported in Ornstein (1977). Education is measured as years of education. Ethnicity is included here (indicated by national origin) to indicate social origin. That a status hierarchy of ethnicities exists has been demonstrated by a number of sociologists (Bogardus 1925; Triandis and Triandis 1965) Hodge et al. derived prestige scores for each of 20 ethnic categories in 1964. We use these scores to measure ethnicity.

Migration status refers to statements describing the general type of area in which the nuclear couple lives and their history of migration. This variable was constructed by assigning to each statement the *b* coefficient associated with it when entered as a dummy variable in an equation predicting ratings of social standing. This was the score used to represent each statement and thus it reflects the effect each statement has on the estimated ratings of households.

Families of origin are described by reference to each member's father only. While it would have been desirable to describe both families of origin as completely as we did the nuclear family (i.e., to describe the couple's mothers as well as their fathers), there are two reasons this was not done.

First, it has been found that at some point the amount of information presented to individuals who are making status judgments becomes burdensome. Thus the complexity of the vignettes used to describe families must be kept to a minimum. We felt that four additional descriptive elements (education and occupation of both mothers) would make the task tiring and generate error.

Second, the information describing families resides on a vignette card the size of a standard data card (IBM card). Formatting such vignettes must be done in such a way as to present the information clearly and neatly Pretests indicated that vignettes containing descriptions of husband's and wife's mothers were difficult to read or evaluate easily.

We do not feel that the omission of this information is very problematic The unit of analysis is the nuclear family, and descriptions of the family

members' parents serve only to indicate social origin. Thus while our indicators of social origin could be more complete, we have chosen those elements we know to be most important to family social standing (husband's occupation and education) and thus feel that we have adequately indicated the ascriptive elements of the study.

The vignettes used in this study were generated by a computer program which randomly combined descriptive statements drawn from lists of each of the characteristics mentioned above, subject to certain restrictions necessary to prevent bizarre or anomalous combinations.[4] This procedure amounts to random sampling from the universe of all possible families like those on the vignettes. (See Alves and Rossi [1978] for further discussion of the vignette technique.)

Since the statistical association among attributes of an individual and between different individuals within a household is set by design at low levels, our results reflect the accurate independent effect of each characteristic. Two examples of the vignettes are shown in figure 1.

A total of 26,800 vignettes was produced in this fashion. Of these, useful ratings were obtained for 26,734 (99%). Most of the missing vignettes resulted from cards being bent both prior and subsequent to fieldwork. Interviews were administered by experienced survey interviewers and lasted about one-half hour. In addition to the rating task, the interview included questions about the socioeconomic status of the respondent and his or her household. Respondents were asked to read each card aloud and place it in one of nine slots in a cardboard box. Each slot corresponded to a level of social standing ranging from a high of nine to a low of one.[5] Cards which the respondent could not sort were placed in a tenth slot. Analysis of them did not show them to differ in any understandably systematic way from the sorted vignettes.

[4] We felt that certain combinations of characteristics would be virtually impossible in the real world, and we therefore forced certain such combinations to be excluded. For example, six occupations were forced to be accompanied by educational attainment of a college graduate (physician, college professor, electrical engineer, lawyer, city superintendent of schools, and high school teacher). In addition, no husband-wife pair of occupations could differ by more than 50 occupational prestige points. Certain occupations could not be associated with residence in the country. Among the vignettes 25% described wives as "housewives" (prestige score from Bose 1973) and 10% described the couple as being "Black-Americans."

[5] The wording of the question used to elicit the rating was: "Families differ in a lot of ways. We have made up descriptions of different kinds of families. Here is a box with nine different slots in it. Here are some cards that have descriptions of families on them. Put a card in the slot at the *TOP* of the box marked with a '9' if you think that a family like the one described on the card would have the highest social standing. Put a card in the slot at the *BOTTOM* of the box marked with a '1' if you think that a family like the one described on the card would have the lowest social standing. If the family belongs somewhere in between, put the card in a slot that matches the social standing of the family."

To explain the determinants of the social standing ratings given to vignettes, we posit a model in which the ratings are a function of the vignette characteristics, characteristics of the respondent, and some interaction of these factors. We have employed ordinary least-squares regression as our mode of analysis using the model $R_i = f[(V_i) + (I_{ij}) + (IV_{ij}) + (VV_{ik, ik+1})] + e$ where R_i = rating given to vignette *i*, V_i = coded characteristics of information of vignette *i*, I_{ij} = characteristics of respondent *j* who rated vignette *i*, IV_{ij} = interaction of characteristics of respondent *j* and characteristics of vignette *i*, $VV_{ik, ik+1}$ = interaction of vignette characteristic *k* and vignette characteristic $k + 1$, etc., and e = random error.

We will consistently refer to two forms of R_i throughout this paper; the original rating converted to a scale which runs from zero to 100 and the *z*-score representation of respondent's ratings. We use HPRES for the former and ZPRES for the latter form of transformed rating as follows HPRES = (original rating − 1) · 12.5; ZPRES = $(R_{ij} - \bar{R}_j)/{}^sR_j$, with

HUSBAND	DID NOT FINISH ELEMENTARY SCHOOL LOCOMOTIVE ENGINEER
HUSBAND'S FATHER	GRADUATED ELEMENTARY SCHOOL ASSEMBLY LINE OPERATOR
WIFE	GRADUATED ELEMENTARY SCHOOL TEXTILE MACHINE OPERATOR
WIFE'S FATHER	GRADUATED COLLEGE PHYSICIAN

THE COUPLE'S NATIONAL BACKGROUND IS POLISH-AMERICAN

THE COUPLE MOVED FROM A SUBURB OF A LARGE CITY TO THE COUNTRY

HUSBAND	GRADUATED COLLEGE PHYSICIAN
HUSBAND'S FATHER	GRADUATED HIGH SCHOOL CAR DEALER
WIFE	FINISHED TWO YEARS OF COLLEGE OFFICE SECRETARY
WIFE'S FATHER	FINISHED TWO YEARS OF HIGH SCHOOL WASHING MACHINE REPAIRMAN

THE COUPLE'S NATIONAL BACKGROUND IS ITALIAN-AMERICAN

THE COUPLE HAS ALWAYS LIVED IN THE SUBURB OF A LARGE CITY

FIG. 1 —Examples of vignettes

HPRES simply a conversion of the original nine-point scale to make it comparable with occupational prestige scales which usually run from zero to 100 and ZPRES the z-score of the rating respondent j gave to vignette i, standardized in terms of the particular respondent's mean ($\bar{R}_j$) and standard deviation (^{s}) of ratings.

In contrast to HPRES, ZPRES reflects the possibility that respondents may vary in both their average ratings and the amount of variance in their ratings. Some individuals may rate all vignettes relatively higher or lower than do other respondents while still using the same rating principles. Similarly, some raters may use more of the rating scale than do others while agreeing on the rating principles used; ZPRES will remove such differences, giving all raters a mean rating of zero and standard deviation of one.

If respondents agreed on the principles they used in making their judgments of the vignette families but differed in terms of the numbers they chose to represent their judgments (for example, one respondent using 2 as the lowest score and another respondent using 3 despite agreement on which families would be given the lowest score), ZPRES will remove the effects of such differences. If such differences exist, ZPRES will better reflect the amount of agreement among respondents on the principles invoked in making status judgments.

VIGNETTE-CHARACTERISTIC EFFECTS

In table 1 we present the zero-order correlations, means, and standard deviations for all vignette characteristics, HPRES, and ZPRES. The positive signs on all coefficients relating vignette characteristics to either form of rating indicate that respondents relied on the characteristics in making their judgments. Had anything other than these characteristics been used by the respondents (such as order of deck placement, one or two characteristics only, random sorting, etc.) these correlations could not have been obtained. Since characteristics on the vignettes are related at very low magnitudes one with the other, every one must have been used by respondents since each is positively related to the ratings.

The equations in table 2 are the result of regressing HPRES and ZPRES (equations [1] and [2], respectively) on the characteristics of the vignettes.[6] One of the most remarkable things about these equations is the rather small amount of explanatory power they express The 10 family characteristics explain between 18% and 26% of the total variance in rating

[6] Standardized coefficients (beta coefficients) are not presented in this table As noted, the variances of vignette characteristics are set by design of the production technique and bear little resemblance to the real-world variances of these characteristics Beta weights are not understandable except within a framework of a world in which the variables studied here are distributed rectangularly (as are ours)

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TABLE 1

CORRELATIONS, MEANS, AND STANDARD DEVIATIONS AMONG VIGNETTE VARIABLES, HPRES, AND ZPRES
(N = 26,253)

	1	2	3	4	5	6	7	8	9	10	11	12
Education:												
1. Husband												
2. Husband's father	.00											
3. Wife	.01	00										
4. Wife's father	00	00	00									
5. Ethnicity*	00	00	00	00								
Occupation:†												
6. Husband	30	00	03	00	00							
7. Husband's father	00	30	00	00	00	.00						
8. Wife	00	00	24	00	.00	.09	00					
9. Wife's father	.03	00	.00	.30	.00	.00	.00	00				
10. Migration status‡	00	00	00	00	09	00	.00	01	−.01			
11. HPRES	24	03	12	02	04	35	04	17	04	.03		
12. ZPRES	29	04	.14	03	05	42	04	22	.05	.02	.82	
Mean	11 63	11 54	11.38	11 62	43 50	43.29	43.40	44 19	43.45	47 69	48 76	0 00
SD	3 59	3 58	3 53	3 57	13 36	16 08	16 12	13 48	16.11	0 86	26.66	1.00

* Ethnicity scores derived by Hodge, Siegel, and Rossi 1964

† Occupational prestige scores from Ornstein 1977.

‡ Variable constructed from 25 migration-pattern statement dummies (see text).

TABLE 2

REGRESSION OF HPRES AND ZPRES ON VIGNETTE CHARACTERISTICS

Vignette Characteristic	HPRES, Equation 1 *b*	HPRES, Equation 1 Standard Error *b*	HPRES, Equation 1 Substantive Criterion*	ZPRES, Equation 2 *b*	ZPRES, Equation 2 Standard Error *b*	ZPRES, Equation 2 Substantive Criterion*
Education:						
Husband	1 166	.044	14 56	0506	.002	16 63
Husband's father	136	044	1 70	0070	002	2 26
Wife	623	044	7 79	0267	002	8 87
Wife's father	109	044	1 36	.0069	002	2 27
Ethnicity†	076	011	3 76	0036	000	4 65
Occupational prestige‡						
Husband	483	010	40 20	0217	.000	47 30
Husband's father	070	010	5 83	0028	000	5 91
Wife	240	011	19 97	0117	.000	25 64
Wife's father.	068	010	5 65	0030	000	6 79
Migration Status§	750	174	2 90	0166	006	1 68

NOTE --Intercept = −52 13 and −3 729, R^2 = 179 and 259, F = 915 and 574, and N = 26,253 and 26,253, for HPRES and ZPRES, respectively

* Percent change in predicted score due to variable

† Ethnicity scores derived by Hodge, Siegel, and Rossi 1964

‡ Occupational prestige scores from Ornstein 1977

§ Coded from 25 categoric statements

depending upon the particular version of rating we use. The standard errors are quite small and are, of course, not affected by the amount of explained variance. The small R^2s, are, in part, a function of the rating task itself. With fewer characteristics other researchers have accounted for considerably more variance by computing mean ratings given to identical vignettes (R^2s ranging from .60 to .80; see Rossi et al. [1974]). We have found that the more information is included on the vignette, the more error is generated by the rating task. Similarly, the more vignettes each respondent rates, the more error is generated. Such a large sample virtually guarantees that a simple linear additive model will express only modest amounts of explained variance. We will demonstrate later that there is some idiosyncratic error among respondents consistent with interindividual similarity in the *structure* of their ratings which reduces the explained variance. We have found that estimating equations for each respondent reveals much higher R^2s even when the structure of equations is unchanged. If our goal were simply to explain more variance we could easily fit a power function which explains 97% of the variance.[7] Since our goal is to explore the rela-

[7] We fitted the equation as a power function, taking our task as a psychophysical one. We were able to explain 97% of the variance ($R^2 = 97$) with a function of the form $\text{rating}_{\ldots} = {}^{0412} \cdot \text{husband's education}^{37} \cdot \text{husband's father's education}^{04} \cdot \text{wife's education}^{03} \cdot \text{wife's father's education}^{03} \cdot \text{husband's occupational prestige}^{60} \cdot \text{husband's father's occupation}^{08} \cdot \text{wife's occupation}^{25} \cdot \text{wife's father's occupation}^{07} \cdot \text{ethnicity}^{12} \cdot \text{migration status}^{9999}$ This equation certainly captures more variance, but the reader will have noticed that the relationship among exponents parallels almost exactly that among linear predictors Thus, the noticeable difference between the linear and the nonlinear equations is only in the amount of explained variance

tive importance of various family characteristics (specifically, ascribed and achieved), we see no persuasive reason to do so. We take our parameter estimates to be good estimates of the actual empirical importance of the characteristics we are studying. Low R^2s in aggregate equations such as those in table 2 are not particularly surprising and are certainly not problematic.

It is not at all surprising that all coefficients attain statistical significance —with such a large sample virtually any departure from zero will result in statistically significant estimates. Further, the large sample gives us reason to believe that even the very small slopes portray accurately the true empirical importance of characteristics and are not simply due to measurement error. Thus it is obvious that some statistically significant variables mean very little in any substantive sense. For this reason, whenever a variable attained statistical significance (and only then), we tested to determine its actual contribution to the entire equation.

The question we asked was whether the inclusion of a variable alters appreciably the value predicted by the equation. We have taken as substantively important any variable which could change the value of HPRES predicted by the equation by as much as 5%. This is determined simply by evaluating each variable at its maximum and minimum contributions to the value of the predicted prestige score for the household. If the difference between these two values is less than 5% of the range of estimated scores, we consider a variable unimportant. We refer to this quantity as "substantive criterion" in the tables in this paper. For example, the range of estimated HPRES scores from equation (1) in table 2 is 1.56 to 89.62 or 88.06 The slope on ethnicity is 0.076. Multiplying this by the minimum value of the variable (21.9, the prestige score for the category Black-American) and the maximum (65.4, the prestige score for the category British-American) and taking the difference (3.31) and dividing by the range tells us that this variable could alter the HPRES estimate by as much as 3.7%—not significant by our standards. In other words, it matters little what value the variable takes on in terms of the value predicted by the equation While the cutoff point of 5% is arbitrary, we feel that elaborating on characteristics which do less than this to the overall results of the equation amounts to much ado about nothing.

A 5% difference in the score predicted by the equation is equivalent to the difference in prestige between such pairs of occupations as accountant and registered nurse, or social worker and economist.

DETERMINANTS OF FAMILY SOCIAL STANDING

Looking now at equation (1) in table 2 we note first that there are considerable differences among the family characteristics in terms of their

importance to the predicted values of HPRES—ranging from a potential contribution of less than 2% (wife's father's education) to more than 40% (husband's occupation). These differences parallel quite closely the relative magnitudes of the bivariate relationships we observed in table 1 above. Obviously, not all characteristics included on the vignettes are important to the estimated value of HPRES. Recall that any characteristic which contributes less than 5% to the value of the predicted score of HPRES is deemed unimportant for this study.

Surprisingly, national origin (ethnicity) fails to meet our criterion of importance. A black family and a British-American family would differ by only 3.31 points in predicted HPRES (i.e., change due to black ethnicity = 4.49), all other things being equal. Past studies cited earlier have found that a prestige hierarchy of ethnicities does exist and members of our society agree on the relative ranking of ethnic titles. However, we find that such information is not very important in the overall evaluation of a household. As with any other characteristic, however, it may be that it is, or is not, important relative only to the other information presented. Thus, for example, ethnicity may not be salient when detailed educational and occupational information is available, but when such information is not available it may indeed be very important. This would appear especially plausible to the extent that ethnicity "stands in" for such characteristics as education or occupation. If ethnicity or any other characteristic included in the study is normally important only by virtue of being a clue to other information, that is, by being associated with any kind of stereotype which involves education or occupation, this type of clue will wash out in the analysis.

Race especially and, to a lesser extent, ethnicity have traditionally been viewed as important ascriptive categories. Our evidence suggests that, as we have specified them, they are not very important. We suggest that the psychodynamics of race and ethnicity are similar. Both are ascriptive categories associated with varying degrees and strengths of stereotypes. However, the important point is that they are stereotypical categories. The "ideal world" of our vignettes violates all rules of stereotyping and thus renders these categories useless. While certain restrictions on educational and occupational attainment mixes were imposed to eliminate bizarre or anomalous combinations, no such restrictions were imposed on ethnic titles and occupational or educational attainments. If ethnicity and race are as probable predictors of great achievements (on those dimensions considered here) as they are of failure, they apparently serve no purpose in terms of their status value. In the real world, of course, we know that race and ethnicity do act to predict occupational and educational achievements. Apparently such information is what makes race and ethnicity important. That is to say, ethnicity is not independent in its effects. This finding is

consistent with that of Sampson and Rossi (1975) in their study of the effects of race on family social standing. In their research families were found to be affected by race by an amount equal to 1.75 HPRES points (p. 208). Whereas our effects are somewhat stronger due to the addition of ethnicity, neither our results nor Sampson and Rossi's meet our criterion of importance. As they note, ". . . a white/black difference of 1.72 points seems quite small" (ibid.).

Second, we note that migration status is not related to household social standing. As we had specified this item, it indicated migration or lack of it for the nuclear couple. What we find is that while such migration as from city to city may enhance achievement empirically, it is not recognized as a status element for the households in our society. Recall that such information was provided to indicate some context within which achievements (or failures) had occurred. We must conclude that even though certain types of spatial origins and migration patterns have definite achievement value in terms of one's occupation, they have no status value in themselves and do not affect the overall social standing of a family. It might be recalled that we had speculated that certain occupations might carry more or less weight depending upon the context within which they were found. Prestige values of various occupations and educational attainments were found to be essentially identical in various spatial contexts, indicating that spatial context as thus specified is not relevant in this regard (i.e., there was no interaction effect between migration status and achievement characteristics).

The statements referring to migration indicated that "the couple moved from ——— to ———," or "the couple has always lived in ———," thus giving no indication of when or why the couple moved. Further, respondents did not know whether the current occupations of husband and wife were the same as when they moved. Thus it is likely that the statement was meaningless even if spatial origins are empirically status relevant. They probably are not, however.

Third, we note that for both husband's father and wife's father educational attainment does not count in the reckoning of family social status. From previous studies we know that education is an independent prestige hierarchy even though occupation is the overwhelmingly most important hierarchy in this regard. In general, the importance of occupation is approximately twice that of education in studies such as these. To the extent that parental characteristics indicating social origin are significant, parental education is apparently the point of diminishing returns. That is, to the extent that respondents care to know about social origins, their concern does not go so far as to include information about parental educational attainment.

When we look at those characteristics which are important to the reckoning of family social standing, we find few surprises. The most important characteristics are the achieved characteristics of the husband and the wife. As represented by similar characteristics of parents, the overall significance of social origin appears to be slight. We have already noted that father's education is not important to family social standing. What is important are the occupations and educational attainments of the husband and wife. Occupational prestige is far and away the most important characteristic. Given this design, it is possible to ascertain the relative importance of similar vignette characteristics in this study by adding their slopes. That is, we can determine the combined effect of husband's and wife's occupation by simply adding the regression coefficients for occupation for each member. The sum of the b's will express the combined effects of the characteristics. Thus the appropriate formula for comparing the effects of husbands' and wives' occupations with the effects of fathers' occupations would be $(b_h + b_w)\Sigma\phi$ versus $(b_{hf} + b_{wf})\Sigma\phi$, where the subscripts h, w, hf, and wf refer to husband, wife, husband's father, and wife's father, respectively, and ϕ is the symbol for occupational prestige. This is possible in this case because for each member of the vignette ϕ is identical with the same variance.

Occupation

First, we wish to compare the nuclear couple with parents in terms of the relative importance of each set of individuals for the overall household social standing determined by their occupations. The sum of b's for occupations for the husband and wife is 0 723, whereas for their parents the comparable figure is 0.138. This difference points to the vast difference in importance between the two sets of occupational achievements. Note, however, that parental occupations are not insignificant in this regard. They are simply far less important than are the occupations of the nuclear couple.

As other studies have found (see Rossi et al. 1974; Nock and Rossi, in press, Sampson and Rossi 1975), even within the nuclear family there is no equality in terms of the contribution made to family standing via occupations. A wife's occupation counts slightly less than half as much as does her husband's (ratio of b's $=$ 0.496). Clearly, we cannot attempt to measure family social standing by using the husband's occupation as the sole measure of it. The husband's and wife's occupations together count slightly more than five times those of their fathers (ratio of $[b_h + b_w]/[b_{hf} + b_{wf}]$ $=$ 5.24). Stated differently, the husband's occupation is twice as important as is the wife's, and the wife's is slightly more than twice as important as the fathers' occupations combined.

Educational Attainment

Turning next to educational attainment we see that, again, there is considerable variation in terms of the importance of this characteristic as we compare the various individuals in the family. We noted earlier that fathers' education coefficients fail to meet our criterion of significance. An ordering of all characteristics in terms of their relative importance to family social standing would show that the education of the nuclear-family couple is second to occupational prestige in this regard. However, we note that the husband's education counts considerably more than does his wife's. The ratio of wife's to husband's education coefficients is 0.534—almost exactly that which we found for the relationship of occupational slopes (0.496). Even so, the wife's education is important. At the highest value for education (college graduate = 16 years), the wife's contribution to an estimated HPRES score would be quite noticeable, amounting to about 10 points ($16 \cdot 0.623 = 9.96$). Remember, this effect is an independent one, as are all effects discussed here. That is, any family characteristic's contribution to estimated HPRES is totally independent of the contribution of any other characteristic. This, of course, highlights the extreme importance we must attach to educational attainment. A 10-point contribution to the family, for example, is a considerable one. It is certainly enough of a contribution to indicate that failure to account for it is a rather serious error if one is trying to characterize a family's social standing.

Do any general patterns suggest themselves from the equations of table 2? To begin with, it appears that information about social origins is of some importance. However, the way social origins work is rather curious. For example, ethnicity and race are not important for the social standing of a household but fathers' occupations are. While we have not yet examined in detail the exact amount of importance attached to information about social origins, it is relatively less important than is information about the nuclear couple's individual achievements—their occupational achievements particularly but also their educational attainments. An attempt to suggest some sort of spatial context within which such achievements occurred apparently failed. Migration patterns do not have independent status effects for households. Thus, of all items considered, four were found to be unrelated to household prestige: ethnicity, husband's father's educational attainment, wife's father's educational attainment, and migration status. This speaks directly to the guiding question of this research. Can we say anything of the relative importance of ascribed and achieved characteristics? We can say that there is a strain toward ascription to the extent that parents' achievements are taken to be ascribed characteristics. In the sense that they are characteristics over which an individual has no control

and which serve to affect his/her family's social standing, they are properly considered ascriptive characteristics.

Second, while there is considerable variation in the importance of any single trait from one person to another, males consistently outweigh females in their importance to the family's social standing. This is to say that although females contribute significantly to the family's social standing they contribute less than do males. Our findings in this regard are similar to those of Rossi et al. (1974), Sampson and Rossi (1975), and Nock and Rossi (in press). Also, we might note here that Parsons's assertion that the husband has primary responsibility for determining the social standing of the family finds some empirical support here. However, the case was overstated and does not depend solely upon occupation (see Parsons 1949).

Third, to the extent that educational attainment is important (as it is, generally), it is less important than is occupational prestige for any one person's individual contribution to family social standing. That is, looking at the contribution made by any single member of either generation, we find that the contribution is disproportionately affected by the occupational prestige of the person.

Parental achievements do play a role in determining household prestige. High-achieving parents provide some honor to their children. How much?

Would it matter if the couple's fathers were each high achievers in terms of occupations? Recall that we are discussing the independent effects of fathers' occupations. Thus, after the husband and wife have done what they can for their social standing, what do they get from their parents? Let us begin by considering a couple with humble origins, each member coming from a family in which the father was a garbage collector (prestige score = 12.6). We have seen that it does not matter how much schooling the fathers had, but let us arbitrarily assign them a value of six years each The husband and wife in this family have both finished high school (education = 12) and the wife is a housewife (prestige score = 46.9). The husband is a postal clerk (prestige score = 43.0) and they have always lived in the suburbs of a large city (migration status score = 46.93). They are Italian-Americans (ethnicity = 50.1). Now where do they stand? We can calculate a predicted HPRES score for this family by applying equation (1) in table 2 with its elements in the order: $HPRES_{est} = -52.13 + 1.166\ (HED) + 0.623\ (WED) + 0.483\ (HOCC) + 0.240\ (WOCC) + 0.750\ (\text{migration status}) + 0.136\ (HFED) + 0.109\ (WFED) + 0.070\ (HFOCC) + 0.068\ (WFOCC) + 0.076\ (\text{ethnicity})$, where H, W, HF, and WF refer to husband, wife, husband's father, and wife's father, respectively, ED = education, and OCC = occupation. Substituting appropriate values for our couple, we have: $HPRES_{est} = -52.13 + 13.99 +$

7.48 + 20.77 + 11.26 + 35.20 + 0.82 + 0.65 + 0.88 + 0.86 + 3.81; $HPRES_{est} = 43.59$.

Now the question is whether it would matter if the couple was from less humble origins. Let us assume that they came from families in which the heads were not garbage collectors but physicians (prestige score = 81.5). The components of the equation which would change are the values for fathers' occupations. (We should also change education to reflect college degrees, but since education is unimportant we will leave it unchanged for this illustration.) Thus the husband's father's contribution now is 5.70 and the wife's father's is 5.54 (compared with 0.88 and 0.86 in the previous example). Now the predicted score for the household is 53.09 compared with 43.59 when fathers are garbage collectors. In other words, the effect of parental contributions was to alter the predicted prestige score for the household 10 points independently of the achievements of the children. Each father contributed about five points to this change. This example has used extreme values for parental characteristics, not typical ones. However, the point of the example was to illustrate the possible effect of father's occupation—an effect that we believe justifies our claim that there exists a significant ascriptive element in our stratification system.

The second equation in table 2, regressing ZPRES on family characteristics, is quite similar to the equation using HPRES. The only change of note is the increased explanatory power of the second equation. This suggests that the ZPRES standardization did remove some interindividual differences in mean rating and amount of variance in ratings. These differences, however, did not alter the structure of the equations appreciably.[8] For this reason as well as the convenient metric of HPRES, we will discuss substantive results in terms of HPRES points where possible.

INTERACTION TERMS

As we noted earlier, our conceptual model included some nonadditive effects. Two types of multiplicative interactions were investigated: the interactions of vignette variables with one another and the interaction of vignette variables with rater characteristics.

We do not present the results of this analysis as it is easily summarized. First, we were surprised to find that no interaction of rater and vignette characteristics emerged as substantively important. We had supposed that respondents might respond differently to certain characteristics of the vignettes depending upon whether or not they were similar to their own. For example, it was hypothesized that respondents with high levels of education

[8] We discern this by examining the relative sizes of the coefficients. Tests such as the Chow test are inappropriate for this task due to the extremely large sample size (see Chow 1960).

might place more emphasis on education than respondents with lower level of educational attainment. We investigated interactions of respondents educational attainment, occupation, income, and family status with simila vignette characteristics. None was significant.

When we examined the interaction of characteristics of the vignett members, we found that the raters responded to certain combinations o characteristics in a manner somewhat different than would be predicte from an additive model. Generally, we found that raters rewarded familie in which there were certain combinations of high achievements but did no dock families in which similar combinations of low achievements occurrec The patterns which were rewarded are not surprising. Families in whic both husband and wife did exceptionally well occupationally are rewarde for this. Such families receive higher ratings (albeit only very slightl higher) than would be predicted by adding the individual effects of hus band's and wife's occupations. Similar patterns obtain for either husban or wife when there is exceptionally high educational attainment combine with occupational attainment. While combinations such as these are nc at all uncommon empirically, raters recognize them as special by awardin them some very slight extra prestige.

An interesting pattern was revealed for the interaction of the wife' occupation with that of her father. When the wife in a family follows he father in attaining high occupational prestige, her family gains some statu benefit. Not so, however, for men who do the same thing—a pattern whic is relatively more common. Respondents reward families in which a woma pursues the unusual pattern of following her father into a highly prestigiou occupation.

The actual amount of extra reward given to families for such achieve ments varies depending upon the type of interaction, but when such term are included in the equations reported in table 2 the predicted score fc families is not altered appreciably. This is because the components of th interaction term are affected by the inclusion of the interaction term in th equation. Thus, for example, while we found some interaction effects du to the interaction of husband's education and husband's occupation, th additional increment due to this was approximately compensated for b the lessened effect of both education and occupation independently (se Allison 1977).

CONSENSUS

Up to this point, we have assumed that the cognitive model responden use to make status-attribution decisions is identical (or at least very sim lar) from one respondent to the next. This is, however, an assumption c great consequence. Recall that the original model posited ratings of hous

holds to be a function, in part, of characteristics of the raters. The very important issue we need to investigate is whether status attribution is, in fact, a collectively agreed-upon process. For it may be that each individual does possess in his head a model which he uses in making status judgments, and it may further be that we can model such a process perfectly for any individual. However, if such models differ idiosyncratically from one individual to the next, our model is of very little utility. Social status must necessarily be collectively agreed upon for it to exist as a dimension of social stratification. Considerable disagreement among respondents about the ratings given to comparable vignettes, or the principles invoked in arriving at these ratings, would suggest that prestige or social status is not a meaningful social fact.

The question we ask is whether our equations reported in table 2 are accurate for all members of our society. Regression equations were estimated in which one randomly selected vignette for each respondent was selected and the rating given it estimated as a function of respondent characteristics such as sex, race, age, education, occupation, and the like. Only one vignette per respondent was used, to avoid entering respondent information redundantly into equations. Five such samples of vignettes were chosen; none revealed any appreciable effect associated with such characteristics.

There is a tremendous dissimilarity between systematic difference associated with respondent characteristics and agreement among respondents. It is possible that each respondent employed a fundamentally different scheme to arrive at evaluations. It has recently been argued that this is the way such evaluations are made (see Baker 1977). If we were to find considerable disagreement on the task of making status judgments we would have demonstrated that social status (as we use the term) is not a meaningful collectively defined social fact.

Our final effort, therefore, will be to evaluate the structure of each individual rater's equation to determine whether there is lack of evidence for the existence of a widely shared cognitive process which allocates units into varying levels of social status. This is the most fundamental issue we must face. It is best stated as follows: A number of individuals made a number of status ratings. We presume that some single process underlies these judgments. It is this process which we are attempting to capture. It could be argued that there is not one consensual model, but that each rater employs his own (perhaps unique) model of social status evaluations. If this is true our equations (table 2) are models computed by averaging individual ratings. We could, however, compute the model for each individual and then average these models, in which case we would have equations computed by averaging individual models of status attribution. The question is whether the two are the same. If they are, one model is adequate

to represent the status-evaluation process. If not, we must conclude that different cognitive processes operate among respondent raters. It is to this very important question that we now turn. Again, we are comparing a model of average ratings with an average of individual models, much as we might compare the total sum of squares with the within sum of squares in an analysis of variance framework. The reason is that although we have not uncovered disagreement among respondents it may be that what disagreement exists is simply not related to any measurable characteristics.

In table 3 we present the univariate statistics summarizing regression equations run for each respondent in the sample. The HPRES formulation of the rating was regressed upon the 10 family characteristics represented on the vignettes sorted by each respondent. Since ZPRES standardizes HPRES to each respondent's mean and variance in ratings, the ZPRES formulation will always produce identical results as the HPRES formulation for any single respondent. Thus we do not present ZPRES results.

First, note that there is considerable variance in the amount of explained variance of these equations, ranging from 0% to 93%. The median figure of 0.527 is much more understandable since there were some respondents who did not introduce any variance in their ratings but rated all vignettes at the same value. Thus to the extent that explained variance can be taken as a measure of agreement among raters, we see that there apparently was some disagreement since the R^2 associated with the total equation (eq. [2]) in table 2 is .26 and the median figure is 0.53. While the incremental increase in R^2 is considerable, almost half of the total variance is still left

TABLE 3

UNIVARIATE STATISTICS FOR PARAMETER ESTIMATES OF LINEAR MODELS FOR 536 RESPONDENTS

Parameter	Mean	Median	Standard Deviation	Minimum	Maximum
Education (years):					
Husband	1 199	944	1 543	−6 751	7 651
Husband's father	.177	137	957	−4 006	6 146
Wife	571	510	1 188	−8 066	5 742
Wife's father	087	141	045	−6 491	3 141
Occupational prestige:*					
Husband	.477	445	373	−1 034	2 838
Husband's father	071	.051	.257	−1.796	1 263
Wife	241	237	.314	−1.330	1 329
Wife's father	.070	.049	226	−0.876	0 996
Ethnicity†	.082	072	312	−1 239	2 303
Migration status‡	1 140	.517	5 065	−9 597	50 280
Constant	−28.757	−10 951	201 123	−988 315	1,065 494
R^2	503	527	176	000	934

* Occupational prestige scores from Ornstein 1977.
† Ethnicity scores derived by Hodge, Siegel, and Rossi 1964.
‡ Categoric variable scaled to continuous variable (see text).

unexplained. However, the results just presented demonstrate that approximately 27% of the variance in ratings is due to idiosyncratic differences among respondents. Does this mean that the equation presented in table 2 is not correct in depicting the empirical rating principles? It may. However, before making such a judgment we must examine the structure of the differences among the raters: Even minor differences among raters over 26,000 vignettes would substantially reduce the overall R^2. In other words, the equations of the raters are more important than is the amount of explained variance. Taking the median as the best indicator of central tendency in this case, the second column in table 3 represents the "super-averaged" model (i.e., an average of individual equations) to be compared with the similar HPRES equation of table 2. How similar are these two equations? Since the results in table 3 are not truly an equation but rather the averages of equations, there is no readily available criterion of structural similarity. For our purposes, however, we may treat them as though both were equations.

The differences in parameter estimates which are the largest in an absolute sense are not particularly important variables for the total equation. For example, we see that the b's on migration status are somewhat different, 0.517 for the average of models and 0.750 for the average of ratings. Recall, however, that this variable is not important to household prestige, thus the difference is of little import. Those parameters most important to household prestige are virtually identical from one equation to the other. For example, the slope on husband's occupation is 0.483 in table 2 and 0.445 in the average of models. This difference is sufficient to produce a difference of 3.10 HPRES points for maximum values of the variable. As a matter of fact, with the single exception of migration status, no two estimates differ by amounts sufficient to produce as much as five-point differences. This variable, it will be recalled, was constructed from the results of regressing HPRES on all categories of migration and then assigning to each its slope. Thus it is not necessarily consistent with the process used by any one respondent. It cannot be used in equations for each respondent as it uses up 25 degrees of freedom and each respondent only rated (at most) 50 vignettes. Thus we have no way of knowing at this juncture whether respondents truly differ in their perception of the variable coded as migration status. What we do know is that no single parameter estimate other than this one is different from the total equation presented in table 2 by sufficient amounts to produce even a three-point difference in the estimated score for HPRES. In effect, the two equations are identical. However, we should note that there is a considerable difference in the value of the constants for the equation. Also, the standard deviation on this estimate is considerable—so large in fact that we really do not know what to consider its value. The respondents varied so much

on this issue that the constant of −10.95 is only a vague indication of its value. As a parameter estimate, the constant tells us the adjustment to the estimated value of the dependent variable (HPRES) which would be made when all other predictors were at their minimum to bring the dependent variable value to the origin of the regression plane. If we were to consider this problem within an analysis of covariance framework, we would consider each respondent as representing a vector upon which all other respondents were scored zero and the respondent was scored one—a dummy variable for each respondent. We see that such vectors would have insignificant coefficients but the constant would differ somewhat from an equation without such vectors. What does all this mean?

Basically, the results in table 3 indicate that respondents agreed quite closely on the relative importance of the family characteristics. They disagreed on the starting point of the scale of family social standing, however. This is not the same as differing on the mean rating given to a set of vignettes. We know that respondents differed in their mean ratings. Given the same mean, however, two respondents could use different values as origins. The differences among respondents on the constant in the equation indicate that this is what happened; but ZPRES would not capture this process since two respondents might have similar mean ratings while having considerably different origins. Such differences are unimportant for our purposes, the only observable effect of locating such differences being an increase in R^2. What is important is the extent to which the respondents agreed on the process of status attribution. While no statistical test of such agreement is available which would allow us to argue this point, we assert that since no differences in coefficients are large enough to produce even three points in HPRES_{est} (save that for migration status), we are looking at identical equations.

We have presented a considerable amount of evidence to suggest that the process of status attribution proceeds along consensually agreed-upon lines. Respondent characteristics are not associated with variance in ratings; different groups of the sample do not produce different equations, and equations representing the rating process of each respondent, while differing somewhat, do not differ enough to make us consider the attribution process individually idiosyncratic. Social status as we have measured it is the product of consensual judgments.

CONCLUSIONS

This research was designed to assess the relative importance of ascribed and achieved characteristics of household members in determining the overall social standing of the household. The principal finding is that parental achievements have appreciable direct and independent effects in deter-

mining the social standing of a nuclear-family couple. The prestige standing of a couple could be altered by 10 points by the occupations of the couple's fathers. Thus, while certainly not one of the most important status characteristics in our society, ascriptive status due to one's father's occupation is important in determining the amount of prestige enjoyed by one's family. Thus parental achievement has independent status-attribution value in addition to the well-known effect it has on status attainment (c.f. Blau and Duncan 1967).

While there is an ascriptive strain to status attribution in America, we do not wish to overstate the case. By far the most important determinants of family social standing are achieved characteristics of the nuclear-family couple. Despite parents' occupations, the achievements of the husband and wife are the principal determinants of the social standing of their family The emphasis on ascription pales when compared with the emphasis on achievement.

We found that husbands and wives contribute unequally to the social standing of their households in the public eye. The husband makes about twice the contribution of his wife via education and/or occupational attainments. Even so, the husband alone is clearly not responsible for family prestige. As a matter of fact, his achievements were found to contribute only a maximum of 50% to overall family social standing. The wife, through her occupation and education, makes sizable and significant contributions to the social standing of the household, potentially contributing as much as 40% to the overall evaluation. This underscores similar findings reported by several researchers (Rossi et al. 1974; Sampson and Rossi 1975; Nock and Rossi, in press). The status of families in our society cannot be summarized by any single member. While the husband is the principal status bearer (see Parsons 1949), he is not solely responsible for the position of his family.

We have found that a number of family characteristics are irrelevant to the status-attribution process. Parental education, for example, was found to have little effect in this process. Similarly, our attempt to indicate areal location or migration history was unsuccessful. Also, it does not matter what a couple's national origin is. Thus, despite the fact that a well-known and stable hierarchy of ethnicities exists in our society, we found that such information is simply not relevant when more detailed information is available.

Finally, and perhaps most important, we found that the hierarchy of social status is well defined and agreed upon by members of our society. Extensive analysis failed to reveal significant differences among individuals in how they arrived at their evaluations of households. We found very little disagreement about how social status was to be reckoned, and there were no conspicuous subgroups in our sample for whom social status of families

depended upon a different pattern of characteristics or a fundamentally different weighting scheme.

This research served two principal purposes: to demonstrate that family social status does contain an element of ascription and to demonstrate that social status judgments are made along consensually agreed-upon lines and follow understandable, predictable rules.

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Human Values and the Market: The Case of Life Insurance and Death in 19th-Century America[1]

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Qualitative analysis of historical data concerning the diffusion of life insurance in the United States during the 19th century helps to explore the problem of establishing monetary equivalents for those aspects of the social order, such as death, that are culturally defined as above financial relationships. The financial evaluation of a man's life introduced by the life insurance industry was initially rejected by many as a profanation which transformed the sacred event of death into a vulgar commodity. By the latter part of the 19th century, the economic definition of the value of death became finally more acceptable, legitimating the life insurance enterprise. However, the monetary evaluation of death did not desacralize it; life insurance emerged as a new form of ritual with which to face death.

For Durkheim and Simmel, one of the most significant alterations in the moral values of modern society has been the sacralization of the human being, his emergence as the "holy of holies" (Wallwork 1972, p. 145; Simmel 1900). In his *Philosophie des Geldes,* Simmel (1900) traces the transition from a belief system that condoned the monetary evaluation of life to the Judeo-Christian conception of the absolute value of man, a conception that sets life above financial considerations. The early utilitarian criterion was reflected in social arrangements, such as slavery, marriage by purchase, and the *wergeld* or blood money. The rise of individualism was the determining factor in the transition. "The tendency of money to strive after ever-growing indifference and mere quantitative significance coincides with the ever-growing differentiation of men . . . and thus money becomes less and less adequate to personal values" (Altmann 1903, p. 58).[2] For Simmel, money the equalizer became money the profaner. Considered "sub specie pecuniae," the uniqueness and dignity of human life vanished.

Only small fragments of Simmel's penetrating analysis of personal and

[1] I am deeply grateful for the generous advice and support of Professors Sigmund Diamond and Bernard Barber I also want to thank Professor Irving Louis Horowitz for his help, and an anonymous reviewer of the *American Journal of Sociology* for very useful suggestions.

[2] Parsons and Lidz (1967, p. 163) also attach the conception of the sanctity of life to the stress on individualism

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monetary values have been translated, and, with a few exceptions, this work has been ignored in the sociological literature.[3] There has been much generalizing about the "cash nexus" but, strangely, very little work on the area. The problem of establishing monetary equivalences for such things as death, life, human organs, and generally ritualized items or behavior considered sacred and, therefore, beyond the pale of monetary definition is as intriguing as it is understudied. Perhaps the absorption of many social scientists with "market" models and the notion of economic man led them and others to disregard certain complexities in the interaction between the market and human values.[4] Market exchange, although perfectly compatible with the modern values of efficiency and equality, conflicts with human values which defy its impersonal, rational, and economizing influence. Titmuss's imaginative cross-national comparison of voluntary and commercial systems of providing human blood for transfusions stands as a lone effort to consider this conflict in depth. His study suggests that commercial systems of distributing blood are not only less efficient than voluntary blood donation but also, and more important, morally unacceptable and dangerous to the social order. Transform blood into a commercial commodity, argues Titmuss, and soon it will become "morally acceptable for a myriad of other human activities and relationships also to exchange for dollars and pounds" (1971, p. 198).[5] Dissatisfied with the consequences of market exchange, Titmuss is persuaded that only reciprocal or gift forms of exchange are suitable for certain items or activities: among others, blood transfusions, organ transplants, foster care, and participation in medical experimentation. His resistance to the laws of the marketplace is not unique. In his early writings, Marx was already concerned with the dehumanizing impact of money. In *The Economic and Philosophic Manuscripts* Marx deplored the fact that in bourgeois society human life is easily reduced to a mere salable commodity; he pointed to prostitution and the sale of persons which flourished in his time as ultimate examples of this degrading process (1964, p. 151).[6] Similarly, Blau, despite his predominantly "market" model of social behavior, states that "by supplying goods that moral standards define as invaluable for a price in the market, individuals prostitute themselves and destroy the cen-

[3] For English versions of some portions of the book, see Becker (1959), Altmann (1903), Levine (1971), Lawrence (1976), and Etzkorn (1968).

[4] On the "absolutization" of the market as an analytical tool for social analysis in most social science disciplines, and for a discussion of the types and functions of different forms of economic and social exchange, see Barber (1974).

[5] According to a recent report, the nation appears to be shifting toward almost total reliance on volunteer, nonpaid donors (*New York Times*, June 19, 1977).

[6] See also the *Manifesto of the Communist Party* (Marx 1971, p. 11). Above all, money for Marx (1964, pp. 165–69) destroys individuality by enabling its possessor to achieve objects and qualities which bear no connection to individual talents or capacities.

tral value of what they have to offer" (1967, p. 63). Using love and salvation as examples, Blau suggests that pricing intangible spiritual benefits inevitably leaves some unwholesome by-product; not love but prostitution, not spiritual blessing but simony.[7] The marketing of human organs presents a similar dilemma. Significantly, while organ donations have become more common, organ sales are still rare.[8] Parsons, Fox, and Lidz note that "regardless of how scientific the setting in which this transaction occurs may be, or how secularized the beliefs of those who take part in it, deep religious elements . . . are at least latently present in the transplant situation" (1973, p 46). Likewise, even after the repeal of most prohibitions against the sale of corpses, the majority of medical schools still obtain corpses and cadavers through individual donations and unclaimed bodies from the morgue. People refuse to sell their bodies for "ethical, religious or sentimental reasons" ("Tax Consequences of Transfers of Bodily Parts," 1973, pp. 862–63). The law itself remains ambivalent. While the Uniform Anatomical Gift Act permits the gift of one's body or organs after death, "the state of the law on anatomical sales remains in a flux" ("Tax Consequences of Transfers of Bodily Parts," 1973, p. 854).

This paper uses data concerning the diffusion of life insurance in 19th-century America as a testing ground to explore the larger theoretical problem of establishing monetary equivalences for sacred things. Our hypothesis is that cultural resistance to including certain items in the social order—namely, those related to human life, death, and emotions—into a market-type of exchange introduces structural sources of strain and ambivalence into their marketing. Life insurance raises the issue in its sharpest terms by posing the question of how one establishes a fixed-dollar amount for any individual death.

Life insurance was part of a general movement to rationalize and formalize the management of death that began in the early part of the 19th century. In the 18th century, the widow and her orphans were assisted by their neighbors and relatives as well as by mutual aid groups that ministered to the economic hardships of the bereaved. In the 19th century, the financial protection of American families became a purchasable commodity. Trust

[7] Cooley formulated another, different perspective on the "moral problem" created by the fact that "pecuniary values fail to express the higher life of society." Although he accepted the fact that human values such as love, beauty, and righteousness were not traditional market commodities, Cooley rejected the permanent segregation of pecuniary values into a special, inferior province of life. His alternative was the enhancement of monetary evaluation; precisely by encouraging "the translation into it of the higher values the principle that everything has its price should be rather enlarged than restricted" (1913, pp. 202–3).

[8] A recent policy-oriented analysis of organ transplants concludes that "if the body is to be made available to others for personal or societal research, it must be a gift" (Veatch 1976, p. 269).

companies, like life insurance companies, replaced more informal systems with professional management (White 1955). The funeral was another "family and neighborhood" affair that became a business. Previously, the physical care and disposal of the dead had been provided mostly by neighbors and relatives, but in the 19th century it became a financially rewarded occupational specialty (Bowman 1959; Habenstein and Lamers 1955). The process of formalization extended to the drafting of wills. The largely informal, generalized provisions drafted by a man shortly before his death turned into a highly structured system of estate planning in the 19th century (Friedman 1964).

The new institutions were primarily concerned with death as a major financial episode. Their business was to make people plan and discuss death in monetary terms. Life insurance defined itself as "the capitalization of affection. . . . Tears are nothing but salt water, to preserve a fresh grief. Insurance is business, genuine, old-fashioned sixteen-ounce precaution" (Phelps 1895, pp. 12–13). Its avowed goal was to encourage men to "make their own death the basis of commercial action" (Beecher 1870). This was no simple enterprise. Putting death on the market offended a system of values that upheld the sanctity of human life and its incommensurability It defied a powerful normative pattern: the division between the nonmarketable and the marketable, or between the sacred and the profane Durkheim has written, "The mind irresistibly refuses to allow the two [sacred and profane] . . . to be confounded or even merely to be put into contact with each other . . ." (1965, p. 55). Sacred things are distinguished by the fact that men will not treat them in a calculating, utilitarian manner

I will argue that resistance to life insurance in this country during the earlier part of the 19th century was largely the result of a value system that condemned the materialistic assessment of death, and of the power of magical beliefs and superstitions that viewed with apprehension any commercial pacts dependent on death for their fulfillment. By the latter part of the 19th century, the economic definition of the value of death became finally more acceptable, legitimating the life insurance enterprise. However, our data suggest that the monetary evaluation of death did not desacralize it; far from "profaning" life and death, money became ritualized by its association with them. Life insurance took on symbolic values quite distinct from its utilitarian function, emerging as a new form of ritual with which to face death and a processing of the dead by those kin left behind.

The present study is based on a qualitative analysis of historical documentary sources. The attempt was made to include an extensive and diversified set of different kinds of data. Among the primary sources consulted were advertising booklets published by life insurance companies, insurance journals and magazines, early treatises and textbooks on insurance, life insurance agents' manuals and their memoirs. Although these sources repre-

sent predominantly the life insurance industry and not its customers, they provide important indicators of public opinion. For instance, the most prevalent objections against life insurance were repeatedly discussed and carefully answered by contemporary advertising copy. Primary sources outside the life insurance industry were consulted as well, among them 19th-century business periodicals and general magazines, widows' and marriage manuals, booklets written by critics of life insurance, and a series of government documents.

A BRIEF BACKGROUND

The first life insurance organizations in the United States were formed during the latter years of the 18th century to assuage the economic distress of the widows and orphans of low-paid Presbyterian and Episcopalian ministers. The idea soon appealed to the secular community, and by the early decades of the 19th century several companies had optimistically undertaken the business of insuring life. Legislatures were encouraging; special charters for the organization of the new companies were granted rapidly and eagerly by many states. Life insurance seemed the perfect solution to the increasing economic destitution of widows and orphans. The public, however, did not respond. Surprised and dismayed by their failure, many pioneering companies withdrew altogether or else turned to other businesses to compensate for their losses in life insurance. The contrasting success of savings banks and trust companies, as well as the prosperity of fire and marine insurance companies, attests to the fact that there was sufficient disposable income among the population at the beginning of the 19th century In addition, the early companies offered a solid economic organization; no life insurance company failed before the 1850s. Epidemics and high mortality rates did not affect their stability; actuarial knowledge was sufficient to calculate adequate premium rates Americans were offered sound policies which they needed and could well afford. They did not, however, want them.

After the 1840s there was a drastic reversal of trends, and life insurance began its fantastic history of financial success, becoming firmly established in the 1870s. Its sudden prosperity has puzzled insurance historians as much as the initial failure of the industry. The new companies were offering the same product; neither rates nor conditions of life insurance policies were significantly improved. Most analysts point to America's stage of economic growth as the major clue to the acceptance of life insurance. The great economic expansion that began in the 1840s and reached its peak in the 1860s explains the boom of life insurance at that time. The increased urbanization of mid-century America is also upheld as an explanation. Urban dependance on daily wages has been particularly linked to the growing acceptance of life insurance. Indeed, the acceleration of urbanization

coincided in many states with the growth of life insurance. The percentage of people living in urban areas doubled between 1840 and 1860, with the greatest increase occurring in New York and Philadelphia, two cities in leading insurance states. The first life insurance companies were all organized in such heavily populated cities as New York, Philadelphia, Boston, and Baltimore.[9]

Other insurance historians, notably Stalson (1969), argue that the "rags-to-riches" transformation of life insurance in mid-century can be attributed unequivocally to the adoption of aggressive marketing techniques. Pioneer American life insurance companies used no agents, limiting themselves to passive marketing tactics such as discreet announcement advertisements. In the 1840s, the new companies introduced person-to-person solicitation by thousands of active, high-pressure salesmen who went into the homes and offices of prospective customers. Marketing systems, however, do not develop in a sociological vaccum. Their structure and characteristics are deeply interrelated with such other variables as customers' social and cultural backgrounds. The struggles and victories of life insurance have remained enigmatic and misunderstood because existing interpretations systematically overlook the noneconomic factors involved in its acceptance and adoption. Indeed, economists and economic historians monopolize the field, while sociologists for the most part have ignored it.[10]

In the first place, the development of the insurance industry reflects the struggle between fundamentalist and modernistic religious outlooks that worked itself out in the 19th century. Contrasting theological perspectives divided the clergy into opposing groups; there were those who denounced life insurance to their congregations as a secular and sacrilegious device that competed against God in caring for the welfare of widows and orphans. Others, more attuned to the entrepreneurial spirit, supported the industry. The cultural incompatibility of life insurance with literalist and fundamentalist beliefs hindered its development during the first part of the century. In opposition, the emerging liberal theology tended to legitimate the enterprise. Religious liberals supported insurance programs for practical considerations as well. Congregations which had been unwilling to raise the meager salaries of their underpaid pastors and ministers were most easily persuaded to pay the relatively small premiums to insure the life of the clergymen.

Changing ideologies of risk and speculation also influenced the develop-

[9] On the impact of economic growth and urbanization on the development of life insurance, see, among others, Buley (1967), North and Davis (1971), and Mannes (1932).

[10] There are a few exceptions. See, e.g., Riley (1963). An entire issue of the *American Behavior Scientist* (May 1963) was devoted to social research and life insurance. Two doctoral dissertations have been written on the life insurance agent (Taylor 1958, Bain 1959).

ment of life insurance. Many practices considered to be deviant speculative ventures by a traditional economic morality were redeemed and transformed into legitimate, even noble investments by a different entrepreneurial ethos. Much of the opposition to life insurance resulted from the apparently speculative nature of the enterprise; the insured were seen as "betting" with their lives against the company. The instant wealth reaped by a widow who cashed her policy seemed suspiciously similar to the proceeds of a winning lottery ticket. Traditionalists upheld savings banks as a more honorable economic institution than life insurance because money was accumulated gradually and soberly. After the 1870s, as the notions of economic risk and rational speculation grew progressively more acceptable, the slower methods of achieving wealth lost some of their luster, and life insurance gained prominence and moral respectability.

The emergence of life insurance is also clearly tied to functional changes in the family system which resulted from urbanization. The urban family could no longer rely on informal, personal social arrangements in times of crisis. The care of widows and orphans, previously the responsibility of the community, became the obligation of the nuclear family with the assistance of formal, impersonal, bureaucratic mechanisms and paid professionals. Life insurance was the institutional response to the uncertain social and economic situation of a new commercial middle class without property and dependent exclusively on the money income of the father. Nineteenth-century writings clearly reflect the prevalent fear among businessmen of failure and downward mobility, if not for themselves, for their children.[11]

Finally, changing attitudes toward death made a major impact on the development of life insurance. Life insurance clashed with a value system that rejected any monetary evaluation of human life. However, by the latter part of the 19th century, a growing awareness of the economic value of death legitimated the life insurance business.

PROFANE MONEY

The resistance to evaluating human beings in monetary terms is among the major cultural factors either ignored by life insurance analysts or else dismissed in their historical accounts as a curious but certainly peripheral issue. Yet its centrality in Western culture is hardly disputable. Cultural aversion to treating life and death as commercial items is reflected in legal attempts to safeguard them from economic valuation. Roman law had early established the doctrine: Liberum corpus nullam recipit aestimationem (the life of a free man can have no monetary estimate) (Goupil 1905, pp. 32–

[11] On the fear of failure among 19th-century businessmen, see Katz (1975). For a fuller explanation of the cultural and sociostructural factors involved in the adoption of life insurance, see Zelizer (1979).

33).[12] Successorial contracts were considered "stipulationes odiosae" and "contra bonos mores" because they surrounded death with financial considerations. Roman tradition was perpetuated in many countries, particularly in France, where the Civil Code ruled that "only things belonging to commerce can be the subject of a contract" (Pascan 1907, p. 2). Declaring that a man's life "cannot be the subject of commercial speculation," French jurists prohibited any contract on the lives of persons, such as life insurance trusts, and successorial contracts. Wills, sufficiently surrounded by religious symbolism to remain untainted by commercial aspirations, remained the only legitimate vehicle to dispose of property after death (Goupil 1905 p. 139).

In the United States, the utilitarian treatment of human lives poses similar problems. American law protects human life from commerce, declaring that the human body is not property and may not be "bargained for, bartered or sold" (Schultz 1930, p. 5). Many social arrangements, regardless of their economic efficiency, have been condemned as offensive to the sacred qualities of life. Life insurance became the first large-scale enterprise in America to base its entire organization on the accurate estimate of the price of death. It was necessary to know the cost of death in order to establish adequate policy benefits and determine premiums. The economic evaluation of human life was a delicate matter which met with stubborn resistance. Particularly, although not exclusively, during the first half of the 19th century, life insurance was felt to be sacrilegious because its ultimate function was to compensate the loss of a father and a husband with a check to his widow and orphans. Critics objected that this turned man's sacred life into an "article of merchandise" (Albree 1870, p. 18). They asked, "Has a man the right to make the continuance of his life the basis of a bargain? Is it not turning a very solemn thing into a mere commercial transaction?" (Beecher 1870). Mennonites, who went to the extreme of excommunicating any member who insured his life, cited similar reasons: "It is equivalent to merchandising in human life; it is putting a monetary price on human life which is considered unscriptural since man is the 'temple of the Holy Ghost'" (*Mennonite Encyclopedia* 1957, p. 343). Life insurance benefits, however profitable, became "dirty money" (Knapp 1851).

MAGICAL MONEY

Whal notes the "remarkable paradox of an almost universal recourse to magic and irrationality" to handle death even among the most firm believers in science and the scientific method (1959, p. 17). But while examples of

[12] Only slaves were considered to have pecuniary value This explains why countries that forbade life insurance in principle allowed the insurance of slaves. Their lack of human value justified economic equivalences without presenting serious moral difficulties (Reboul 1909, p 23)

the relationship of magic to death in less-developed cultures are easily found (see Malinowski 1954; Habenstein and Lamers 1955; Simmons 1945; Blauner 1966), little is known about contemporary magic rituals.

For instance, few people make plans for their own death, largely because of magical fears that to do so will hasten it. Most wills are drafted shortly before death (Dunham 1963). Likewise, people rarely prearrange their own funerals despite the evidence that this reduces expenses considerably (Simmons 1975).

Its commercial intimacy with death made life insurance vulnerable to objections based on magical reasoning. A New York Life Insurance Co. newsletter (1869, p. 3) referred to the "secret fear" many customers were reluctant to confess: "the mysterious connection between insuring life and losing life." The lists compiled by insurance companies in an effort to respond to criticism quoted their customers' apprehensions about insuring their lives: "I have a dread of it, a superstition that I may die the sooner" (*United States Insurance Gazette* [November 1859], p. 19). Responding to the popular suspicion that life insurance would "hasten the event about which it calculates," Jencks urged the necessity to "disabuse the public mind of such nonsense" (1843, p. 111). However, as late as the 1870s, "the old feeling that by taking out an insurance policy we do somehow challenge an interview with the 'king of terrors' still reigns in full force in many circles" (*Duty and Prejudice* 1870, p. 3).

Insurance publications were forced to reply to these superstitious fears. They reassured their customers that "life insurance cannot affect the fact of one's death at an appointed time" (*Duty and Prejudice* 1870, p. 3). Sometimes they answered one magical fear with another, suggesting that not to insure was "inviting the vengeance of Providence" (Pompilly 1869). The audience for much of this literature was women. It is one of the paradoxes in the history of life insurance that women, intended to be the chief beneficiaries of the new system, became instead its most stubborn enemies. An Equitable Life Assurance booklet quoted wives' most prevalent objections: "Every cent of it would seem to me to be the price of your life. . . . It would make me miserable to think that I were to receive money by your death. . . . It seems to me that if [you] were to take a policy [you] would be brought home dead the next day" (June 1867, p. 3).

Thus, as a result of its commercial involvement with death, life insurance was forced to grapple with magic and superstition, issues supposedly remote from the kind of rational economic organization it represented.

SACRED MONEY

Until the late 19th century, life insurance shunned economic terminology, surrounding itself with religious symbolism and advertising more its moral

value than its monetary benefits. Life insurance was marketed as an altrui tic, self-denying gift rather than as a profitable investment. Most life i surance writers of this period denied the economic implications of the enterprise: "The term life insurance is a misnomer . . . it implies a valı put on human life. But that is not our province. We recognize that life intrinsically sacred and immeasurable, that it stands socially, morally, ar religiously above all possible evaluation" (Holwig 1856, p. 4).

Later in the 19th century, the economic value of human life finally b came a less embarrassing topic in insurance circles. The *United States I surance Gazette* could suggest, "The life of every man has a value; n merely a moral value weighed in the scale of social affection and family ti but a value which may be measured in money" (May 1868, p. 2).[13] Tł Rev. Henry Ward Beecher (1870, p. 2) urged men to make their death "tl basis of commercial action." The process of introducing the economic valı of human life culminated in 1924 when the concept was formally present at the annual convention of life underwriters: "The most important ne development in economic thought will be the recognition of the econom value of human life. . . . I confidently believe that the time is not fa distant when . . . we shall apply to the economic organization, manageme and conservation of life values the same scientific treatment that we no use in connection with property" (Huebner 1924, p. 18).

Death was redefined by the new economic terminology as "all even ending the human life earning capacity" (Huebner 1959, p. 22). It wa neatly categorized into premature death, casket death, living death (di ability), and economic death (retirement). From this perspective, disea was the "depreciation of life values" (Dublin and Lotka 1930, p. 112) an premature death an unnecessary waste of money. In 1930, Dublin an Lotka developed the first estimate of capital values of males as a functio of their age. By establishing differential financial values for lives, they al set a new criterion for stratifying them. Exceptional lives were those th made the greatest contributions, while substandard lives burdened the communities with financial loss (Dublin and Lotka 1930, pp. 80–82)

[13] The greater acceptance of the economic value of a man's life did not include wom The *Insurance Monitor*, among others, was outspoken against insuring wives for tl benefit of husbands: "The husband who can deliberately set a money value upon h wife, is so far destitute not only of affection for her, but of respect for himself T him she is but a chattel . . ." ("The Insurable Value of a Wife" [September 1870], 712d). The insurance of children was similarly opposed by many individuals and o ganizations who objected to the economic evaluation of a child's life. In the 1870 industrial insurance companies began insuring the poor. For the first time childr under 10 years of age were insured on a regular basis There were at least 70 legislati attempts in various states to prohibit it as being against public policy and the publ interest. The *Boston Evening Transcript* reflected their prevalent feeling that "no man man and no womanly woman should be ready to say that their infants have pecunia value" (March 14, 1895).

is claimed that the rational-utilitarian approach to death typified by life insurance has deritualized and secularized death (Vernon 1970; Gorer 1965). Death, however, is not tamed easily. Keener observers deny the hypothesis of deritualization and see instead the secularization of religious ritual (Faunce and Fulton 1957; Pine and Phillips 1970; Blauner 1966). This "metamorphosis of the sacred" (Brown 1959, p. 253) does not exempt ritual but changes its nature. The dead can be mourned in very different ways. Paradoxically, money that corrupts can also redeem: dollars can substitute for prayers.

Brown (1959) criticizes traditional sociology for perpetuating a secular and rational image of money without paying due attention to its symbolic and sacred functions (pp. 239–48). There is a dual relationship between money and death, actual or symbolic. While establishing an exact monetary equivalence for human life represents a profanation of the sacred, the symbolic, unrestrained use of money may contribute to the sanctification of death. Durkheim briefly dwells on the sacred qualities of money: "Economic value is a sort of power of efficacy and we know the religious origins of the idea of power. Also richness confers mana, therefore it has it. Hence, it is seen that the ideas of economic value and of religious value are not without connection" (1965, p. 466). The widespread practice of spending large sums of money at times of death testifies to the existence of a powerful and legitimate symbolic association between money and death. Expensive funerals are held without regard to the financial position of the deceased (Dunham 1963). Accusing fingers point routinely at the undertakers, blaming unreasonable expenses on their exorbitant prices (Mitford 1963; Harmer 1963). Historical evidence, however, shows that high expenditures at the time of death preceded the rise of the professional undertaker in the 19th century. Haberstein and Lamers describe the "wanton lavishness" of 18th century funerals, when gloves, scarves, and all kinds of expensive gifts were distributed (1955, p. 203). The symbolic ties between money and death are also revealed by the norm that proscribes bargaining at times of death (Simmons 1975). Comparison shopping for funerals is strictly taboo, even though it reduces costs. Similarly, in the case of life insurance, "to count our pennies is tempting the Gods to blast us" (Gollin 1969, p. 210). Parsons and Lidz suggest that spending large sums of money may be an attempt to affect "the ultimate well being, or even the salvation of the deceased soul" (1967, p. 156).

When it comes to death, money transcends its exchange value and incorporates symbolic meanings. The dual relationship between money and death—actual as well as symbolic—is essential to the understanding of the development of life insurance. Sacrilegious because it equated cash with life, life insurance became on the other hand a legitimate vehicle for the symbolic use of money at the time of death. We will briefly examine three dif-

ferent aspects of the ritualization of life insurance: its emergence as a secular ritual, as an additional requirement for a "good death", and as a form of immortality.

LIFE INSURANCE AS RITUAL

Funeral expenditures have been defined as a secular ritual (Pine and Phillips 1970, p. 138; Bowman 1959, p. 118).[14] Our evidence suggests that life insurance became another one. Curiously, its critics and not its proponents have been particularly sensitive to the ritualistic overtones of life insurance. Among others, Welsh claims that life insurance is a way of coming to terms with death not only financially but also emotionally and religiously (1963, p. 1576).

The view of life insurance as ritual can be substantiated with firmer evidence. From the 1830s to the 1870s life insurance companies explicitly justified their enterprise and based their sales appeal on the quasi-religious nature of their product. Far more than an investment, life insurance was a "protective shield" over the dying, and a consolation "next to that of religion itself" (Holwig 1886, p. 22). The noneconomic functions of a policy were extensive: "It can alleviate the pangs of the bereaved, cheer the heart of the widow and dry the orphans' tears. Yes, it will shed the halo of glory around the memory of him who has been gathered to the bosom of his Father and God" (Franklin 1860, p. 34).

LIFE INSURANCE AND THE "GOOD DEATH"

Most societies have some conception of what constitutes an appropriate death, whether than means dying on a battlefield or while working at a desk. A "triumphant" death in pre–Civil War America meant a holy death, it involved spiritual transportation and the "triumph" of the faith (Saum 1975). Religiosity and moral generosity alone, however, soon became dysfunctional to a changed social context. In the 18th and early 19th centuries, widows and orphans had generally inherited sufficient land to live on and support themselves. Urbanization changed this, making families exclusively dependent on the father's wage. If he did not assume responsibility for the economic welfare of his wife and children after his death, society would have to support them. The principle of testamentary freedom in American law exempted men from any legal obligation to their children after death. Moral suasion, therefore, had to substitute for legal coercion. It was crucial to instill in men a norm of personal financial responsibility

[14] Ariès (1975) sees the contemporary American funeral rite as a compromise between deritualization and traditional forms of mourning. Group therapy and family reunions have also been suggested as secular rituals (Patterson 1975).

toward their families that did not stop with death. More and more a good death meant the wise and generous economic provision of dependents. A man was judged posthumously by his financial foresight as much as by his spiritual qualities. Only the careless father left "naught behind him but the memory of honest, earnest work and the hopeless wish that loved ones . . . might somehow find their needed shelter from poverty. . . ." (*Insurance Journal*, October 1882, p. 313). Diamond (1955) and Goody (1962) point out how attitudes toward death and the dead serve as efficient mechanisms for controlling the behavior of the living. Newspaper obituaries or clergymen's eulogies, for instance, remind the living what behavior is sanctioned by a particular social system. The public reformulation of social norms after a man's death reaffirms their value for the living. Life insurance writings referred to the new standards of dying in America: "The necessity that exists for every head of family to make proper provision for the sustenance of those dear to him after his death, is freely acknowledged and there is no contingency whereby a man stand excused from making such a provision" (*Life Insurance*, journal of the Manhattan Life Insurance Co., 1852, p. 19).

As an efficient mechanism to ensure the economic provision of dependents, life insurance gradually came to be counted among the duties of a good and responsible father. As one mid-century advocate of life insurance put it, the man who dies insured and "with soul sanctified by the deed, wings his way up to the realms of the just, and is gone where the good husbands and the good fathers go" (Knapp 1851, p. 226). Economic standards were endorsed by religious leaders such as Rev. Henry Ward Beecher, who pointed out, "Once the question was: can a Christian man rightfully seek Life Assurance? That day is passed. Now the question is: can a Christian man justify himself in neglecting such a duty?" (1870). The new criteria for a "good death" emerge from this excerpt from a sermon delivered in the 1880s:

> I call to your attention Paul's comparison. Here is one man who through neglect fails to support his family while he lives or after he dies Here is another who abhors the Scriptures and rejects God. . . . Paul says that a man who neglects to care for his household is more obnoxious than a man who rejects the Scriptures. . . When men think of their death they are apt to think of it only in connection with their spiritual welfare. . . It is meanly selfish for you to be so absorbed in heaven . . . that you forget what is to become of your wife and children after you are dead. . . . It is a mean thing for you to go up to Heaven while they go into the poorhouse. [T. DeWitt Talmage, quoted in Hull 1964, p 240]

LIFE INSURANCE AND ECONOMIC IMMORTALITY

Theological concern with personal immortality was replaced in the 19th century by a growing concern with posterity and the social forms of immor-

tality. Carl Becker (1932) points out that as early as the 18th century European *philosophes* replaced the Christian promise of immortality in the afterworld with the belief that good men would live in the memory of future generations. This shift was reflected in the changing nature of wills. Earlier wills were concerned primarily with the spiritual salvation of the dying. The testator regulated all the details of his burial, assuring his chances of salvation by donations to the poor who would pray for his soul and by funding hundreds of thousands of masses and religious services in his honor, often in perpetuity (Vovelle 1974). After the mid-18th century, wills were no longer concerned with matters of personal salvation; they became lay instruments for the distribution of property among descendants. Vovelle attributes the change in wills to the "de-Christianization" and deritualization of attitudes toward death in the mid-18th century. It is likely, however, that the new format of wills was less the reflection of a loss of religious belief than an indicator of a new set of ideas and beliefs on immortality.[15] Feifel describes the transition in America: "When we gave up the old ideas of personal immortality through an afterlife we created the idea of social immortality. It meant that I could not live on but I would live on [*sic*] my children" (1974, p. 34). The Puritan concern with individual salvation was pushed aside by the new emphasis on posterity. Men became preoccupied less with their souls and more with leaving an estate for their heirs. The concern with social immortality interacted with structural pressures generated by new economic conditions and the process of urbanization. The multiplication of people with no more capital than their personal incomes made the economic future of their children painfully precarious. The premature death of the breadwinner spelled economic disaster to his widow and orphans. The new institutions that specialized in the economic consequences of death, such as life insurance and trusts, responded to that economic plight by serving the practical needs of dependents. However, they went beyond mere functionality by also symbolizing a form of economic immortality.

The appeal of life insurance as a pathway to immortality was early recognized by the insurance companies, which used it very explicitly to attract their customers. Life insurance was described as "the unseen hand of the provident father reaching forth from the grave and still nourishing his offspring and keeping together the group" (United States Life Insurance Co. booklet, 1850, p. 5). The idea of rewards and punishments after death also served to reinforce the father's responsibility for his widow and orphans.

[15] Ariès's interpretation of Vovelle's data may have some bearing on this hypothesis. Ariès uses the rise of the family and of new family relationships based on feelings and affection in the mid-18th century to explain the change in wills. The dying person no longer used legal means to regulate the rituals of his burial because he now trusted his family to remember him voluntarily (1974, pp. 64–65). The growing importance of family ties may have encouraged religious belief in posterity and social forms of immortality.

ɔoody suggests that the belief in afterworld retribution, like other superıatural beliefs, reinforces the system of social control over the living by ›lacing it beyond human questioning (1962, pp. 375–78). The uninsured :ould anticipate an uneasy afterlife. The dead also assumed a more active ·ole than in the past; there was a shift from "service to serving" (Goody ¦975, p. 4). They were no longer the passive recipients of their survivors' ›rayers; it was soon recognized that "the desire to outlive life in active ›eneficence is the common motive to which [life insurance] appeals" (Tyng ı881, p. 4).

'ONCLUSION

Иy concern in this paper goes beyond a historical narrative of life insurınce. Using previously unanalyzed aspects of that history, I explore the nore general problem of establishing monetary equivalents for relations or ›rocesses which are defined as being beyond material concerns, a problem ›f long-standing interest in sociological thought. With life insurance, man ınd money, the sacred and the profane, were thrown together; the value of nan became measurable by money. The purely quantitative conception of ıuman beings was acceptable in primitive society where only the gods beonged to the sacred sphere while men remained part of the profane world. The growth of individualism resulted in a new respect for the infinite worth ıf human personality, displacing the earlier utilitarianism with an absolute ·aluation of human beings. In an increasingly industrialized market econımy dominated by the "cash nexus," human life and human feelings were :ulturally segregated into their separate, incommensurable realm. Life in›urance threatened the sanctity of life by pricing it. In the earlier part of he 19th century, the American public was not ready to commercialize leath. Life insurance was rejected as a sacrilegious enterprise.

The task of converting human life and death into commodities is highly 'omplex, creating inescapable sources of structural ambivalence in any -nterprise that deals commercially with such sacred "products." Business lemands profits for survival, yet profits alone remain a justification too base for an institution of its kind. I suggest that one solution, in the case of life insurance, was its "sacralization"; the transformation of the monetary evaluation of death into a ritual. Death yielded to the capitalist ethos -but not without compelling the latter to disguise its materialist mission in spiritual garb. For instance, life insurance assumed the role of a secular ritual and introduced new notions of immortality that emphasized remembrance through money. A "good death" was no longer defined only on moral grounds; the inclusion of a life policy made financial foresight another prerequisite. One finds, in addition to religious legitimation, attempts at moral and social legitimation of the industry. The public was assured that marketing death served the lofty social purpose of combating poverty, thereby

reducing crime. At the individual level, there were moral rewards for tl selfless and altruistic insurance buyer.

This religious, moral, and social legitimation was also true of Americ business in general until the 1870s. Sanford (1958) refers to the "psychic factor of moral justification which distinguished America's industrial pi neers from their European counterparts. American industry was not justi fied by profits alone but as an agency of moral and spiritual uplift. Busine was seen to serve God, character, and culture.[16] But if profit alone was a unacceptable motivation for most commercial enterprises, it was a partic larly unseemly justification for a business, like life insurance, that dea with human life and death. Indeed, by the latter part of the 19th centur when American business felt sufficiently confident to seek no other justi cation than the wealth it produced, life insurance still retained part of i religious camouflage. Even some of the most hard-bitten business leade of the industry slipped into sentimentalism in speaking of life insurance a "conviction first and then a business" (Kingsley 1911, p. 13).

We do not suggest that ingenious sales pitches alone were responsible fo the adoption of life insurance. Its newly acquired legitimate status by th latter part of the 19th century was the result of profound economic, socia and cultural changes in America. Marketing techniques, however, can b useful indirect indicators of cultural values. In the case of life insuranc its earlier moralistic appeal reflected the powerful ideological resistance t commercializing death. As the economic definition of death became finall more acceptable by the latter part of the 19th century, life insurance coul afford a more direct business-like approach to death without, however, full discarding its ritualistic appeal. The pivotal role of the life insurance ager further confirms the cultural struggle of the industry. Life insurance sale began to improve in the 1840s when companies introduced personal solic tation. In sharp contrast to life policies, marine and fire insurance sold wit only minor participation of agents. Customers who would not insure thei lives unless pursued sought voluntarily the protection of their homes an ships. The distinctive role of the agent in life insurance was not simply a ingenious marketing device. It was a response to powerful client resistanc From the data available it is safe to hypothesize that the adoption of lif insurance would have been much slower and far less successful without th agency system. Persuasive and persistent personal solicitation alone coul break through the ideological and superstitious barriers against insurin life.[17] Indeed, historical evidence clearly attests to the failure of all experi

[16] The accumulation of great fortunes was justified by the ultimate social and philar thropic purposes to which the money was put (Diamond 1955, pp 13-15) On th subject, see also Hofstadter (1963, p. 251).

[17] For the impact of personal influence on the diffusion of innovations, see Rogers an Shoemaker (1971); on marketing, see Katz and Lazarsfeld (1955)

ments to sell life insurance directly in this country and abroad.[18] The agent was indispensable. His role, however, was ambiguous. The dilemma of marketing life was again evident in the ambivalent role definition of agents. Death could not be pushed and promoted as a common ware. Official rhetoric urged agents to remain above materialistic concerns, performing their task with the spiritual devotion of a missionary. The rewards, however, went to the successful salesman who solicited the most policies.

Other "businessmen" of death are caught in the same structural ambivalence as life insurance. To undertakers, as to life insurance salesmen, death is a money-making business. As "businessmen" of death they are differentiated from the "professionals" of death, physicians and clergymen, whose connection to death is made legitimate by their service orientation.[19] Parsons (1949) and Merton (1975) distinguish between individual motivational patterns and the institutional structures of business and the professions. Regardless of the individual motivations of the practitioners—their greed or beneficence—professions institutionalize altruism while businesses institutionalize self-interest. Particularly when it comes to death, to save and to heal is holier than to sell. The powerful normative stigma of the utilitarian association of money with death results in a negative evaluation of those involved in making money out of death. In sum, marketing death is what Hughes has instructively called "dirty work" (1958, pp. 49–52). As with life insurers, undertakers attempt to legitimate their business by transforming it into a sacred ritual. Warner describes the tendency on the part of the undertaker "to borrow the ritual and sacred symbols of the minister . . to provide an outward cover for what he is and does. His place of business is not a factory or an office but a 'chapel' or a 'home' " (1959, p. 317).

This paper has shown that the "profanation" of the sacred, such as making money out of death, creates sources of strain and ambivalence in its practitioners which can be assuaged but not resolved by "sacralizing" the profanation. This hypothesis would be enriched by further investigation of the marketing of other similarly "sacred" products such as human organs or even the recently expanding business of mercenary mothers and their "black-market" babies, in which human life is routinely handled as a commodity to be exchanged, as Titmuss feared, for "dollars and pounds."

[18] Savings bank life insurance, e g , which has offered low-price quality policies since 1907, has never been very successful Interestingly, one of the few commercial failures of the Sears Roebuck catalogue business was an attempt in the 1930s to sell life insurance directly.

[19] Parsons (1951, p. 445) suggests that even medical students need certain rites to justify their association to death, such as the ritualistic dissection of cadavers in the early stages of medical training.

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Income and Independence Effects on Marital Dissolution: Results from the Seattle and Denver Income-Maintenance Experiments[1]

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Using data from the Seattle and Denver Income-Maintenance Experiments, this paper discusses the effects of income and changes in income on marital dissolution. In an earlier article, we presented evidence of an experimental impact on marital dissolution and discussed how the pattern of effects found could be accounted for by nonlinear income and independence effects. The income effect decreases the marital dissolution rate by increasing the family's economic well-being. The independence effect increases the dissolution rate by reducing the economic dependence of the more dependent partner (usually the wife) on the marriage. In this article, we present a model of nonlinear income and independence effects that accounts for much of the experimental-control difference reported in the earlier paper. According to the model, the effect of an income-maintenance program on marital dissolution depends not only upon the magnitude of the payment a couple receives but also on their level of income before the program, the level of the wife's independence, and the magnitude of the change in the wife's independence.

1 INTRODUCTION

Income-maintenance experiments provide a rare opportunity to study the effects of variations in resource flows on the stability of marital arrange-

[1] This article is a revision of a paper presented at the 1977 annual meetings of the American Sociological Association, Chicago. We wish to thank Andrew Cherlin, James Coleman, Milton Friedman, Michael Keeley, Robert Spiegelman, James Sweet, and Peggy Thoits for comments and Helen Garrison, Beverly Lauwagie, Edward Mendelssohn, and Carlisle Shoemaker for computational assistance. The research reported here was performed pursuant to contracts with the states of Washington and Colorado, prime contractors for the Department of Health, Education, and Welfare under contract numbers SRS-70-53 and HEW-100-78-0004, respectively. The opinions expressed are those of the authors and should not be construed as representing the opinions or policies of the states of Washington or Colorado or any agency of the United States government. During the period of this research, Hannan was a fellow at the Center for Advanced Study in the Behavioral Sciences and was supported by National Science Foundation grant BNS76-22943

ments. In these experiments families are assigned randomly to treatments that alter their financial constraints. By observing exits from preexisting marriages, we can determine whether short-term (three- or five-year) changes in the structure of constraints affect the rate at which marriages break up. If, as is often argued, marital stability reflects primarily the social origins of the partners and the early circumstances of the marriage—for example, age at marriage—then we should find only slight effects, if any. On the other hand, if marital stability responds to current resource levels and to the quality of alternative marital arrangements, such interventions ought to alter rates of marital dissolution.

In an earlier paper (Hannan, Tuma, and Groeneveld 1977), we addressed these broad issues. We found that income-maintenance interventions had little, if any, impact on the rate at which single women with children marry (except in the sample of Chicanas). But for each group studied, income-maintenance treatments substantially raised rates of marital dissolution. Thus, we concluded that short-term alterations in financial circumstances do indeed affect marital stability.

While this earlier research leaves little doubt that the experiment had effects, it leaves open the question how they occurred. That is, we conducted what might be termed a pure experimental analysis. We relied heavily on the stratified-random assignment and asked simply, Do the experimental groups differ from the control group more than would be expected by chance? Having satisfied ourselves that they do, we turn to explaining how the effects were produced. Again, we use data from the Seattle and Denver Income-Maintenance Experiments (hereafter referred to as SIME/DIME).

A natural point of departure is the seeming paradox of the support-level effects. All the negative income tax programs increase the rate of marital dissolution, but the effect varies inversely with the level of income

TABLE 1

ESTIMATE OF SUPPORT-LEVEL EFFECTS MULTIPLIERS OF THE DISSOLUTION RATE OF CONTROL COUPLES WITH SIMILAR CHARACTERISTICS[a]

	Blacks	Whites	Chicanos
Low support treatment	2 16**	1 94*	1 20
Medium support treatment	2 06**	1 56	80
High support treatment	1.45	88	73
Three-year treatment	.81	83	1.13
N	939	1,297	535

[a] The other causal variables used are listed in n 3

* .01 < P ≤ 05.

** P ≤ .01.

support (Hannan, Tuma, and Groeneveld 1977). Programs with an income support level close to the poverty line increase significantly the dissolution rates for both black and white couples. So do programs with a support level 25% above the poverty line. But programs with a support level 50% above the poverty line have smaller effects that are not statistically significant. This is the paradox: Slight changes in economic circumstances have strong effects on marriage, while larger changes do not. This pattern of findings holds for a variety of model specifications and for two different estimators. Moreover, it is robust with respect to attrition bias (see Hannan, Tuma, and Groeneveld 1976).

The findings in table 1 are typical. The entries are multipliers of the dissolution rate for controls[2] (i.e., those not on a financial treatment) who have the same values on other causal variables.[3] They tell the value of the dissolution rate for those on the support level relative to the rate for those on the control treatment. The estimates differ slightly from those reported earlier (Hannan, Tuma, and Groeneveld 1977, table 2) because the observation period, the sample, and other causal variables in the equation differ.[4] For whites the low support treatment almost doubles the rate of dissolution; this effect is significant at the .05 level. The next higher support level raises the dissolution rate by 56%; this increase is not statistically significant. The high support actually decreases the rate but is not significant at even the .10 level. A similar pattern holds for blacks. Both low and medium support levels more than double the rate. Both effects are significant at the .05 level. The high support has a considerably smaller and insignificant effect.

In earlier reports we indicated a significant effect of the low support treatment for the Chicano sample. While the multiplier for the low sup-

[2] In the control group, the percentages of marriages ending within two years after the beginning of the experiment is 156 for blacks, 102 for whites, and 145 for Chicanos The coefficients in table 1 are multipliers of the rates for control families with specified characteristics (see below) A treatment with no effect has a multiplier of one Multipliers greater than unity indicate that the program increases the rate the proportional change (increase or decrease) is found by subtracting one from the multiplier

[3] The causal variables included are duration of marriage at the beginning of the experiment, ages of husband and wife, education of husband and wife, number of children, a dummy for previous AFDC experience, a dummy for the site, dummy variables for the experimental manpower treatment, and preexperimental levels of family income (squared) and wife's income if single (inverse) The latter two variables do not include experimental payments We also include variables that tell whether payments to families from SIME/DIME would be minimum owing to high levels of nonwage income or earnings.

[4] The main differences are that here we follow marriage for two years instead of 18 months, analyze only those couples married at the beginning of the experiment rather than all marriages observed during the study period, analyze only families with children, and control for domination of SIME/DIME by existing nonwage income or earnings. Each of these changes reduces the estimated support-level effects for whites.

port treatment is higher than that reported for other support levels for this group in table 1, it does not differ significantly from one. The multiplier for this treatment has apparently been reduced by the addition of controls for initial levels of wife's independence (see below). Whatever the reason for the change in our findings, we now have no experimental effect to explain for Chicanos. Nonetheless, for purposes of comparability we report analyses for all three race-ethnic categories.

Earlier, we proposed an explanation for the strange pattern of support-level effects (Hannan, Beaver, and Tuma 1974; Hannan, Tuma, and Groeneveld 1977). We argued that income maintenance has two opposing effects on rates of marital dissolution. It raises level of family income, and it decreases the dependence of partners on the marriage by providing new financial alternatives to marriage. Both theory and empirical work indicate that the income effect lowers the rate of marital dissolution. The independence effect increases the rate. Depending on the strength and functional forms of the two effects, income maintenance can increase the rate of dissolution, decrease it, or leave it unchanged.

This argument bears directly on the paradox of the support-level effects. We suspected that each of the support levels induces a strong independence effect, but that only the high support program generates an income effect strong enough to offset the independence effect. The purpose of this paper is to test this hypothesis.

Before we began to analyze the data, we proposed that both income and independence effects would have a threshold and floor (or ceiling, as the case may be). In Section II, we propose specific functional forms that are consistent with our understanding of the processes and that at least permit income-maintenance treatments to produce the pattern of effects we observed. Though the ideas that underlie the model are very simple, the mathematical statement of it will undoubtedly appear both unfamiliar and complex to many sociologists. Therefore, we always complement our algebraic treatment with graphical displays of the functions. Some readers may prefer to skip over the algebra and rely on the graphs, which display the qualitative features of the model.

Our earlier arguments also emphasized nonpecuniary differences between income maintenance and welfare. This issue arises because the low support treatment has almost the same financial impact on a family as the welfare system available to control families (AFDC and food stamps). If income maintenance and welfare do not differ in other ways, we cannot explain the large difference in dissolution rates of controls and subjects enrolled on the low support treatment. Earlier, we identified several differences between the two types of income support programs. They suggest that income from the current welfare system is discounted in its behavior

consequences. That is, for a married woman considering her financial situation if she leaves her marriage, a dollar of welfare has a smaller independence effect than a dollar of income maintenance. We discuss the procedure for incorporating the welfare discount in Section II.

We make brief remarks about the structure of the data and our estimation procedures in Section III. In Section IV we report means of pre-experimental levels of family income and independence, as well as means of experimental changes in these variables. Then, in Section V, we report two types of findings. First, we present findings on income and independence effects, both parameter estimates and plots of the estimated curves. Then we show that by incorporating the income and independence effects of the experimental payments we can explain a good deal of the experimental impact. This is the crucial demonstration of the paper. Finally, we show graphically in Section VI how the paradox of the support levels arose in the first place.

II INCOME AND INDEPENDENCE EFFECTS

Theoretical and empirical work on marriage (reviewed in Hannan, Tuma, and Groeneveld 1976) suggests that the marital dissolution rate decreases with family income and increases with the income of the financially dependent partner (usually the wife) outside the marriage. Income maintenance generally has both effects: it increases both family income and the wife's income outside the marriage. In this section we move beyond these qualitative statements and propose a parametric model for income and independence effects. We used several criteria in choosing among alternative specifications. Of course, we entertained only models consistent with the argument—this criterion ruled out nonmonotonic income and independence effects. We also insisted that the model fit our observations on both control families and those with a financial treatment. Finally, we used the results in table 1 as benchmarks. To each candidate model, we applied the test of consistency with the benchmark. Some models could not possibly produce the pattern observed in table 1. Others at least admit the possibility of so doing. We concentrated on the latter.

The Income Effect

Forget the experiment for a moment. The rate of marital dissolution, r, should be a monotonically decreasing function of a family's level of disposable income, Y. One possible specification is the linear function $r = \alpha Y$. This choice has the advantage of simplicity and wide usage. We doubt, however, that a $1,000 increase in income has the same effect for all levels of income, as this function implies. Another widely used specification, the

log-linear relationship $r = \exp(\alpha Y)$, avoids part of the problem. With this specification, the effect of increases in income becomes smaller as income increases (assuming α is negative), that is, there is a floor effect. However, this model stipulates that the largest income effects occur at the smallest income levels. That is, the effect of moving from $0 to $1,000 is larger than that of a $1,000 increase at any other level of family income. We doubt, however, that income changes below a subsistence income level will have strong effects on the rate of dissolution. We suspect that there are threshold as well as floor effects in the income-dissolution relationship. In other words, we argue that increases in family income do not have strong effects on rates of marital dissolution for either very low or very high levels of income. Instead, we expect that the strongest effects occur for some intermediate level.

A simple function that behaves in this fashion is

$$r = A_1 \exp(\alpha Y^2)\ , \tag{1}$$

where A_1 summarizes the effects of all other causal variables, including the wife's independence.

As long as $\alpha < 0$, the function in equation (1) is a monotonically declining function of Y. As Y increases, $\exp(\alpha Y^2)$ approaches zero, so the rate cannot become negative. Inspection of the second derivatives of equation (1) establishes that the function is concave for $Y^2 < -1/2\alpha$ and
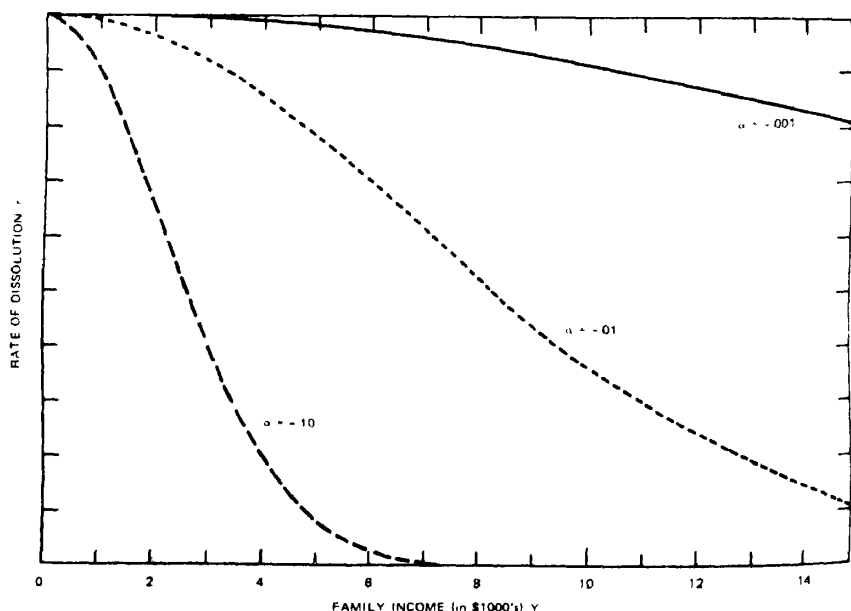
Fig. 1.—Income effects, $r = \exp(\alpha Y^2)$, for various levels of α

convex for $Y^2 > -1/2\alpha$. When $\alpha < 0$ and $|\alpha|$ is large, the point at which the shape of the function becomes convex is close to zero, and the dissolution rate declines exponentially over most of the range of Y. When $|\alpha|$ is small, the function is very flat initially. The shapes of the income effect for three values of α are depicted in figure 1. The choice of this form for the income effect does not greatly constrain our analysis. Depending on our estimates of α, the income effect may be steep or flat over the entire range of incomes we observe; it may also have a pronounced backward S shape. This flexibility plays an important part in what follows. It means that the functional form we have chosen permits, but does not constrain, the empirical results to explain our previous findings. In particular, we can explain the support-level effects only if the income effect is fairly steep over the range of incomes that predominate in the sample assigned to the high support treatment.

Now consider the experiment. Families assigned to the control treatment do not receive any income-maintenance payments. Therefore, on the average, their incomes during the two years should be the same as during the preexperimental period. But families on financial treatments receive an income guarantee, and most have incomes low enough that they will receive some payment. For simplicity, we assume that families on financial treatments do not adjust their earnings under income maintenance.[5] Then, it is straightforward to calculate the expected change in income due to the experiment, ΔY, based on the preexperimental level of income, Y. On the average, a family on an experimental treatment will have an income level of $Y + \Delta Y$ during the experiment.

The simplest model for the effects of the experiment is that exogenous changes in income, ΔY, have effects similar to those of levels of income, Y. That is,

$$r = A_1 \exp[\alpha(Y + \Delta Y)^2]\,. \tag{2}$$

We find that the simple model in equation (2) fits well for blacks and Chicanos but not whites. For whites, we find we must use a model in which the effects of changes and of levels differ and where the effect of a change depends on the level:[6]

$$r = A_1 \exp[\alpha Y^2 + \gamma \Delta Y^2 + \delta Y \cdot \Delta Y]\,. \tag{3}$$

[5] We presume no effects of the experiment on earnings in order to postpone the thorny problem of modeling the joint effects of labor supply and family behavior. For the same reason, we use measures of income over the preexperimental year for families on financial treatments Thus, our estimates of experimental effects include both direct effects and indirect effects mediated by impact on work We are just beginning to analyze more complex models that interrelate experimental changes in work effort to changes in marital status

[6] We find for whites that experimental changes in income interact (in the log-linear specification) in affecting dissolution rates. However, we had no a priori expectations

The Independence Effect

The effect of independence, measured by the wife's disposable income if single, should be monotonically increasing, holding constant family income. Here we expect a floor and a ceiling.[7] That is, we suspect that small increases in independence will not alter rates of dissolution for families with the most dependent wives—there is some threshold level of independence beyond which the effects of any given change in independence will increase. Also, we expect that a given change in independence will not greatly affect families with wives who are already very independent (though our sample may not contain any such families).

A simple functional form that fits these requirements is

$$r = A_2 \cdot \exp(\beta Y_s^{-1}) , \tag{4}$$

where Y_s denotes the wife's disposable income if single in the control environment and A_2 denotes the effects of all other causal variables, including family income. Independence has a positive effect on the dissolution rate as long as β is negative. In that case, the function is convex for $Y_s < -\beta/2$ and approaches the ceiling A_2. The shape of the function over the range of sample observations is determined by the estimate of β obtained from the data. If $\beta < 0$ and $|\beta|$ is large, the function has a threshold. That is, it is initially relatively flat and then becomes steep. If $|\beta|$ is small, the function is steep for low values of Y_s and flattens out more quickly. Three alternatives are sketched in figure 2. Depending on our estimates of β, we may obtain independence effects over the range of our sample observations that are flat, steep for small values of Y_s, and then relatively flat, etc. As we pointed out in discussing the income effect, this

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about the precise nature of such interactions We have experimented with several forms All the forms of interaction we tried lead to the same qualitative conclusions concerning the main issue under study, explaining the support-level effects. However, some specifications give larger standard errors on income effects than others One of the referees suggested that we use a quadratic form with linear, squared, and interaction terms. However, our maximum likelihood program would not converge under this specification due to collinearity The specification used, in eq (3), gives smaller standard errors on income effects than other interactions tried

[7] Again we reject the more usual linear and log-linear specifications on theoretical grounds The former implies that increments in independence have the same effect on the rate of dissolution at each level of independence so that experimental effects hold with equal strength over the entire range of preexperimental independence The log-linear specification with a positive coefficient implies that the rate of dissolution grows exponentially with increases in independence, so the more independent a woman is initially, the greater the effect of any increase in independence The specification in eq (4) avoids both problems. We did, nonetheless, test the fit of linear and log-linear independence effects. They rarely fit as well as (and never fit better than) the model in eq (4).

flexibility is important to our ability to test the model. Unless the independence effect is steep over the observed range of values of Y_s, we cannot explain the support-level effects.

Measurement of wives' independence involves two issues: (1) estimating what wives think they would earn as single women and (2) taking welfare payments into account. Of course, we do not know what level of earnings a wife would achieve as a single woman. We do not even know that this is the proper variable. What we wish to measure is the level of income a wife expects (or is expected by her husband) to achieve as a single woman. We tried two procedures to measure expected earnings. The first assumes that earnings as a married woman are the best proxy for subjective assessments of earnings as a single woman. The second involves predicting earnings from the behavior of single women with similar characteristics. Results from analyses using the two measures are usually quite similar; however, the estimates employing the measure based on actual earnings have smaller standard errors. Therefore, we restrict attention to that measure in presenting results.

The environment of control families contains welfare (AFDC and food stamp programs). Hannan, Tuma, and Groeneveld (1977) identified four nonpecuniary differences between SIME/DIME and welfare relevant to this analysis: (i) Unlike welfare, participation in income maintenance presumably involves no public (or perhaps private) stigma. (ii) Benefits
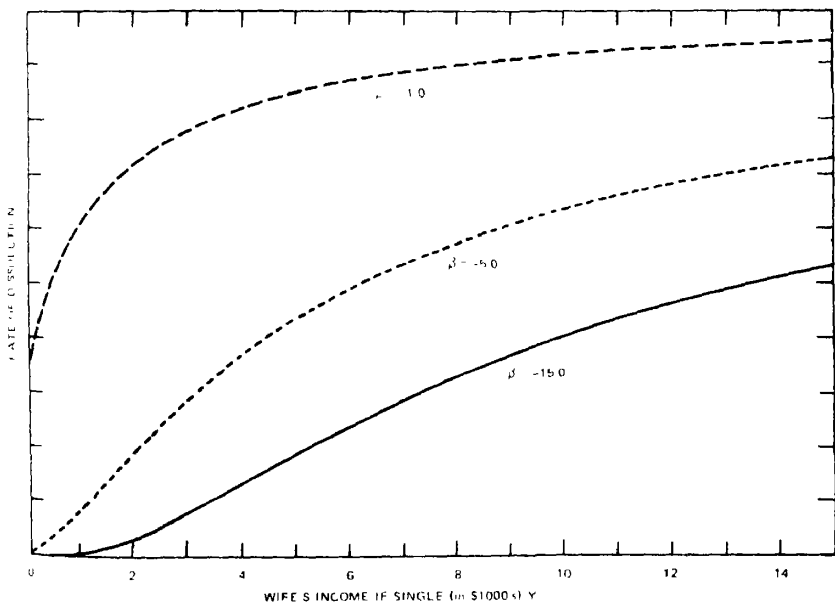
FIG. 2.—Independence effects, $r = \exp(\beta Y_s^{-1})$ for various levels of β

and rights under SIME/DIME are thoroughly explained to participants; AFDC and food stamp benefits are not. (iii) Participation in income maintenance involves much less effort than is the case with welfare. Since only minimal contacts with any bureaucracy are required, transaction costs for income maintenance are probably lower than for welfare. (iv) All families on financial treatments in SIME/DIME receive some minimal payment. Thus, SIME/DIME may be more salient to married women than welfare and may therefore have a greater impact on decisions to end a marriage. For all these reasons, the impact of $1.00 of the welfare benefit available to a married woman upon becoming single should be less than the impact of $1.00 of income maintenance. That is, the potential welfare benefit should be discounted.

High welfare participation rates among those eligible do not necessarily conflict with this view. The population of unmarried women with children overrepresents women with low welfare discounts. We are modeling the behavior of married women. At least some may be married at the beginning of the experiment only because they refuse the alternative of welfare. It is precisely these women who should react most strongly to income maintenance.

We have made a number of attempts to estimate a welfare discount. However, the nonlinear functional form chosen for the independence effects has frustrated our attempts to this point. For the present we must choose a discount a priori. We have used a variety of discounts in our analysis, ranging from 25% to 75%. The qualitative inferences do not vary much with the discount used. However, the 25% discount seems too low in that we do not do as well in explaining the experimental effects. So we choose to present findings based on a 50% discount. That is, we assume that it takes $2.00 of welfare guarantee to produce the level of independence equal to that under $1.00 of earnings or income-maintenance guarantee.

To incorporate the welfare discount, we calculate a wife's disposable income if single as follows. Let E_s denote a woman's expected single earnings (the same as her earnings during the preexperimental year) and N_s denote nonwage income other than welfare. Based on E_s, N_s, and family size, we calculate potential AFDC and food stamp grants. Denote the sum of these grants as W_s. The discounted welfare benefit, W^*_s, is then simply $.5W_s$. Finally, the control level of disposable income if single is defined as

$$Y_s = E_s + N_s + W^*_s \,. \tag{5}$$

To consider the contribution of income maintenance to the independence effect, we calculate an income-maintenance payment if single, P_s, for each woman on a financial treatment, ignoring W_s. Since the experiment taxes

welfare payment at 100%, we define change in Y_s due to the experiment as

$$\Delta Y_s = \max \begin{cases} P_s - W^*_s \\ 0 \end{cases} \quad (6)$$

By definition, ΔY_s is zero for all controls.

The simplest model for a payment effect is that a change in independence, ΔY_s, has the same effect as the initial level, Y_s:

$$r = A_2 \exp [\beta(Y_s + \Delta Y_s)^{-1}] . \quad (7)$$

For all three race-ethnic groups, this model fits as well as any of the more complex models we tested. That is, permitting ΔY_s to have effects different from those of Y_s does not significantly improve the fit of the model.

Family Size Interactions

At the risk of overcomplicating the model, we introduce one additional consideration. To this point, we have made no allowance for the effects of family size. We expected, however, that both income and independence effects would vary by family size, particularly in low-income populations.[8] A unit increase in either family income or wife's income if single may have an effect different from that in larger families in a small family. Social scientists typically adopt some standardization for such effects. The available family-size indices are designed to reflect the effects of income on consumption. Thus, they may not tell us anything about the effects of income and independence on rates of dissolution. So, instead of adjusting Y and Y_s for family size, we estimate models in which income and independence effects depend on family size.

Families without children face different conditions in both the control and experimental environments. As controls, they are not eligible for AFDC. As experimentals, their payments if single do not vary by support level; that is, the guarantee to a one-person family is constant across experimental programs ($100 in 1971 dollars). Dealing with these differences greatly complicates empirical analysis. Thus, we restrict attention here to families with children.

Our strategy was to begin with complex models that include a variety of family-size interactions and to compare the fit with simpler models. As the various sets of family-size interactions were at least partially hierarchically ordered, we used tests of significance to choose the functional form of the family-size adjustment for each race-ethnic group. That is, we began

[8] We presume throughout that the children in a marriage will stay with the wife following a marital dissolution.

with the most complex model and tested whether dropping certain interactions significantly reduced the fit of the model (evaluated in terms of a likelihood ratio test). If the fit was reduced at or beyond the .10 level, we retained the more complex model. Only for whites do we find that the family size interactions significantly improve the fit of our models.

Thus, the model we estimated for blacks and Chicanos is

$$r = \exp\left[\alpha(Y + \Delta Y)^2 + \beta(Y_s + \Delta Y_s)^{-1} + X\theta +\right] . \tag{8}$$

The vector X contains the variables listed in footnote 3.

The model estimated for whites can be written

$$r = \exp\left[\alpha^* Y^2 + \gamma^* \Delta Y^2 + \delta^* Y \cdot \Delta Y + \beta^*(Y_s + \Delta Y_s)^{-1} + X\theta\right] , \tag{9}$$

where $\alpha^* = \alpha_0 + \alpha_1 C$, $\beta^* = \beta_0 + \beta_1 C$, $\gamma^* = \gamma_0 + \gamma_1 C$, and $\delta^* = \delta_0 + \delta_1 C$, where C denotes the number of children in the family.

III. METHODOLOGY

We use information on changes in family composition of all black, white, and Chicano families enrolled in SIME/DIME. The sample selection and experimental design have been discussed elsewhere (see Hannan, Tuma and Groeneveld 1976, 1977). In this paper we analyze only couples married at the beginning of the experiment. We analyze dissolutions occurring during the first two years of the experiment. To estimate the parameters of the model, we use two pieces of information: whether the marriage ended and, if it did, the ending date in experimental time. Since the estimation procedure is the same as that used in our earlier reports, we note only the essential details.

Let $F(t|t', X)$ denote the probability distribution function that a marriage existing at time t' with characteristics X breaks up by t. (Here X includes measures of income and independence as well as other causal variables, and t' is the date of enrollment in the experiment.) Then the instantaneous rate, $r(t|t', X)$, is defined as

$$r(t|t', X) = \frac{[dF(t|t', X)/dt]}{[1 - F(t|t', X)]} . \tag{10}$$

The likelihood function for the joint distribution of observed ending dates is given by

$$L = \prod_{i=1}^{N} \left[\frac{dF(t_i|t'_i, X_i)}{dt}\right]^{y_i} [1 - F(t_i|t'_i, X_i)]^{(1-y_i)} , \tag{11}$$

where N is the number of couples and $y_i = 1$ if the marriage of couple i

breaks up before the end of the observation period and equals zero otherwise. It is straightforward, using equations (8) or (9), (10), and (11), to write the likelihood function in terms of the observable variables and parameters. We estimate the parameters and their standard errors by an iterative maximum likelihood procedure (a FORTRAN program called RATE [see Tuma and Crockford 1976 for documentation]). A detailed treatment of the technical literature on this estimator, along with results on small sample properties, is found in Tuma and Hannan (1979). Advantages of this estimation strategy and our ability to fit the observed data using it are discussed in Tuma, Hannan, and Groeneveld (1979).

IV LEVELS OF INCOME AND INDEPENDENCE

Given the nonlinear specifications of income and independence effects, it is particularly important to understand the typical levels of these variables and of changes in them in our samples. The average levels of the components of these variables are tabulated by support level in table 2. Look first at family income. We see that the average for control families is roughly $6,600. For those on income-maintenance plans, the average preexperimental level of family income varies from $5,286 for low support to $7,164 for high support. This difference reflects the stratified assignment by family normal income. The typical payments to families (assuming no change in labor supply) also vary by support level but less than one might imagine, due to the stratified assignment. Those on the low support plan receive an average payment of roughly $1,000, while those on the high support receive roughly double that amount.

Next consider the levels of wife's income if single prior to the experiment For the controls the average was $1,274 Families assigned to the low support plan had the most dependent wives on the average: $697. Those on the high support earned slightly more than the controls. The next row of table 2 reports discounted welfare guarantees (i.e., half the

TABLE 2

MEANS OF COMPONENTS OF INCOME AND INDEPENDENCE VARIABLES BY SUPPORT LEVEL

	Control	$3,800 Support	$4,800 Support	$5,600 Support
Preexperimental family income (Y)	6,594	5,286	6,226	7,164
Payments to families (ΔY)	0	1,092	1,695	2,146
Preexperimental wife's income ($E_s + N_s$)	1,274	697	1,052	1,507
Discounted welfare benefit if single (W_s^*)	1,233	1,452	1,264	1,208
Payments to wives on becoming single (P_s)	0	3,145	3,721	4,383
N families	1,117	467	557	352

actual guarantee) to women leaving marriages based on earnings and family size. Finally, we report the average levels of payments to women upon leaving marriage. They are $3,145 for those on the low support treatment and $4,383 on the average for those on high support. So, on the average (under our assumptions), the high support treatment increased family income by about $1,000 more than did the low support treatment. On the average, the high support treatment also paid a woman leaving her marriage about $1,000 more than did the low support treatment.

V. RESULTS

We have two concerns. The first is whether the empirical results yield substantively meaningful and statistically significant income and independence effects. The second is whether our model for the effects of income maintenance accounts for the support-level effects reported in table 1.

The Independence Effect

We look first at estimated parameters to assess the statistical significance of the independence effect and then at graphical displays of the effect to judge substantive importance. The upper half of table 3 reports estimates of the various parameters of the independence effect. The estimated effect

TABLE 3

EFFECTS OF INDEPENDENCE AND INCOME ON THE LOG OF THE DISSOLUTION RATE[a]

Independence Effect ($1,000s)	
Blacks $(Y_s + \Delta Y_s)^{-1}$	− 3 046***
Whites $(Y_s + \Delta Y_s)^{-1}$	1.923 − 1.266**C
Chicanos $(Y_s + \Delta Y_s)^{-1}$	− 3222
Income Effect ($1,000s)	
Blacks:	
Initial level and changes in income $(Y + \Delta Y)^2$	−.0055***
Whites:	
Initial level of income, Y^2	.0095 − 0053**C
Experimental changes in income $(\Delta Y)^2$.	1000* − 0480*C
Interaction, $Y \cdot \Delta Y$	−.0592 + .0247C
Chicanos:	
Initial level and changes in income $(Y + \Delta Y)^2$	−.0012

[a] See text for explanation of symbols.
* .05 < $P \leq$ .10.
** 01 < $P \leq$ .05.
*** $P \leq$ .01.

is significant for blacks and whites but not Chicanos. For blacks, the effect is positive as expected.[9] For whites, it varies with family size, having the wrong sign for families with only one child. In an effort to determine whether this sign reversal might be due to sampling error, we estimated a model that constrains the effect to be positive for all family sizes. The likelihood ratio test of this constrained model against the unconstrained model whose estimates appear in table 3 is not significant at even the .10 level. That is, imposing the constraint does not significantly worsen the model's fit to the data. Thus, we conclude that wife's independence has the predicted effect for both black and white samples.

The estimates in table 3 are difficult to understand directly. It is much easier to grasp their significance from graphical displays of the estimated relationship between Y_s and the rate of dissolution. Given the basic experimental findings and the results in table 3, we focus attention on white and black families. In the case of whites, we plot estimated curves for families with two and three children, the most common family sizes in our sample.

Recall that our model is multiplicative. Thus, the independence effect for any couple must be multiplied by the base rate, that predicted by all other variables, to give the actual estimated rate of dissolution for that couple. The base rate varies from family to family, depending on the values of control variables and income effects. Let us denote the rate of dissolution given by all other variables in the model as r^*. Thus, r^* is the rate of dissolution that holds for a family if the independence effect multiplier is unity, that is, if Y_s is very large. At all lower levels of Y_s, the rate is smaller; it is some fraction of r^*. The fraction depends on Y_s and on estimates of the independence effect. We plot independence effects in these terms. The vertical axis in figure 3 is the rate of dissolution, which varies from zero to r^*. So, the metric of this dimension varies from family to family depending on r^*. The horizontal axis is the wife's income if single.

The simplest comparisons in figure 3 are for a couple shifting from one level of Y_s to another. If a specified change in Y_s shifts a couple from $.4r^*$ to $.6r^*$, their rate of dissolution is increased by 50%. In inspecting the curves in figure 3, it is important to keep in mind the main range of variation in Y_s. Recall that table 2 records that the mean of Y_s prior to the experiment was in the range of $2,000–$3,000. Almost no women in our sample had Y_s exceeding $6,000. Therefore, we are most interested in the behavior of the estimated independence curves in the $1,000–$6,000 range of Y_s. The curves imply considerable increases in rates of marital dissolution for each $1,000 increase in independence over this range. Moreover, the levels of Y_s at which the effects are strongest are close to the minimum levels of

[9] Recall that a negative coefficient for Y_s^{-1} implies that independence is positively related to the dissolution rate.

independence in our samples. In other words, the independence effect is strongest in marriages in which the wife was initially most dependent.

The Income Effect

Estimates of the income effect are also reported in table 3. As with the independence effect, we find significant effects for blacks and whites but not Chicanos. We noted in the previous section that for blacks a simple model in which experimental changes in income and initial levels of income have similar effects fits as well as more complex models. Thus, the estimates of table 3 imply that both levels and experimental changes in income significantly decrease the dissolution rate. In other words, income maintenance has an income effect for the black sample.

For whites, too, the initial level of family income significantly decreases the dissolution rate. Once again the effect varies by family size and has the wrong sign for families with one child. As was the case with the independence effect, a model that constrains the effect to have the predicted sign for all family sizes fits virtually as well as the model whose estimates appear in table 3.

We turn to plots of the estimated effect in order to explore the substantive meaning of the estimates. Figure 4 contains the plots of income effects for black control and experimental families and white control families with
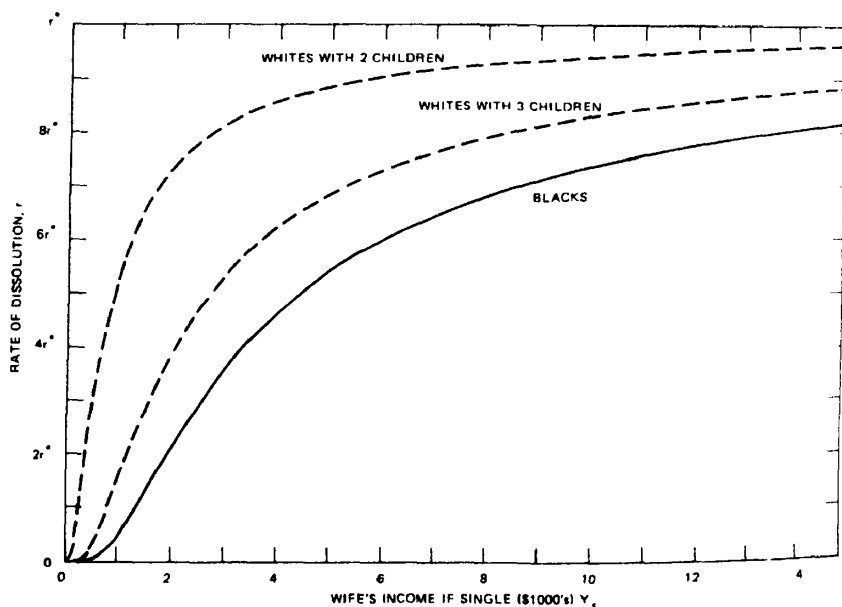
FIG. 3.—Estimated independence effects (see text for definition of r^*)
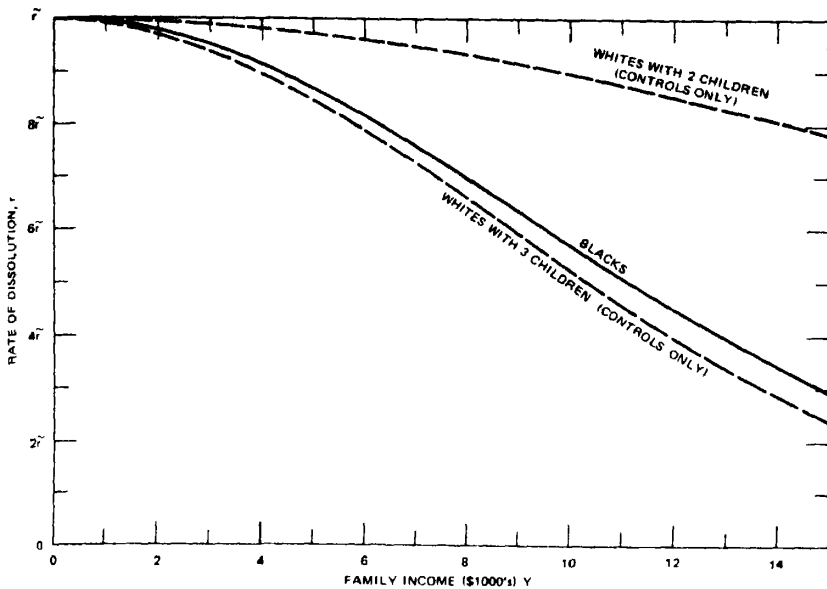
FIG. 4.—Estimated income effects (see text for definition of $\bar{r}$)

two and three children.[10] The structure of figure 4 is similar to that of figure 3. The vertical axis is the rate of dissolution in multiples of $\bar{r}$, the rate predicted from the levels of all other variables in the model, including the independence effect.

For all three groups, the curves have a pronounced backward *S* shape, as hypothesized. The effect of increases in family income on the dissolution rate is slight at low income levels. But the curves decline sharply in the neighborhood of \$4,000 \$6,000 and are steep from this point to beyond \$15,000. The effect is strongest (i.e., steepest) at \$9,500 for blacks, at \$8,600 for white families with three children, but at roughly \$22,000 for white families with two children. So, for whites, income variations in the lower range have stronger impacts on larger families.

As in the case of the plotted independence effects, we find that the effects are strong in the regions that characterize the SIME/DIME samples. All three curves in figure 4 imply that marital dissolution rates for families at the poverty line (about \$4,000 in 1971) differ markedly from those for families with incomes near the national median (about \$10,000). Moreover, the addition of \$1,000 in the \$6,000–\$12,000 range has a large effect.

[10] The model for white experimentals is more complex and cannot easily be represented in a two-dimensional graph. For this group, it is necessary to have a third dimension to allow ΔY to have different effects from Y.

These curves imply that income maintenance has a strong income effect on the marital dissolution rate of the black sample.

We turn next to the estimated effect of changes in income for whites. Recall that this involves both "main" and "interaction" effects. The main effect is significant but the interaction is not. However, we cannot explain the paradox of the support levels for whites without this interaction. So, though the interaction is not statistically significant, it appears to be important substantively.

Both main and interaction effects of experimental changes in income vary with family size. These effects do not have the expected sign for all combinations of family size, level of income, and changes in income. So once again we wished to test the constraint that the effect has on the predicted sign. The natural constraint is that the entire income effect is negative for each family in the sample. However, we could not employ this complex constraint in our estimation procedure. Instead, we used the more stringent constraint that each of the three pieces is negative. Introducing these constraints does not significantly worsen the fit of the model reported in table 3. So the data are consistent with the argument that both levels and increases in family income stabilize marriages.

Support Level Effects

The results in table 3 generally support our argument that income maintenance has both income and independence effects. But our argument also holds that the joint operation of the two types of effects accounts for the varying impact of support levels reported in table 1. We now evaluate this portion of the argument.

The test is simple. The support-level multipliers in table 1 come from models that are identical with those used to calculate the income and independence effects of table 3 in every respect but one. The models in table 1 ignore the experimental changes in income and independence, ΔY and ΔY_w. We repeat the procedure used to calculate the effects in table 1, but this time we introduce ΔY and ΔY_w into the income and independence effects as in equations (8) and (9). The results are reported in table 4.

Including income-maintenance payments in income and independence effects does reduce the support-level multipliers substantially—compare table 4 and table 1. Moreover, none of the support-level effects is now significant at even the .10 level. The model works best for the black sample, where the low and medium support multipliers are shifted close to unity. We are somewhat less successful with whites: although the low support dissolution rate is smaller than in table 1 and is not significant, it is

still 50% above the rate for controls. Thus, we can still improve on the specification used here, presumably by estimating the welfare discount and by taking labor supply during the experiment into account. Nonetheless, we have succeeded in explaining a good deal of the previously reported experimental effect on rates of dissolution. The effects in table 1 do reflect, at least partly, the joint operation of income and independence effects.

VI DISCUSSION

The key to understanding how income and independence effects diminish the support-level effects for blacks and whites lies in considering the joint operation of the two effects. To this point, we have presented curves in only two of the three relevant dimensions. We obtain more insight into the nature of the process by considering slices from the three-dimensional surface relating Y and Y_s to the dissolution rate.

We choose to display independence curves for several selected levels of family income. The vertical axis in figures 5 and 6 is the rate of dissolution, predicted on the basis of the control variables, denoted as $\check{r}$. So the interpretation of each independence curve is exactly as in figure 3. However, we now see how the independence effect varies with family income (whereas in figure 3, we incorporated the effects of family income into r^*). Our model implies that the curves will differ for each family size and for blacks; it also implies whether any children are young. Since our purpose in this section is illustrative, we choose cases with strong income and independence effects so that the nature of the joint impact is clear. The same process works for other family sizes, though the magnitude of the effects differs.

Figure 5 displays the relationship of independence and income to the

TABLE 4

SUPPORT-LEVEL MULTIPLIERS ESTIMATED FROM A MODEL INCLUDING EXPERIMENTAL INDEPENDENCE AND INCOME EFFECTS*

	Blacks	Whites	Chicanos
Low support treatment	1.26	1.50	.78
Medium support treatment	1.08	1.13	.49
High support treatment	.77	.56	.46
Three-year treatment	.69	.80	1.10
N	939	1,297	535

NOTE.—None of the support level multipliers differs significantly from one at the .10 level.

* The variables included in the model are the same as those in table 1, with the exception that ΔY and ΔY_s have been added to the measures of income and independence as described in table 2.
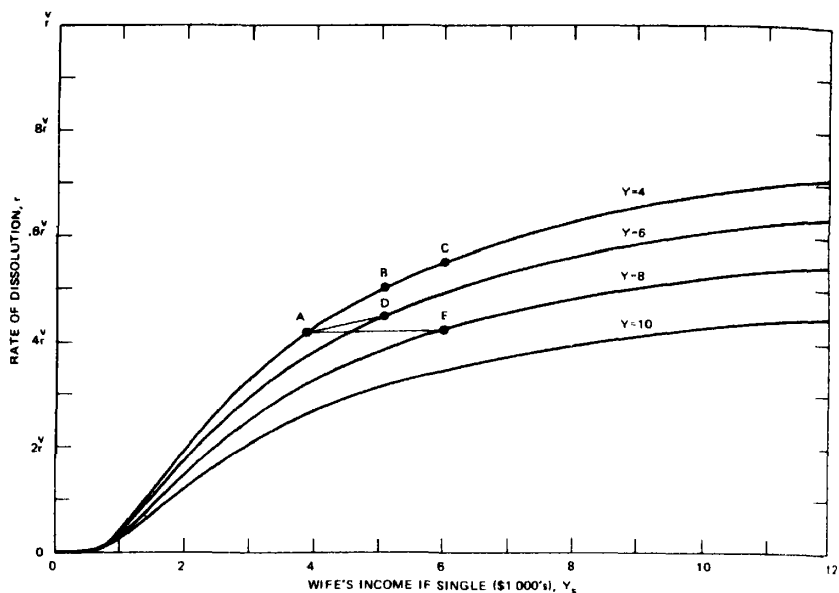
FIG. 5.—Estimated income and independence effects: blacks. Y denotes family income (see text for definition of $\check{r}$).
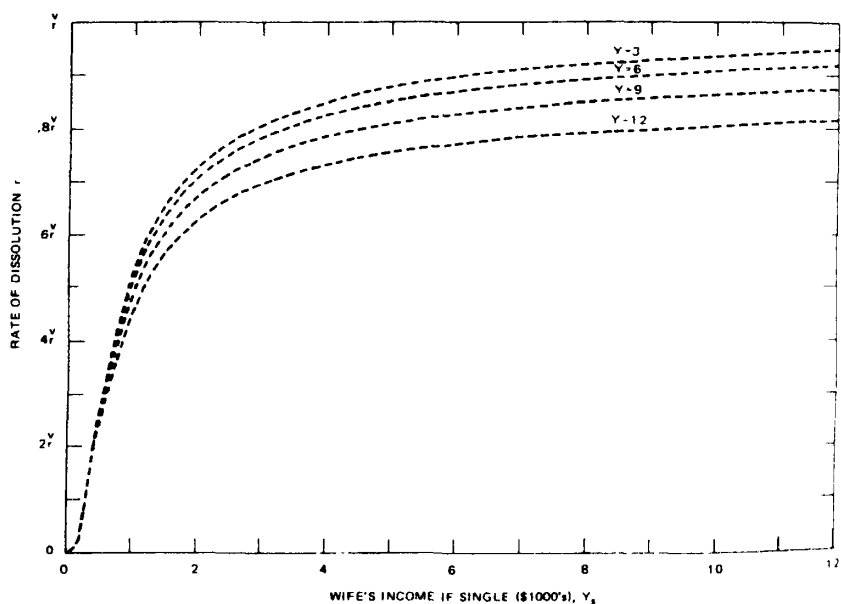
FIG. 6.—Estimated income and independence effects: white controls with two children. Y denotes family income (see text for definition of $\check{r}$).

dissolution rate for black families. Figure 6 displays the same relationship for white control families with two children. We delay consideration of white experimentals because it requires yet another dimension, ΔY.

In both figures 5 and 6, the independence effect is steepest at the lowest levels of family income and becomes less steep with each positive increment to family income. This shows how increases in family income offset independence effects. At a sufficiently high level of family income, an increase in independence has little effect on the dissolution rate.

These curves also show how an income-maintenance program affects rates of dissolution. In general, income maintenance increases both Y and Y_s. It has the consequence of shifting couples to the right on an independence curve and down to a higher income curve. The total effect of income maintenance on the rate of dissolution combines the two changes.

An examination of hypothetical cases shows how income and independence effects could produce the pattern of support-level effects in table 1. Consider two identical families assigned to income-maintenance plans with different support levels. Suppose both families begin at point A in figure 5. Our hypothetical income-maintenance program shifts the woman on the low support along the independence effect curve to B and the woman on the high support to C. So far the richer program produces a larger increase in the dissolution rate. But we must also consider the income effect. Suppose the family on the lower support program receives a \$2,000 increase in income. Then this family is shifted to a new income level, that is, from one independence curve to another, from B to D. The family on the higher support gets \$4,000, say, and is shifted from C to E. The total effects of the two negative income tax programs are the vertical distances between A and D and between A and E. As we have chosen the points, and as apparently occurred in SIME/DIME, E is below D. The lower support program increases the dissolution rate, but the higher support does not.

Finally, we consider the response of white couples on financial treatments. We can construct a figure suitable for displaying the experimental income and independence effects for whites by fixing the level of preexperimental family income. Figure 7 shows curves relating income maintenance payments to the dissolution rate for white families with initial income of \$6,000. The curves are similar to those in figure 6, except that the effect of each \$1,000 of income-maintenance payment is much larger than the effect of a similar change in levels of nonexperimental income. Thus, relatively small differences in levels of payments to families may make a substantial difference in marital dissolution rates.

This series of illustrations shows why the nonlinearities in our model play an essential role in explaining the experimental response. It also shows that the stratified random assignment of families to treatments in SIME/

DIME (see Hannan, Tuma, and Groeneveld 1977, p. 1194) contributes to the differences in impact among support levels (table 1). Higher income families were more often assigned to the high support treatment. The same ΔY and ΔY_s have a smaller joint impact on the dissolution rate for richer families than for poorer families. The reader can easily see this by considering a family that begins at point E in figure 5 and receives a treatment with the same ΔY and ΔY_s that we gave to the family that began at A and ended at D. This family ends up below D. Purely as a consequence of initial levels of income and independence, families assigned to the high support treatment respond differently to any income-maintenance program.

Though the scheme for assigning families to experimental plans apparently contributes to the paradox of the support-level effects, it cannot account single-handedly for the paradox. The support level effects reported in table 1 are adjusted for preexperimental levels of income and independence. Moreover, we estimate multipliers of initial rates of dissolution rather than additive effects. Thus, the estimates in table 1 give the relative increase in the rate of dissolution for each support level adjusting for the assignment, including preexperimental levels of income and independence

So we are left with a simple conclusion: Our earlier supposition that income-transfer programs have two competing effects can, indeed, explain
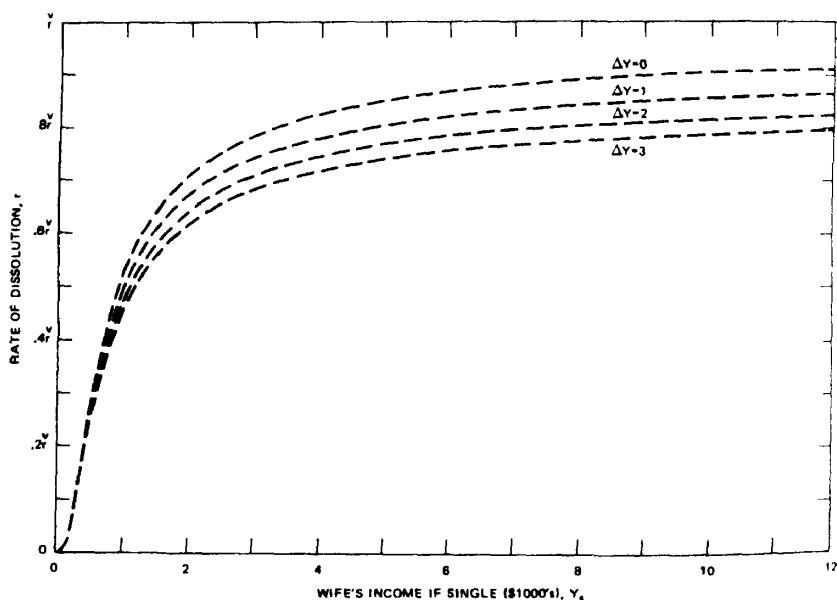
FIG. 7.—Estimated income and independence effects: white families (with two children and preexperimental income of $6,000) on income-maintenance treatments ΔY denotes the change in family income due to income maintenance (see text for definition of $\check{r}$).

much of the support-level paradox. Transfers of income to family members do seem to have both income and independence effects. Both effects appear to be nonlinear, so that a family's response to any transfer program depends on its initial levels of income and independence. Depending on which effect is stronger and depending on a family's initial situation, income-transfer programs can increase or decrease rates of marital dissolution. In the Seattle and Denver experiments, the high support programs have an income effect that partially offsets the destabilizing influences of increased independence. But for the low and medium supports, the independence effect tends to dominate the income effect so that overall the rate of dissolution rises.

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Remarriage as an Incomplete Institution[1]

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The higher divorce rate for remarriages after divorce than for first marriages, it is argued, is due to the incomplete institutionalization of remarriage after divorce in the United States. Persons who are remarried after a divorce and have children from previous marriages face problems unlike those encountered in first marriages. The institution of the family provides no standard solutions to many of these problems, with the result that the unity of families of remarriages after divorce often becomes precarious. The incomplete institutionalization of remarriage shows us, by way of contrast, that family unity in first marriages is still supported by effective institutional controls despite claims that the institutional nature of family life has eroded in the 20th century. Some suggestions for future research on remarriage and on the institutionalization of married life are presented.

Sociologists believe that social institutions shape people's behavior in important ways. Gerth and Mills (1953, p. 173) wrote that institutions are organizations of social roles which "imprint their stamps upon the individual, modifying his external conduct as well as his inner life." More recently, Berger and Luckmann (1966) argued that institutions define not only acceptable behavior, as Gerth and Mills believed, but also objective reality itself. Social institutions range from political and economic systems to religion and language. And displayed prominently in any sociologist's catalogue of institutions is a fundamental form of social organization, the family.

The institution of the family provides social control of reproduction and child rearing. It also provides family members with guidelines for proper behavior in everyday family life, and, presumably, these guidelines contribute to the unity and stability of families. But in recent years, sociologists have de-emphasized the institutional basis of family unity in the United States. According to many scholars, contemporary families are held together more by consensus and mutual affection than by formal, institutional controls.

The main source of this viewpoint is an influential text by Ernest Burgess and Harvey Locke which appeared in 1945. They wrote:

[1] I wish to thank Doris Entwisle, George Levinger, Valerie Oppenheimer, and Richard Rubinson for comments on earlier drafts.

> The central thesis of this volume is that the family in historical times has been, and at present is, in transition from an institution to a companionship. In the past. the important factors unifying the family have been external, formal, and authoritarian, as the law, the mores, public opinion, tradition, the authority of the family head, rigid discipline, and elaborate ritual. At present, in the new emerging form of the companionship family, its unity inheres less and less in community pressures and more and more in such interpersonal relationships as the mutual affection, the sympathetic understanding, and the comradeship of its members. [P. vii]

In the institutional family, Burgess and Locke stated, unity derived from the unchallenged authority of the patriarch, who was supported by strong social pressure. But, they argued, with urbanization and the decline of patriarchal authority, a democratic family has emerged which creates its own unity from interpersonal relations.

Many subsequent studies have retained the idea of the companionship family in some form, such as the equalitarian family of Blood and Wolfe (1960) or the symmetrical family of Young and Wilmott (1973). Common to all is the notion that patriarchal authority has declined and sex roles have become less segregated. Historical studies of family life demonstrate that the authority of the husband was indeed stronger in the preindustrial West than it is now (see, e.g., Ariès 1962; Shorter 1975). As for today, numerous studies of "family power" have attempted to show that authority and power are shared more equally between spouses (see Blood and Wolfe 1960). Although these studies have been criticized (Safilios-Rothschild 1970), no one has claimed that patriarchal authority is as strong now as the historical record indicates it once was. Even if we believe that husbands still have more authority than wives, we can nevertheless agree that patriarchal authority seems to have declined in the United States in this century.

But it does not follow that institutional sources of family unity have declined also. Burgess and Locke reached this conclusion in part because of their assumption that the patriarch was the transmitter of social norms and values to his family. With the decline of the patriarch, so they believed, a vital institutional link between family and society was broken. This argument is similar to the perspective of Gerth and Mills, who wrote that a set of social roles becomes an institution when it is stabilized by a "head" who wields authority over the members. It follows from this premise that if the head loses his authority, the institutional nature of family life will become problematic.

Yet institutionalized patterns of behavior clearly persist in family life, despite the trend away from patriarchy and segregated sex roles. As others have noted (Dyer and Urban 1958; Nye and Berardo 1973), the equalitarian pattern may be as firmly institutionalized now as the traditional

pattern was in the past. In the terms of Berger and Luckmann, most family behavior today is habitualized action which is accepted as typical by all members—that is, it is institutionalized behavior. In most everyday situations, parents and children base their behavior on social norms: parents know how harshly to discipline their children, and children learn from parents and friends which parental rules are fair and which to protest. These sources of institutionalization in the contemporary American family have received little attention from students of family unity, just as family members themselves pay little attention to them.

The presence of these habitualized patterns directly affects family unity. "Habitualization," Berger and Luckmann wrote, "carries with it the important psychological gain that choices are narrowed" (1966, p. 53). With choices narrowed, family members face fewer decisions which will cause disagreements and, correspondingly, have less difficulty maintaining family unity. *Thus, institutional support for family unity exists through the* routinization of everyday behavior even though the husband is no longer the unchallenged agent of social control.

Nowhere in contemporary family life is the psychological gain from habitualization more evident than in the families of remarried spouses and their children, where, paradoxically, habitualized behavior is often absent. We know that the unity of families of remarriages which follow a divorce is often precarious—as evidenced by the higher divorce rate for these families than for families of first marriages (U.S. Bureau of the Census 1976). And in the last few decades, remarriage after divorce—as opposed to remarriage after widowhood—has become the predominant form of remarriage. In this paper, I will argue that the higher divorce rate for remarriages after divorce is a consequence of the incomplete institutionalization of remarriage after divorce in our society. The institution of the family in the United States has developed in response to the needs of families of first marriages and families of remarriages after widowhood. But because of their complex structure, families of remarriages after divorce that include children from previous marriages must solve problems unknown to other types of families. For many of these problems, such as proper kinship terms, authority to discipline stepchildren, and legal relationships, no institutionalized solutions have emerged. As a result, there is more opportunity for disagreements and divisions among family members and more strain in many remarriages after divorce.

The incomplete institutionalization of remarriage after divorce reveals, by way of contrast, the high degree of institutionalization still present in first marriages. Family members, especially those in first marriages, rely on a wide range of habitualized behaviors to assist them in solving the common problems of family life. We take these behavioral patterns for

spouses had remarried after a divorce (U.S. Bureau of the Census 1973). Most of this increase is due to the rise in the divorce rate, but some part is due to the greater tendency of divorced and widowed adults to remarry. The remarriage rate for divorced and widowed women was about 50% higher in the mid-1970s than in 1940 (Norton and Glick 1976).

At the same time, the percentage of divorces which involved at least one child increased from 46% in 1950 to 60% in 1974 (U.S. National Center for Health Statistics 1953, 1977). The increase in the percentage of divorces which involve children means that more families of remarriages after divorce now have stepchildren. Although it is not possible with available data to calculate the exact number of families with stepchildren, we do know that in 1970 8.9 million children lived in two-parent families where one or both parents had been previously divorced (U.S. Bureau of the Census 1973). Some of these children—who constituted 15% of all children living in two-parent families—were from previous marriages, and others were from the remarriages.

Can these families of remarriages after divorce, many of which include children from previous marriages, maintain unity as well as do families of first marriages? Not according to the divorce rate. A number of studies have shown a greater risk of separation and divorce for remarriages after divorce (Becker, Landes, and Michael 1976; Bumpass and Sweet 1972; Cherlin 1977; Monahan 1958). Remarriages after widowhood appear, in contrast, to have a lower divorce rate than first marriages (Monahan 1958). A recent Bureau of the Census report (U.S. Bureau of the Census 1976) estimated that about 33% of all first marriages among people 25–35 may end in divorce, while about 40% of remarriages after divorce among people this age may end in divorce. The estimates are based on current rates of divorce, which could, of course, change greatly in the future.[2]

Conventional wisdom, however, seems to be that remarriages are more successful than first marriages. In a small, nonrandom sample of family counselors and remarried couples, I found most to be surprised at the news that divorce was more prevalent in remarriages. There are some plausible reasons for this popular misconception. Those who remarry are older, on the average, than those marrying for the first time and are presumably more mature. They have had more time to search the marriage market and to determine their own needs and preferences. In addition, divorced

[2] A study by McCarthy (1977), however, suggests that remarriages may be more stable than first marriages for blacks. Using life-table techniques on data from 10,000 women under age 45 collected in the 1973 Survey of Family Growth, McCarthy reported that the probability of separation and divorce during the first 15 years of marriage is lower for blacks in remarriages than in first marriages, but is about 50% higher for whites in remarriages than for whites in first marriages.

men may be in a better financial position and command greater work skills than younger, never-married men. (Divorced women who are supporting children, however, are often in a worse financial position—see Hoffman [1977].)

But despite these advantages, the divorce rate is higher in remarriages after divorce. The reported differences are often modest, but they appear consistently throughout 20 years of research. And the meaning of marital dissolution for family unity is clear: when a marriage dissolves, unity ends. The converse, though, is not necessarily true: a family may have a low degree of unity but remain nominally intact. Even with this limitation, I submit that the divorce rate is the best objective indicator of differences in family unity between remarriages and first marriages.

There are indicators of family unity other than divorce, but their meaning is less clear and their measurement is more difficult. There is the survey research tradition, for example, of asking people how happy or satisfied they are with their marriages. The invariable result is that almost everyone reports that they are very happy. (See, e.g., Bradburn and Caplovitz 1965; Glenn 1975; Campbell, Converse, and Rodgers 1976.) It may be that our high rate of divorce increases the general level of marital satisfaction by dissolving unsatisfactory marriages. But it is also possible that the satisfaction ratings are inflated by the reluctance of some respondents to admit that their marriages are less than fully satisfying. Marriage is an important part of life for most adults—the respondents in the Campbell et al. (1976) national sample rated it second only to health as the most important aspect of their lives—and people may be reluctant to admit publicly that their marriage is troubled.

Several recent studies, nevertheless, have shown that levels of satisfaction and happiness are lower among the remarried, although the differences typically are small. Besides the Campbell et al. study, these include Glenn and Weaver (1977), who found modest differences in marital happiness in the 1973, 1974, and 1975 General Social Surveys conducted by the National Opinion Research Center. They reported that for women, the difference between those who were remarried and those who were in a first marriage was statistically significant, while for men the difference was smaller and not significant. In addition, Renne (1971) reported that remarried, previously divorced persons were less happy with their marriages than those in first marriages in a probability sample of 4,452 Alameda County, California, households. Again, the differences were modest, but they were consistent within categories of age, sex, and race. No tests of significance were reported.

The higher divorce rate suggests that maintaining family unity is more difficult for families of remarriages after divorce. And the lower levels of

marital satisfaction, which must be interpreted cautiously, also support this hypothesis. It is true, nevertheless, that many remarriages work well, and that the majority of remarriages will not end in divorce. And we must remember that the divorce rate is also at an all-time high for first marriages. But there is a difference of degree between remarriages and first marriages which appears consistently in research. We must ask why families of remarriages after divorce seem to have more difficulty maintaining family unity than do families of first marriages. Several explanations have been proposed, and we will now assess the available evidence for each

PREVIOUS EXPLANATIONS

One explanation, favored until recently by many psychiatrists, is that the problems of remarried people arise from personality disorders which preceded their marriages (see Bergler 1948). People in troubled marriages, according to this view, have unresolved personal conflicts which must be treated before a successful marriage can be achieved. Their problems lead them to marry second spouses who may be superficially quite different from their first spouse but are characterologically quite similar. As a result, this theory states, remarried people repeat the problems of their first marriages.

If this explanation were correct, one would expect that people in remarriages would show higher levels of psychiatric symptomatology than people in first marriages. But there is little evidence of this. On the contrary, Overall (1971) reported that in a sample of 2,000 clients seeking help for psychiatric problems, currently remarried people showed lower levels of psychopathology on a general rating scale than persons in first marriages and currently divorced persons. These findings, of course, apply only to people who sought psychiatric help. And it may be, as Overall noted, that the differences emerged because remarried people are more likely to seek help for less serious problems. The findings, nevertheless, weaken the psychoanalytic interpretation of the problems of remarried life

On the other hand, Monahan (1958) and Cherlin (1977) reported that the divorce rate was considerably higher for people in their third marriages who had divorced twice than for people in their second marriages. Perhaps personality disorders among some of those who marry several times prevent them from achieving a successful marriage. But even with the currently high rates of divorce and remarriage, only a small proportion of all adults marry more than twice. About 10% of all adults in 1975 had married twice, but less than 2% had married three or more times (U.S. Bureau of the Census 1976).

Most remarried people, then, are in a second marriage. And the large

number of people now divorcing and entering a second marriage also undercuts the psychoanalytic interpretation. If current rates hold, about one-third of all young married people will become divorced, and about four-fifths of these will remarry. It is hard to believe that the recent increases in divorce and remarriage are due to the sudden spread of marriage-threatening personality disorders to a large part of the young adult population. I conclude, instead, that the psychoanalytic explanation for the rise in divorce and the difficulties of remarried spouses and their children is at best incomplete.[3]

A second possible explanation is that once a person has divorced he or she is less hesitant to do so again. Having divorced once, a person knows how to get divorced and what to expect from family members, friends, and the courts. This explanation is plausible and probably accounts for some of the difference in divorce rates. But it does not account for all of the research findings on remarriage, such as the finding of Becker et al. (1976) that the presence of children from a previous marriage increased the probability of divorce for women in remarriages, while the presence of children from the new marriage reduced the probability of divorce. I will discuss the implications of this study below, but let me note here that a general decrease in the reluctance of remarried persons to divorce would not explain this finding. Moreover, the previously divorced may be more hesitant to divorce again because of the stigma attached to divorcing twice. Several remarried people I interviewed expressed great reluctance to divorce a second time. They reasoned that friends and relatives excused one divorce but would judge them incompetent at marriage after two divorces.

Yet another explanation for the higher divorce rate is the belief that many remarried men are deficient at fulfilling their economic responsibilities. We know that divorce is more likely in families where the husband has low earnings (Goode 1956). Some remarried men, therefore, may be unable to earn a sufficient amount of money to support a family. It is conceivable that this inability to be a successful breadwinner could account for all of the divorce rate differential, but statistical studies of divorce suggest otherwise. Three recent multivariate analyses of survey data on divorce have shown that remarried persons still had a higher probability of divorce or separation, independent of controls for such socioeconomic variables as husband's earnings (Becker et al. 1976), husband's educational attainment (Bumpass and Sweet 1972), and husband's

[3] Despite the lack of convincing evidence, I am reluctant to discount this explanation completely. Clinical psychologists and psychiatrists with whom I have talked insist that many troubled married persons they have treated had made the same mistakes twice and were in need of therapy to resolve long-standing problems. Their clinical experience should not be ignored, but this "divorce-proneness" syndrome seems inadequate as a complete explanation for the greater problems of remarried people.

and wife's earnings, employment status, and savings (Cherlin 1977). These analyses show that controlling for low earnings can reduce the difference in divorce probabilities, but they also show that low earnings cannot fully explain the difference. It is possible, nevertheless, that a given amount of income must be spread thinner in many remarriages, because of child-support or alimony payments (although the remarried couple also may be receiving these payments). But this type of financial strain must be distinguished from the questionable notion that many remarried husbands are inherently unable to provide for a wife and children.

INSTITUTIONAL SUPPORT

The unsatisfactory nature of all these explanations leads us to consider one more interpretation. I hypothesize that the difficulties of couples in remarriages after divorce stem from a lack of institutionalized guidelines for solving many common problems of their remarried life. The lack of institutional support is less serious when neither spouse has a child from a previous marriage. In this case, the family of remarriage closely resembles families of first marriages, and most of the norms for first marriages apply. But when at least one spouse has children from a previous marriage, family life often differs sharply from first marriages. Frequently, as I will show, family members face problems quite unlike those in first marriages—problems for which institutionalized solutions do not exist. And without accepted solutions to their problems, families of remarriages must resolve difficult issues by themselves. As a result, solving everyday problems is sometimes impossible without engendering conflict and confusion among family members.

The complex structure of families of remarriages after divorce which include children from a previous marriage has been noted by others (Bernard 1956; Bohannan 1970; Duberman 1975). These families are expanded in the number of social roles and relationships they possess and also are expanded in space over more than one household. The additional social roles include stepparents, stepchildren, stepsiblings, and the new spouses of noncustodial parents, among others. And the links between the households are the children of previous marriages. These children are commonly in the custody of one parent—usually the mother—but they normally visit the noncustodial parent regularly. Thus they promote communication among the divorced parents, the new stepparent, and the noncustodial parent's new spouse.

Family relationships can be quite complex, because the new kin in a remarriage after divorce do not, in general, replace the kin from the first

marriage as they do in a remarriage after widowhood. Rather, they add to the existing kin (Fast and Cain 1966). But this complexity alone does not necessarily imply that problems of family unity will develop. While families of remarriages may appear complicated to Americans, there are many societies in which complicated kinship rules and family patterns coexist with a functioning, stable family system (Bohannan 1963; Fox 1967).

In most of these societies, however, familial roles and relationships are well defined. Family life may seem complex to Westerners, but activity is regulated by established patterns of behavior. The central difference, then, between families of remarriages in the United States and complicated family situations in other societies is the lack of institutionalized social regulation of remarried life in this country. Our society, oriented toward first marriages, provides little guidance on problems peculiar to remarriages, especially remarriages after divorce.

In order to illustrate the incomplete institutionalization of remarriage and its consequences for family life, let us examine two of the major institutions in society: language and the law. "Language," Gerth and Mills (1953, p. 305) wrote, "is necessary to the operations of institutions. For the symbols used in institutions coordinate the roles that compose them, and justify the enactment of these roles by the members of the institution." Where no adequate terms exist for an important social role, the institutional support for this role is deficient, and general acceptance of the role as a legitimate pattern of activity is questionable.

Consider English terms for the roles peculiar to remarriage after divorce. The term "stepparent," as Bohannan (1970) has observed, originally meant a person who replaced a dead parent, not a person who was an additional parent. And the negative connotations of the "stepparent," especially the "stepmother," are well known (Bernard 1956; Smith 1953) Yet there are no other terms in use. In some situations, no term exists for a child to use in addressing a stepparent. If the child calls her mother "mom," for example, what should she call her stepmother? This lack of appropriate terms for parents in remarriages after divorce can have negative consequences for family functioning. In one family I interviewed, the wife's children wanted to call their stepfather "dad," but the stepfather's own children, who also lived in the household, refused to allow this usage. To them, sharing the term "dad" represented a threat to their claim on their father's attention and affection. The dispute caused bad feelings, and it impaired the father's ability to act as a parent to all the children in the household.

For more extended relationships, the lack of appropriate terms is even more acute. At least the word "stepparent," however inadequate, has a

widely accepted meaning. But there is no term a child living with his mother can use to describe his relationship to the woman his father remarried after he divorced the child's mother. And, not surprisingly, the rights and duties of the child and this woman toward each other are unclear. Nor is the problem limited to kinship terms. Suppose a child's parents both remarry and he alternates between their households under a joint custody arrangement. Where, then, is his "home"? And who are the members of his "family"? These linguistic inadequacies correspond to the absence of widely accepted definitions for many of the roles and relationships in families of remarriage. The absence of proper terms is both a symptom and a cause of some of the problems of remarried life.

As for the law, it is both a means of social control and an indicator of accepted patterns of behavior. It was to the law, for instance, that Durkheim turned for evidence on the forms of social solidarity. When we examine family law, we find a set of traditional guidelines, based on precedent, which define the rights and duties of family members. But as Weitzman (1974) has shown, implicit in the precedents is the assumption that the marriage in question is a first marriage. For example, Weitzman found no provisions for several problems of remarriage, such as balancing the financial obligations of husbands to their spouses and children from current and previous marriages, defining the wife's obligations to husbands and children from the new and the old marriages, and reconciling the competing claims of current and ex-spouses for shares of the estate of a deceased spouse.

Legal regulations concerning incest and consanguineal marriage are also inadequate for families of remarriages. In all states marriage and sexual relations are prohibited between persons closely related by blood, but in many states these restrictions do not cover sexual relations or marriage between other family members in a remarriage—between a stepmother and a stepson, for example, or between two stepchildren (Goldstein and Katz 1965). Mead (1970), among others, has argued that incest taboos serve the important function of allowing children to develop affection for and identification with other family members without the risk of sexual exploitation. She suggested that current beliefs about incest—as embodied in law and social norms—fail to provide adequate security and protection for children in households of remarriage.[4]

The law, then, ignores the special problems of families of remarriages after divorce. It assumes, for the most part, that remarriages are similar to first marriages. Families of remarriages after divorce, consequently,

[4] Bernard (1956) noted this problem in the preface to the reprinted edition of her book on remarriage "Institutional patterns," she wrote, "are needed to help remarried parents establish relationships with one another conducive to the protection of their children."

often must deal with problems such as financial obligations or sexual relations without legal regulations or clear legal precedent. The law, like the language, offers incomplete institutional support to families of remarriages.

In addition, other customs and conventions of family life are deficient when applied to remarriages after divorce. Stepparents, for example, have difficulty determining their proper disciplinary relationship to stepchildren. One woman I interviewed, determined not to show favoritism toward her own children, disciplined them more harshly than her stepchildren. Other couples who had children from the wife's previous marriage reported that the stepfather had difficulty establishing himself as a disciplinarian in the household. Fast and Cain (1966), in a study of about 50 case records from child-guidance settings, noted many uncertainties among stepparents about appropriate role behavior. They theorized that the uncertainties derived from the sharing of the role of parent between the stepparent and the noncustodial, biological parent. Years ago, when most remarriages took place after widowhood, this sharing did not exist. Now, even though most remarriages follow divorce, generally accepted guidelines for sharing parenthood still have not emerged.

There is other evidence consistent with the idea that the incomplete institutionalization of remarriage after divorce may underlie the difficulties of families of remarriages. Becker et al. (1976) analyzed the Survey of Economic Opportunity, a nationwide study of approximately 30,000 households. As I mentioned above, they found that the presence of children from a previous marriage increased the probability of divorce for women in remarriages, while the presence of children from the new marriage reduced the probability of divorce. This is as we would expect, since children from a previous marriage expand the family across households and complicate the structure of family roles and relationships. But children born into the new marriage bring none of these complications. Consequently, only children from a previous marriage should add to the special problems of families of remarriages.[5]

In addition, Goetting (1978*a*, 1978*b*) studied the attitudes of remarried people toward relationships among adults who are associated by broken marital ties, such as ex-spouses and the people ex-spouses remarry. Bohannan (1970) has called these people "quasi-kin." Goetting presented hypothetical situations involving the behavior of quasi-kin to 90 remarried

[5] In an earlier paper (Cherlin 1977), I found that children affected the probability that a woman in a first marriage or remarriage would divorce only when the children were of preschool age. But the National Longitudinal Surveys of Mature Women, from which this analysis was drawn, contained no information about whether the children of remarried wives were from the woman's previous or current marriage. Since the Becker et al (1976) results showed that this distinction is crucial, we cannot draw any relevant inferences about children and remarriage from my earlier study.

men and 90 remarried women who were white, previously divorced, and who had children from previous marriages. The subjects were asked to approve, disapprove, or express indifference about the behavior in each situation. Goetting then arbitrarily decided that the respondents reached "consensus" on a given situation if any of the three possible response categories received more than half of all responses. But even by this lenient definition, consensus was not reached on the proper behavior in most of the hypothetical situations. For example, in situations involving conversations between a person's present spouse and his or her ex-spouse, the only consensus of the respondents was that the pair should say "hello." Beyond that, there was no consensus on whether they should engage in polite conversation in public places or on the telephone or whether the ex-spouse should be invited into the new spouse's home while waiting to pick up his or her children. Since meetings of various quasi-kin must occur regularly in the lives of most respondents, their disagreement is indicative of their own confusion about how to act in common family situations.

Still, there are many aspects of remarried life which are similar to life in first marriages, and these are subject to established rules of behavior. Even some of the unique aspects of remarriage may be regulated by social norms—such as the norms concerning the size and nature of wedding ceremonies in remarriages (Hollingshead 1952). Furthermore, as Goode (1956) noted, remarriage is itself an institutional solution to the ambiguous status of the divorced (and not remarried) parent. But the day-to-day life of remarried adults and their children also includes many problems for which there are no institutionalized solutions. And since members of a household of remarriage often have competing or conflicting interests (Bernard 1956), the lack of consensual solutions can make these problems more serious than they otherwise would be. One anthropologist, noting the lack of relevant social norms, wrote, "the present situation approaches chaos, with each individual set of families having to work out its own destiny without any realistic guidelines" (Bohannan 1970, p. 137).

DISCUSSION AND SUGGESTIONS FOR RESEARCH

The lack of institutionalized support for remarriage after divorce from language, the law, and custom is apparent. But when institutional support for family life exists, we take it for granted. People in first marriages rarely stop to notice that a full set of kinship terms exists, that the law regulates their relationships, or that custom dictates much of their behavior toward spouses and children. Because they pay little attention to it, the institutional nature of everyday life in first marriages can be easily underestimated. But such support contributes to the unity of first marriages

despite the decline of the patriarch, who was the agent of social control in past time. Institutional guidelines become manifest not only through the transmission of social pressure by a family head but also through the general acceptance of certain habitual behavior patterns as typical of family life. Since this latter process is an ongoing characteristic of social life, the pure "companionship" family—which, in fairness, Burgess and Locke defined only as an ideal type—will never emerge. We have seen this by examining the contrasting case of remarriage after divorce. In this type of marriage, institutional support is noticeably lacking in several respects, and this deficiency has direct consequences for proper family functioning. I have tried to show how the incomplete institutionalization of remarriage after divorce makes the maintenance of family unity more difficult.

One of the first tasks for future research on remarriage is to establish some basic social demographic facts: what proportion of remarried couples have children present from a previous marriage, what proportion have children present from the remarriage, how many children visit noncustodial parents, how frequent these visits are, and so on. As I mentioned, there is no reliable information on these questions now. The U.S. Bureau of the Census, for example, has not discriminated in most of its surveys between parents and stepparents or between children and stepchildren. Yet until figures are available, we can only guess at the number of families which face potential difficulties because of complex living arrangements.

And if we reinterviewed families of remarriage some time after obtaining this information from them, we could begin to test the importance of institutional support for family unity. It follows from the argument advanced here that the more complex the family's situation—the more quasi-kin who live nearby, the more frequently adults and children interact with quasi-kin, the more likely each remarried spouse is to have children from a previous marriage—the more serious becomes the lack of institutional guidelines. Thus, adults in remarriages with a more complex structure should be more likely to divorce or separate in the future, other things being equal. Also, a more complex structure might increase the financial strain on family members, so their earnings and financial obligations should be carefully assessed.

But beyond collecting this fundamental information, we need to discover, by a variety of means, what norms are emerging concerning remarriage and how they emerge. Content analyses of literature, for example, or close study of changes in the language and the law may be illuminating. Just in the past few years, discussion groups, adult education courses, newsletters, and self-help books for remarried parents have proliferated. Whether these developments are central to the institutionalization of remarriage remains to be seen, but they represent possible sources of information about

institutionalization which should be monitored. In addition, detailed ethnographic studies could allow us to uncover emerging patterns of institutionalization among families of remarriages.

And in all these investigations of the institutionalization of remarried life, we must develop a perspective different from that of traditional family research. In much past research—starting with the work of Burgess and others—family sociologists have been concerned primarily with the interpersonal relations of family members, especially of husbands and wives (Lasch 1977). But sociologists' theories—and their research strategies—have assumed, for the most part, that interpersonal relations in families can be accounted for without many references to social institutions. Thus, Burgess and Locke (1945) popularized the notion of the companionship family, whose stability depended largely on what went on within the household. And Locke (1951) measured marital adjustment through a questionnaire which focused largely on such personal characteristics as adaptability and sociability. Yet in order to understand family life—whether in first marriages or remarriages—we must explicitly consider the influences of social institutions on husbands and wives and on parents and children.

We need to know what the institutional links are between family and society which transmit social norms about everyday behavior. That is, we need to know exactly how patterns of family behavior come to be accepted and how proper solutions for family problems come to be taken for granted. And the recent rise in the number of remarriages after divorce may provide us with a natural laboratory for observing this process of institutionalization. As remarriage after divorce becomes more common, remarried parents and their children probably will generate standards of conduct in conjunction with the larger society. By observing these developments, we can improve our understanding of the sources of unity in married—and remarried—life.

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Cross-national Evidence of the Effects of Foreign Investment and Aid on Economic Growth and Inequality: A Survey of Findings and a Reanalysis[1]

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As an outgrowth of dependency theories of national development, there have been a large number of cross-national empirical studies of the effects of foreign investment and aid on economic growth and inequality. This paper reviews these studies in order to discover what can be concluded about these relationships. Our strategy is as follows: First, we discuss the conceptualization of the four main variables at issue. Second, we discuss the differences in the research designs and measurements in the studies. Third, we compare their results and explain contradictory and inconsistent findings. Fourth, we present some new analyses based on this review. We conclude: (1) The effect of direct foreign investment and aid has been to increase economic inequality within countries. (2) Flows of direct foreign investment and aid have had a short-term effect of increasing the relative rate of economic growth of countries. (3) Stocks of direct foreign investment and aid have had the cumulative, long-term effect of decreasing the relative rate of economic growth of countries. (4) This relationship has been conditional on the level of development of countries. The stocks of foreign investment and aid have had negative effects in both richer and poorer developing countries, but the effect is much stronger within the richer than the poorer ones. (5) These relationships hold independently of geographical area.

As a theoretical perspective on development, dependency theory emphasizes that a country's position in the world division of labor is a major cause of development and underdevelopment. One focus in dependency theory has been the analysis of the consequences of foreign investment and foreign aid for the development of the structure of the national economy and state,

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the rate of economic growth, and the degree of inequality within countries. This concern has by now generated a large number of empirical cross-national studies which have investigated these relationships.

This paper reviews a subset of these studies. We want to find out what can be concluded from them about the effects of foreign investment and aid on national economic growth and inequality. We do not discuss the extensive theoretical literature on dependency theory or recent extensions of that theory into world-system analysis. Nor do we discuss critiques of these theories.[2] Our aim is to review a set of empirical studies, to explain inconsistent and contradictory findings, and to summarize the conclusions.

A review of these studies is important to the study of national development because the question of the effects of dependence on growth and inequality is widely debated and considered crucial for many theories of development (Frank 1972; Papanek 1972; Singer 1950, 1975; Heintz 1969; Heintz and Heintz 1973; Senghaas 1977). These writings are widely scattered in the literature of the social sciences, and most researchers are familiar with only a limited subset of them. Also, they present many seemingly contradictory findings. Consequently, different researchers draw different conclusions about the relationships, depending on which studies they use. A review, then, will increase our empirical knowledge about the process of development and aid the building of a theory of development.

To anticipate our general conclusions, we find that foreign investment and aid have the long-term effect of decreasing the rate of economic growth and of increasing inequality. This finding is consistent with dependency theory, but we make no claim that any one finding can either prove or disprove such a complex theoretical framework. One reason is that there are many varieties of dependency theory (O'Brien 1975). Another is that no theory of national development is formulated rigorously enough to be capable of a simple confirmation or refutation (Portes 1976). We do insist, however, that any theory of national development must be able to explain this empirical finding.

We limit consideration to cross-national, quantitative studies which analyze the effects of either foreign investment, including multinational enterprises, or foreign aid on economic growth or inequality.[3] We do not imply that other research designs are inferior. There have been excellent case studies, comparative historical studies, and studies using multinational cor-

[2] For an introduction to this literature see Lall (1975), Oxaal, Barnett, and Booth (1975), Portes (1976), and Wallerstein (1974).

[3] There is also an extensive cross-national empirical literature on the effects of the structure and composition of international trade on economic growth. This body of research requires its own review. For examples, see Alschuler (1976), Delacroix (1977), Galtung (1971), Quinlan (1977), Tyler and Wogart (1973), Rapkin (1976), and Walleri (1976).

porations as the unit of analysis. Our purpose, though, is to compare studies which are similar enough to make it possible to draw valid conclusions. Even limiting our scope in this way, we initially find a number of contradictory and inconsistent findings which must be explained.

Our strategy is as follows: First, we discuss the conceptualization of the four main variables with which we are concerned. Second, we discuss the differences in the research designs and measurements in the studies. Third, we compare their results and attempt to explain the contradictory and inconsistent findings. Finally, we state our conclusions and suggest areas for future research.

We are aware that some researchers working within the dependency framework have criticized the type of studies reviewed here for implicitly claiming that dependent development can be explained as a response to external forces, to the neglect of internal processes of class formation and political organization. We do not believe that these studies should be interpreted in this manner. Studying the effects of external forces such as foreign investment, aid, or trade does not imply that internal structures and processes are unimportant or even less important. But we would go further and argue that any such "internal-external" distinction obscures the fundamental point that all these structures and processes are internal to the world-economy. Therefore, we are not claiming that studies of the type reviewed here pertain to the primary dynamic in development and underdevelopment, but just that they treat one important set of processes in understanding the core-periphery division of labor in the world-economy.

CONCEPTUALIZATION OF THE MAJOR VARIABLES

Table 1 lists the 16 studies grouped according to which independent and dependent variables they include. We refer to the two variables whose effects we are studying as investment dependence and aid dependence. Investment dependence is most often conceptualized as the extent to which a country's economy is penetrated and controlled by direct private foreign capital investment. The operations of subsidiaries of multinational corporations (direct investment), rather than portfolio investment, are often the focus of interest. Some studies, however, combine portfolio and direct foreign investment in their measurement of investment dependence.[4] By most theorists and researchers sympathetic to dependency theory, investment dependence is understood as a structural feature of a national economy which gives some degree of economic and political power to those groups

[4] Direct foreign investment confers rights of ownership and control to the investor. Portfolio investment, usually in government guaranteed bonds, does not legally confer control of operations to the investor

TABLE 1

CROSS-NATIONAL STUDIES OF THE EFFECTS OF FOREIGN INVESTMENT AND AID ON ECONOMIC GROWTH AND INEQUALITY

INDEPENDENT VARIABLES	DEPENDENT VARIABLES	
	Economic Growth	Economic Inequality
Investment dependence . .	Alschuler Bornschier (1975) Bornschier and Ballmer-Cao Chase-Dunn (1975*a*) Evans Kaufman et al. McGowan and Smith Papanek (1973) Ray and Webster Rubinson (1977) Stevenson Stoneman Szymanski	Bornschier (1975, 1978) Chase-Dunn (1975*a*) Kaufman et al. Rubinson (1976) . . .
Aid dependence . . .	Chase-Dunn (1975*a*) Griffin and Enos McGowan and Smith Papanek (1973) Rubinson (1977) Stevenson Stoneman Szymanski	Chase-Dunn (1975*a*) Rubinson (1976) . . . . .

whose interests are involved with the foreign investment. By theorists and researchers working in the framework of neoclassical economics, investment dependence is seen as a flow of resources into a country (Chase-Dunn 1975*a*). These different viewpoints come to affect the way this variable is operationalized in the studies.

Aid dependence refers to the amount of foreign aid, both private and public, that a country receives. Aid dependence also is viewed in two distinct ways. By some researchers, it is seen as creating a control structure by which international or bilateral aid agencies influence the governments that depend on them for resources. By others, aid is seen as a bilateral or multilateral resource flow into countries. Some of the studies we review focus on the extent to which foreign aid is extended as credit, which may result in foreign indebtedness. We include such "debt dependence" in our review of the effects of foreign aid.

Economic development and income inequality are the two major dependent variables with which we are concerned. A national economy is considered developed if it has high levels of internal differentiation, integration, and energy consumption, employs scientific technology in production, and has a high level of labor productivity in all sectors. Most studies use GNP per capita or GDP per capita, or a growth rate of these, as a measure

of economic development.[5] Despite the criticisms of such aggregate me sures, they still remain the best single indicators of economic developmen

Inequality is most often studied by focusing on income inequality, whic refers to the distribution of money income across households or individua in a country. It is understood that this measure is not an estimate inequality of wealth, property ownership, or power, but a variable whic reflects the outcome of a complex interplay of these and other factors. Son researchers use inequality in land holdings or in the product per work across economic sectors as a proxy for income inequality, though the measures may not always be valid substitutes for income inequality (Rubi son and Quinlan 1977).

DIFFERENCES AMONG THE STUDIES

Table 2 presents a summary of the major design factors and measuremen used in the different studies. In this section we discuss the major differenc which are relevant for understanding the findings.

Sample Size and Composition

The number of countries in these studies varies from seven to 91. Son of the researchers attempt to overcome the statistical problems of studyir a limited number of countries by using data at different time points. F example, Stevenson (1972) pools seven yearly cross-sections on seven Lati American nations to give him a sample size of 49 units to analyze; Ston man (1975) pools four time periods of 47 countries to give him a samp size of 188. Most researchers include only formally independent natio with populations greater than 1 million. Half of the studies focus on a pa ticular geographic area, and seven of those exclusively on Latin Americ (Alschuler 1976; Evans 1972; Griffin and Enos 1970; Kaufman, Che notsky, and Geller 1975; Ray and Webster, in press; Stevenson 1972; an Szymanski 1976) and one on Africa (McGowan and Smith 1978).

Nations are sometimes excluded because of extreme scores on one more variables. Oil producing countries are excluded by Kaufman et a (1975) and Rubinson (1977) for this reason. Many authors have also e

[5] GDP is gross domestic product. GDP differs from GNP (gross national product) I including "net factor income paid abroad" and inflows of factor income. GDP th includes all transactions taking place on the national territory regardless of the ci zenship of the actors. GNP includes only transactions among national citizens a excludes flows abroad or inflows from abroad "Factor income paid abroad" is used l some researchers as a measure of dependence because it includes as one of its comp nents the expatriated profits on foreign investments. GNP is a better measure of ec nomic activity which is uncontaminated by the direct exchanges with foreign investo The term "domestic" has created much confusion about GDP and GNP.

TABLE 2

SUMMARY OF CROSS-NATIONAL STUDIES OF THE EFFECTS OF FOREIGN INVESTMENT AND AID ON ECONOMIC GROWTH AND INEQUALITY

Study	N	Sample	Design	Estimator	Time	Investment	Aid	Growth	Inequality	Controls
ischuler	18	Latin America, excluding Cuba, Haiti	Panel residuals	Canonical correlation	1960–65	Factor income paid abroad/national income	None	Deviations of 1960–65 GNP/population	None	Trade dependence
ornschier (1975)	25	Excludes poorest and richest, w/wo Socialist states	Panel correlations	Partial correlation	1950–70 with different lags	Number of MNCs in manufacture/energy consumption	None	GDP/population % growth rate; energy consumed/population	Income share to top 20% of households; Gini Index of sectoral income distribution	Change in the number of MNCs; income inequality; energy consumption
ornschier and Ballmer-Cao	91	All with data; also analyses in subsamples	% growth of national income per capita with lagged and logged national income per capita	Multiple regression	1960–75	Book value of the total stock of private foreign direct investment/total energy consumption × population	None	% growth rate of national income per capita, 1960–75	Income share to top 20% of households	Lagged ln national income per capita, capital formation; % growth of foreign stock; income inequality; population growth
ornschier (1978)	72	All with data, w/wo Socialist states and highest developed, developing countries	Cross-sectional	Multiple regression	Differs with year of inequality survey, about 1965–70	Book value of the total stock of private foreign direct investment/total energy consumption × population	None	None	Income share to quintiles and top 5% of households; Gini Index	Ln national income per capita; linear and nonlinear models
hase-Dunn (1975a)	36	Excludes developed countries	Panel analysis; cross-sectional	Multiple regression	1950–70	Debits on foreign investment/population	External public debt/population	GNP/population; KWH/population	Gini Index of personal and sectoral inequality	Savings; % GDP in mining
vans	19	Latin America, excluding Cuba	Panel analysis; cross-sectional	Multiple regression	1950–66	(Book value of U.S direct investment/GNP) × (number of MNCs/GNP)	None	GNP/population; % GDP in manufacturing	None	Lagged dependent variable
riffin and Enos	12	Latin America	Cross-sectional with % growth rate	Multiple regression	1957–64	None	Total flows of aid and investment capital	% growth rate of GNP, 1957–64	None	None
Kaufman et al	17	Latin America, excluding Cuba, Haiti, Venezuela	Cross-sectional with % growth rate	Zero order correlation	1961–69, individual variables at different times	Inflow of capital/population, inflow of capital/GNP; book value of U S direct investment/population	None	% growth rate of GNP, 1961–69	Gini Index of land and sectoral inequality	None

TABLE 2 (*Continued*)

Study	N	Sample	Design	Estimator	Time	Investment	Aid	Growth	Inequality	Controls
McGowan and Smith	30	Sub-Saharan Africa	Cross-sectional with % growth rate	Partial correlation	1961–68, individual variables in 1967	Book value of direct investment/population; total book value of direct investment	None	% growth rate of GNP/population, 1961–68	None	None
Papanek (1973)	34	Excludes developed countries	Pooled cross-sections with % growth rate	Multiple regression	1950–70	Inflows of long-term borrowing and net direct investment	Flows of net transfers to government; long-term loans	% growth rate of GDP	None	Savings; development level; population; aid and investment in same equation
Ray and Webster	19	Latin America, excluding Cuba, Haiti	Cross-sectional with % growth rate, panel analysis, panel residuals	Multiple regression	1960–70	Book value of U.S. investment/population, factor income paid abroad/NI, inflow of investment/population	None	% growth rate of GNP, 1960–70; deviations of 1960–70 GNP/population, GNP/population	None	GNP/population; lagged dependent variable
ubinson (1976)	39	All with data except Socialist states	Cross-sectional	Multiple regression	Differs with year of inequality survey, about 1965	Debits on foreign investment/GDP	External public debt/population	None	Income shares received by quintiles and Gini Index	Ln KWH/population; savings/GDP; government revenue/GDP
ubinson (1977)	42, 37	All with data except Socialist states	Panel, weighted least squares	Multiple regression	1955–70	Debits on foreign investment/GDP	External public debt/population	GNP	None	Population; government revenue: interactions of government revenue and level of development
tevenson	7	Latin America	Pooled cross-sections	Rank order correlation	1961–67	Inflows of U.S. direct investment	Inflows of foreign aid	GDP/population	None	None
toneman	65	Excludes developed, includes analyses by region and time period	Pooled cross-sections with % growth rate	Multiple regression	1950–70	Stocks of foreign investment/GDP, net inflows of foreign investment/GDP	Net inflows of foreign aid/GDP	5-year growth rates of GDP	None	Savings/GDP, all investment and aid variables in same equation
zymanski	19, 12	Latin America	Cross-sectional with % growth rate	Rank order correlation	1960–72	Book value of U.S. direct investment/GDP; net repatriated profits on U.S. investment/GDP	U.S. aid/GDP	% growth rate of GDP/population, 1960–72; % growth rate of GDP/population in manufacturing	None	Savings; level of development; population; aid and investment variables in same equation

cluded Socialist countries either for theoretical reasons or because of problems of data availability. Some studies exclude the most developed countries because they want to study only developing countries (Bornschier 1975; Chase-Dunn 1975*a;* Papanek 1973; and Stoneman 1975).

Many studies create subsamples and analyze them separately. Bornschier (1975) *does his analysis with and without the Socialist countries*, and Bornschier and Ballmer-Cao (1978) present analyses separately for highly developed countries, large developing countries, and small (and poor) developing countries. Stoneman (1975) divides his sample by geographical area, presenting separate analyses for Latin America, Africa, Asia, and Mediterranean countries. Rubinson (1977) presents analyses for developed and underdeveloped countries. Ray and Webster (in press) present results for their Latin American sample with and without Venezuela.

The sample size and composition of these studies, then, varies greatly For any one study viewed in isolation, this presents problems of scope and generalizability. But for our purposes, this variability provides distinct advantages. One advantage is that we have replications of many similar samples. Another is that some countries excluded by some studies have been included in others, so we can check on possible biases produced by these exclusions. And, as a third advantage, we can make a wide variety of comparisons for the effects we are studying: the effects in developed versus developing countries, the effects within developing countries, the effects within geographical areas, and the effects between such areas. As we report later, such comparisons allow us to say something about the conditions under which dependence effects occur.

Research Designs

All these studies attempt to infer causal relationships from nonexperimental research designs. The primary difference in these designs lies in the way in which the temporal dimension is employed. There are seven different designs: cross-sectional, pooled cross-sectional, cross-sectional with a percentage growth rate as the dependent variable, the same with the addition of a lagged dependent variable, pooled cross-sectional with a percentage growth rate as the dependent variable, panel analysis, and panel residuals

A simple cross-sectional design compares measures on variables at one point in time without attempting to measure change over time in any of the variables. This design is employed by Rubinson (1976) and by Bornschier (1978).

A pooled cross-sectional design combines cross-sections for different years into a single analysis. This design is used by Stevenson (1972). Like the simple cross-section, no measure of change is included in the analysis.

The most common design has been a cross-section with a percentage change score used as the dependent variable. For instance, the dependent variable used by Szymanski (1976) is the average yearly growth rate of GDP per capita for the period 1960–72, with the independent variables measured in 1960. This design is also used by Griffin and Enos (1970), Kaufman et al. (1975), and McGowan and Smith (1978). Ray and Webster (in press) also employ it as one of their designs and, using the same data, compare it with others. It should be noted that the time at which independent variables are measured is sometimes not the beginning of the period for which the percentage growth rate is calculated. For example, Kaufman et al. estimate the effect of an independent variable measured in 1929 on a growth rate measured between 1961 and 1969. Most independent variables, however, are measured within the time period for which the percentage growth rate is calculated, but not always at the beginning of this period.

The fourth design is a modification of the previous one, in which a lagged dependent variable is included in the regression equation. Thus, Bornschier and Ballmer-Cao (1978) introduce GNP per capita in 1960 as a control variable in an equation in which the dependent variable is the percentage change in GNP per capita between 1960 and 1975. The inclusion of GNP per capita in 1960 in the equation is intended to control for the relationship between the dependent variable and the independent variables at the first point in time.

The fifth design involves the pooling of cross-sections with the dependent variable being a percentage growth rate For example, Stoneman (1975) pools four cross-sectional series together, with the dependent variable in each series being a percentage growth rate. Papanek (1973) also uses this design.

The sixth design is panel analysis, in which the dependent variable at time t is regressed on its value at time $t - 1$ (the lagged dependent variable) and other independent variables measured at $t - 1$. For example, Evans (1972) regresses GNP per capita in 1966 on GNP per capita in 1950 and on investment intensity in 1950. This design is also used by Chase-Dunn (1975*a*) and by Rubinson (1977) And Bornschier (1975) used the same design with panel correlations instead of regressions.

The final design is similar to panel analysis except that the dependent variable is the residuals of the regression of an indicator on itself at an earlier point in time. Thus, Alschuler (1976) uses as his measure of economic growth the residuals from the regression of GNP per capita in 1965 on GNP per capita in 1960. Ray and Webster (in press) also use this method

These studies, then, employ a great variety of research designs in relation to the handling of the temporal dimension in the measurement of economic

growth. Several researchers have investigated the potential biases in the use of different types of change scores and panel designs (Bohrnstedt 1969; Heise 1970; Ray and Webster, in press). With a few exceptions to be noted later, however, we do not find that the different designs used in these studies have affected the findings.

Statistical Estimators

Most of the studies employ linear regression to estimate the relationships between variables. Two studies use partial correlation; three studies use rank order or zero order correlations; and one study uses canonical correlations.

Time Period

The time period most frequently analyzed is the 1960s. Some analysts use data as recent as 1975, and Kaufman et al. (1975) use a measure dating from 1929. Several studies use data from the 1950s to 1970 (Bornschier 1975; Chase-Dunn 1975*a;* Rubinson 1977; Papanek 1973; and Stoneman 1975). Thus, these studies cover the entire post–World War II period. It is important to keep in mind that the results found during this period may be conditional on certain characteristics of the world-economy during this 20-year period.

Measurement

The studies often use different indicators to measure the four major variables, and we discuss each one in turn. All the studies weight the absolute size of foreign investment (whether stock or flow) and aid by some measure of size, either population, gross product, or energy consumption.

1. *Investment dependence.*—To obtain a measure of an economy's structural dependence on foreign investment most researchers try to approximate a ratio of the value of stock of foreign direct investment to the domestically owned capital stock of the country. This ideal indicator would measure the proportion of the capital ownership of a country that is controlled by foreign actors, that is, it would measure the degree of foreign capital penetration. Data to construct such a measure are not available for most countries, so none of the studies actually uses this measure. Only since the OECD study of 1972 have there been estimates of the stock of private foreign direct investment for a large number of underdeveloped countries, but this estimate is available only beginning in 1967. The studies by Bornschier (1978) and Bornschier and Ballmer-Cao (1978) used these OECD

data. The other studies that measure stocks of foreign investment estimate the total value by the various methods discussed below.

Some measure the value of foreign investment flowing into a country in a particular year, including reinvested earnings, as shown in the IMF *Balance of Payments Yearbook* (1950–70) (Kaufman et al. 1975; Papanek 1973; Stevenson 1972). The decision to use such a "flow" measure as opposed to a "stock" measure is quite significant, as we show below. Stock measures estimate the total cumulated value of foreign-owned capital in a country. Flow measures are based on current account inflows of foreign capital for some time period.

To measure stock value of foreign investment other than by using the OECD figures, researchers have used four methods. (1) Several studies of Latin America have used various U.S. Department of Commerce figures on the value of U.S. investment in those countries (Evans 1972; Kaufman et al. 1975; Ray and Webster, in press; Szymanski 1976). These figures are analogous to those of the OECD except that they measure only the book value of United States foreign investment. They do not, of course, take into account the value of investment in Latin America by other countries such as England, Germany, Japan, and France and are therefore a rather problematic proxy.[6] (2) Two studies use the number of subsidiaries of multinational corporations operating in each country as a measure of investment dependence (Bornschier 1975; Evans 1972). (3) Several studies use the amount of profit made by foreign controlled firms as indicated in the IMF *Balance of Payments Yearbook* item: "debits on direct investment income" (Chase-Dunn 1975*a*; Rubinson 1976, 1977; Szymanski 1976), or the somewhat less specific item of "factor income paid abroad" (Alschuler 1976; Ray and Webster, in press). (4) Stoneman (1975) estimates the stock of foreign investment by cumulating yearly flows of foreign investment, including reinvested earnings from 1950, into five-year sums. He combines these sums with the 1967 OECD (OECD 1972) data to estimate stocks for earlier years.

2 *Aid dependence.*—The distinction between flows and stocks which was discussed with regard to investment dependence can also be applied to aid dependence, although there is no aid equivalent to foreign ownership. Flows

[6] According to the OECD (1972) estimates of the book value of the stock of private foreign direct investment in 1967 (in developing countries), $17 4 billion was controlled by U S. actors and $17.7 billion was controlled by actors from other developed countries The U S share was not the same across regions. For example, in Africa the U.S. share was $1 4 billion and the non-U.S share $5.2 billion. In South America the U.S. share was $7 4 billion and the non-U.S share $4 7 billion. Nor are the relative shares of U S and non-U.S. investment the same across countries within regions. Within Latin America, e g, the U.S share in Argentina is 56%, in Brazil 36%, in Colombia 86%, and in Venezuela 73%. These figures strongly suggest that the U.S. stock of foreign investment is not a good indicator for total foreign investment.

of public grants and loans to development projects are indicated on current account in the IMF *Balance of Payments Yearbook*. Some researchers have combined flows of private and public capital into a single measure of aid dependence (Griffin and Enos 1970; Kaufman et al. 1975; Ray and Webster, in press), while others examine aid flows separately from private capital flows (Papanek 1973; Stoneman 1975; Stevenson 1972).

Four studies attempt to measure overall structural aid dependence, the conceptual equivalent of stocks of foreign investment. Szymanski (1976) uses the accumulated total of U.S. aid flows to Latin American nations from 1945 to 1960. Chase-Dunn (1975*a*) and Rubinson (1976, 1977) use the accumulated government, or government guaranteed, foreign debt as presented by the IBRD *World Tables* (1971).

3. *Economic growth.*—Most of the studies use GNP per capita as a measure of level of economic development and study its rate of change over some time period to estimate growth. Three studies also analyze economic growth in the manufacturing sector (Chase-Dunn 1975*b*; Evans 1972; Szymanski 1976). Four studies employ measures of change in total GNP rather than in GNP per capita (Griffin and Enos 1970; Kaufman et al. 1975; Ray and Webster, in press; Stoneman 1975).[7] Chase-Dunn (1975*a*) and Bornschier (1975) use, in addition, measures of energy consumption per capita and labor force structure to measure economic development.

4. *Inequality.*—Inequality is measured in three ways in these studies: by individual or household income inequality, sectoral inequality, and land tenure inequality. Most of the studies use personal income inequality, which is the relative distribution of money income across individuals or households in a country (Chase-Dunn 1975*a*; Bornschier 1975, 1978; Bornschier and Ballmer-Cao 1978; Rubinson 1976). Income inequality is measured either by the Gini Index, the percentage of income going to the richest 5% and to the richest 20% of income recipients, or by the percentage of income to each quintile.

Sectoral inequality, the relative distribution of per worker product in eight economic sectors, is used by Chase-Dunn (1975*a*), Bornschier (1975), and Kaufman et al. (1975). Although some researchers use it as a proxy for income inequality, it is understood by others as an indicator of uneven economic development. Kaufman et al. (1975) also use land tenure inequality, a measure of the distribution of the geographical size of land holdings. These measures of inequality are not measured over time because of problems of data availability, and so all the analyses using inequality measures are cross-sectional.

[7] Rubinson (1977) also uses total GNP rather than GNP per capita, but he estimates the equations using weighted least squares, weighting by population This technique produces results equivalent to the use of per capita measures.

Control Variables

The studies also differ in their use of other variables to control for spurious factors or to better specify the model. The need for proper control or specification is important because of the potential biases that could affect the relationships being studied. Four of the studies do not include any control variables (Griffin and Enos 1970; Kaufman et al. 1975; Stevenson 1972; and Szymanski 1976). And in relating the economic growth rate to the stock of foreign direct investment per capita, McGowan and Smith (1978) also do not use any control variables.

In studying the effects of foreign investment and aid on economic growth and inequality, researchers have included six types of variables to control the relationships or specify the model. First, several studies control for other types of dependence measures. Alschuler (1976) controls for trade dependence. In Stoneman's (1975) analysis of economic growth, he includes the stock of foreign investment, flows of foreign private investment capital, and flows of foreign aid in the same equation. Bornschier (1975) controls for the change in the number of multinational subsidiaries when estimating the effect of the level of penetration of multinational corporations on economic growth. The use of such controls as these allows researchers to sort out the independent effects of different types of dependence.

Second, several studies control for the effects of capital formation or domestic savings. Controlling for capital formation is important because this variable is an important cause of economic growth, and low levels of domestic capital formation may cause foreign capital to flow in to take up investment opportunities. The studies that control for this factor are those of Bornschier and Ballmer-Cao (1978), Chase-Dunn (1975*a*), Papanek (1973), Rubinson (1976), and Stoneman (1975).

Third, Chase-Dunn (1975*a*) and Rubinson (1977) control for the amount of mineral resources in a country. The possession of mineral resources has a positive effect on economic growth and also is likely to attract foreign capital to exploit these resources This variable was controlled by including in the models the percentage of GDP in mining and petroleum production.

Fourth, several authors control for the relationship between foreign investment or aid and the prior level of economic development. This control is important in order to sort out the direction of causal effects. It is often argued that foreign investment or aid goes to countries with a relatively higher level of economic development because there are greater investment opportunities. This mechanism would produce a positive relationship between level of economic development and foreign investment at any one point in time. To study the effect of foreign investment on later economic

growth, this initial positive correlation between economic growth and foreign investment must be controlled. That is, two processes may be occurring: one by which economic growth leads to more foreign investment, and another by which foreign investment reduces the rate of economic growth. Studies which control for the former relationship are Bornschier (1975), Bornschier and Ballmer-Cao (1978), Chase-Dunn (1975*a*), Evans (1972), Ray and Webster (in press), and Rubinson (1977).

Fifth, one study, that by Bornschier and Ballmer-Cao (1978), controls for population growth to test the hypothesis that the effects of dependence on economic growth are mediated by population growth.

Sixth, two studies specify their equations by including measures of intervening mechanisms by which foreign investment or aid is thought to affect economic growth. Thus, Bornschier and Ballmer-Cao (1978) control for income inequality, and Rubinson (1977) controls for the degree of state strength in a country.

COMPARISON OF RESULTS

We now turn to a comparison of the results of the studies under review. The question at issue here is, What are the effects of foreign investment and aid on economic growth and inequality? Table 3 contains a summary of these findings presented in terms of the direction (positive or negative) of the effect. We first discuss the findings upon which all the studies agree, and then we consider the problems raised by the contradictory or inconsistent findings.

Inequality

The five studies dealing with inequality all find that the effects of dependence on inequality are positive. That is, these studies show that dependence tends to increase the amount of inequality within countries. Five studies show positive effects of measures of investment dependence on measures of inequality (Bornschier 1975, 1978; Chase-Dunn 1975*a*; Kaufman et al. 1975; and Rubinson 1976). Two studies show positive effects of measures of aid dependence on inequality (Chase-Dunn 1975*a*; Rubinson 1976). No studies find either zero or negative effects of dependence on inequality.

Given the unanimity of these results, we conclude that investment and aid dependence have the effect of increasing inequality. We note, however, that all the studies of inequality are cross-sectional because sufficient data on inequality have not been available for a longitudinal analysis. It is possible, therefore, to also argue that countries with more unequal income

TABLE 3

THE DIRECTION OF EFFECTS OF FOREIGN INVESTMENT AND AID ON ECONOMIC GROWTH AND INEQUALITY AS REPORTED IN CROSS-NATIONAL STUDIES

	Economic Growth	Economic Inequality
Investment dependence:		
Positive effects	Kaufman et al.	Bornschier (1975, 1978)
	McGowan and Smith	Chase-Dunn (1975*a*)
	Papanek (1973)	Kaufman et al.
	Ray and Webster	Rubinson (1976)
	Stoneman (flows)	
	Szymanski	. .
Negative effects	Alschuler	. .
	Bornschier (1975)	
	Bornschier and Ballmer-Cao	.
	Chase-Dunn (1975*a*)	.
	Evans	. . .
	Rubinson (1977)	.
	Stevenson	
	Stoneman	
Aid dependence:		
Positive effects	Kaufman et al.	Chase-Dunn (1975*a*)
	McGowan and Smith	Rubinson (1976)
	Papanek (1973)	.
	Ray and Webster	
	Stoneman (flows)	
	Szymanski	. . .
Negative effects	Chase-Dunn (1975*a*)	. . .
	Griffin and Enos	.
	Rubinson (1977)	.
	Stevenson	. .

distributions attract more foreign investment and aid and that inequality and dependence form a mutually reinforcing pattern. Studying these reciprocal processes, then, becomes an important area for further research.[8]

Given that the evidence indicates that dependence increases inequality, there is a need for studies which research the mechanisms of this relationship A number of theoretical discussions have already developed several hypotheses that can be tested in future research (Amin 1976; Girling 1973; Bornschier 1978). The different types of inequality studied may also reveal different processes. The effect of dependence on sectoral inequality may be due to the peculiarly uneven development caused by foreign direct investment, which causes growth in the immediate enterprise or sector in which it is invested but structurally retards overall economic growth in other sectors. The effect of dependence on income inequality is most likely due to its effects on the class structure of the country and the translation of this class structure into political power. These hypotheses are promising areas for future research.

[8] Chase-Dunn (1975*b*) performs a crude test of the hypothesis that inequality causes dependence by entering inequality in a panel model examining the causes of dependence He finds no effects of inequality on dependence

Economic Growth

Table 3 shows that there are contradictory findings about the effects of foreign investment and aid on economic growth.

Six studies find positive effects of investment dependence on economic growth; eight find negative effects. Six studies find positive effects of foreign aid on economic growth; four find negative effects. Using our previous examination of the differences among the studies, we now attempt to account for these different findings.

Stocks and Flows

One difference which may account for some of these contradictory findings is suggested by our earlier discussion of studies which measure dependence by using measures of flows instead of measures of stocks. Recall that measures employing flows look at inflows of capital or aid on current account, while those which employ stocks try to measure the total amount of accumulated foreign capital or aid in a country. Flow measures, then, describe the amount of foreign capital or aid coming into a country within a limited time period, while stock measures describe the accumulated amount that exists in a country. Table 4 lists the studies of economic growth by whether they employ flow or stock measures or both.

Table 4 presents an interesting set of results. Looking at the studies which employ measures of stocks, we see that seven of the 11 find negative effects on growth. Of the four studies which find positive effects, note that all of them are restricted to a single geographical area; three to Latin America and one to sub-Saharan Africa. Of the six studies which employ

TABLE 4

STUDIES OF THE EFFECTS OF FOREIGN INVESTMENT ON ECONOMIC GROWTH BY MEASUREMENT OF FOREIGN INVESTMENT AND DIRECTION OF EFFECTS

Measurement of Foreign Investment	Direction of Effects	
	Positive	Negative
Stocks	Kaufman et al.	Alschuler
	McGowan and Smith	Bornschier (1975)
	Ray and Webster	Bornschier and Ballmer-Cao
	Szymanski	Chase-Dunn (1975*a*)
		Evans
	.	Rubinson (1977)
	.	Stoneman
Flows	Kaufman et al.	Griffin and Enos
	Papanek (1973)	Stevenson
	Ray and Webster	
	Stoneman	..

measures of flows, four find positive effects and only two find negative effects. This pattern of findings is the same for the effects of both investment and aid dependence.

Thus, employing the distinction between stocks and flows, we can reconcile a good number of the seemingly contradictory findings about the direction of effects of dependence on economic growth. The results of table 4 suggest, then, that current flows of investment capital and aid have positive effects on growth, whereas long-term stocks of foreign investment and aid have negative effects. The findings of Griffin and Enos (1970) for aid and of Stevenson (1972) for investment are the only studies of flows which do not fit this pattern.[9] These two findings may be due to sampling error, since Stevenson uses only seven cases and Griffin and Enos use only 12.

This difference in findings as a consequence of whether one measures dependence by stocks or flows is more than just a measurement result. It should be interpreted as a substantive finding. What this pattern suggests is that the immediate effect of inflows of foreign capital and aid is to increase the rate of economic growth, while the long-run cumulative effects operate to reduce the rate of economic growth. This has been suggested by Bornschier (1975) and by Bornschier and Ballmer-Cao (1978). And there is a variety of other evidence in these studies to support such a proposition. Stoneman (1975) includes both stock and flow measures in the same analysis; he finds that the flow measures are consistently positive while the stock measures are consistently negative. He also reports that lagging the stock measure increases the significance of the negative effect. Chase-Dunn (1975*b*) finds in a series of panel analyses that stock measures tend to have zero effects on growth over short time lags but increasingly negative effects over longer and longer lag periods. That is, the immediate effect of stocks is zero, but the longer-term effect is negative. This same result is shown by Bornschier (1975) with a different measure of investment dependence. Bornschier (1975) also shows a similar result in analyses in which he includes both the level of penetration by multinational subsidiaries and the change in the level of penetration. He finds that short-term increases in penetration have positive effects on growth but become zero and then negative as the lag period is lengthened.

These results tend to confirm the hypothesis that current inflows of investment capital and aid cause short-term increases in growth due to the contribution to capital formation and demand as foreign corporations purchase land, labor, and materials and start production, while the long-run structural distortions of the national economy produced by foreign investment and the exporting of profits tend to produce negative effects over time.

[9] We consider the positive findings about the effect of stocks of foreign investment on growth below.

We conclude, then, that the effect of short-term flows of investment and aid has positive effects on growth, but that their cumulative effect over time is negative. Many of the seemingly contradictory findings of these studies can be reconciled under this proposition.

Geographic Subsamples

Distinguishing between studies using measures of flows and those using measures of stocks eliminates many of the apparent contradictions in table 3, but there still remain four studies which use measures of stocks and find positive effects of foreign investment or aid on economic growth (Kaufman et al. 1975; McGowan and Smith 1978; Ray and Webster, in press, Szymanski 1976). The common feature of these studies is that each one studies only a particular geographic area. The studies by Kaufman et al., Ray and Webster, and Szymanski concern Latin America only; that by McGowan and Smith concerns black Africa only. However, in table 3, there are also two studies which find negative effects of the stock of foreign investment in Latin American samples (Alschuler 1976 and Evans 1972). Stoneman (1975) also presents some separate analyses by region, and he finds negative effects in his Latin American sample and positive effects in his Asian and African samples. In this section, then, we discuss the contradictory findings that appear in studies of separate geographic regions

Table 5 shows those studies which use stocks measures by the samples they use and their findings. We note first that all five studies which use samples unrestricted by geographic region find negative effects (Bornschier 1975; Bornschier and Ballmer-Cao 1978; Chase-Dunn 1975*a*; Rubinson 1977; Stoneman 1975). These five use either all countries for which data were available, including both developed and underdeveloped, or just the

TABLE 5

STUDIES OF THE EFFECTS OF STOCK OF FOREIGN INVESTMENT ON ECONOMIC GROWTH BY SAMPLE COMPOSITION AND DIRECTION OF EFFECTS

Sample Composition	Effect of Stock of Foreign Investment	
	Positive	Negative
Unrestricted	.	Bornschier (1975)
	.	Bornschier and Ballmer-Cao
	.	Chase-Dunn (1975*a*)
	.	Rubinson (1977)
		Stoneman
Latin America . . .	Kaufman et al.	Alschuler
	Ray and Webster	Evans
	Szymanski	Stoneman
Africa	McGowan and Smith	.
	Stoneman	.
Asia . . .	Stoneman	.

less developed countries. This table shows, then, that with large samples, unrestricted by region, only negative effects are found. On the other hand, of the six studies of Latin America, three find positive effects and three find negative ones. The two studies of Africa and the one of Asia find positive effects. We see, then, a clear difference: the studies with large unrestricted samples show consistent negative effects; conflicting findings appear in the studies of a single geographic region, Latin America.

In comparing the three studies of Latin America which find positive effects with the three that find negative effects, it is not easy to discover factors which could explain the different findings. The studies include basically the same set of countries, with the inclusion or exclusion of Venezuela being the main difference. Ray and Webster (in press) show that the including or excluding of Venezuela has little effect. Sample composition, then, is not an explanation. The measurement of foreign capital dependency, however, suffers from shortcomings in all studies except Stoneman's (1975). Evans (1972), Kaufman et al. (1975), Ray and Webster (in press), and Szymanski (1976) use U.S. figures for the stock of foreign direct capital as a proxy for the total stock. This is a problematic proxy (see n. 6). We should point out, in addition, that none of the findings are very large in a statistical sense.[10] Also, the sample size ranges from 17 to 19 cases, which is rather small for correlation and regression analysis, since differences in one or two cases can affect the magnitude and direction of estimates.

Thus, our comparison of the different studies reveals no systematic reason to explain the different findings of the studies of Latin America. Similarly, the positive effect found in the studies of the African and Asian subsamples is problematic. McGowan and Smith's (1978) finding of a positive effect of investment on growth may be due to the fact that they use a very short lag; as noted above, the longer-term effects of foreign investment are negative. Stoneman's (1975) findings, however, do not suffer from this potential drawback. The problem, then, is to explain why the studies of separate geographic regions diverge in findings from those with larger and unrestricted samples.

Thinking about the pattern in table 5, we can offer four hypotheses to explain these findings from the studies of geographic regions:

[10] Only Ray and Webster (in press) find large and significant positive effects of the stock of foreign investment on growth within Latin America But in reanalyses of their results and those of the other Latin American studies, we found that their unusually large effects resulted from their use of the GNP per capita data compiled by the USAID Use of the GNP per capita data compiled by either the World Bank or the United Nations produces small positive effects similar to the findings of Kaufman et al (1975) and Szymanski (1976). Given the extreme care with which the World Bank and the United Nations compile GNP data, we suspect that data from either of these organizations are superior to those provided by the USAID.

Hypothesis 1.—The discrepant results in geographical regions may indicate that the relationship between foreign investment and growth actually varies by geographical region. Although the researchers who use only regional subsamples do not offer any theoretical reason why the relationship should vary by region, the possibility still exists.

Hypothesis 2.—A second explanation may be that variables which should be controlled on theoretical grounds are not associated with foreign capital in world samples but are in regional subsamples. A case in point is the variable "domestic capital formation," which has a substantial correlation with the stock of foreign capital in the total sample, but quite different correlations in regional subsamples. Therefore, a more specified, but spurious "geographical-region hypothesis" may be operating. To test this hypothesis requires that the same set of variables be used in each equation that tests the effects of foreign investment in the regional samples.

Hypothesis 3.—These results may indicate that the relationship between foreign investment and growth varies with the level of development of countries. Most countries in Asia and Africa are among the most underdeveloped countries in the world, while Latin American countries are much richer. Geographical samples, then, confound the effect of region with the effect of level of development. Some evidence for this explanation is found in Bornschier and Ballmer-Cao's (1978) study. They find that the relationship between foreign investment and growth is negative for the sample of all less developed countries; but within the sample of the very small (in terms of the absolute size of the modernized segment) and poor countries (mainly African: 25 out of 38), they find a small positive effect. These effects, however, become negative after the introduction of controls (especially capital formation). The hypothesis that the effect might vary by level of development is also suggested by McGowan and Smith (1978) and by Stoneman (1975).

Hypothesis 4.—The results may be a "statistical artifact" produced by the limited range of variation and small sample sizes that naturally characterize any such subsample analysis. Since all the studies which use larger samples with greater variation find negative effects, it is possible that the studies which use smaller samples with limited variation are subject to greater sampling error. That is, if we imagine that the true regression line relating foreign investment to growth is negative, it is still possible that any limited subset of points along that line shows a different relationship between investment and growth than the entire set of points. Stoneman (1975) suggests this explanation to account for why his African and Asian subsamples differ from his much larger sample of all less developed countries.

NEW ANALYSES

Our method of comparing and contrasting the differences among the studies does not allow us to choose among these four alternatives. The effects of level of development, geographical region, and limited variation are all confounded in those studies which use only countries from a single region. Therefore we now present results from a new set of analyses to try to clarify these different findings. The new analysis is done in two ways. First, following the form of the previous studies in table 5, we present analyses separately by the different regions and level of development, using the set of control variables our previous review has shown to be important. Second we use a large sample of developing countries in an analysis of covariance model to allow us to test simultaneously for the possible interactions between foreign investment and region and between foreign investment and level of development, as suggested by the hypotheses. This analysis of a large sample also allows us to overcome the problems of small sample size and restricted variation that occur in the separate analyses by region.

Tables 6 and 7 report these analyses, which include 76 less developed countries (listed in Appendix A). Excluded from the analyses are the 1! most highly developed countries and the Socialist nations. The variables included in all the equations in tables 6 and 7 are as follows: The dependent variable is the growth rate of gross national product per capita from 1960 to 1975.[11] The stock of direct foreign investment, referred to as

TABLE 6

REGRESSION ESTIMATES OF THE EFFECTS OF THE STOCK OF PRIVATE FOREIGN DIRECT INVESTMENT ON ECONOMIC GROWTH, 1960 1975, FOR DIFFERENT SAMPLES

Eq	Sample	FORCAP	FORINV	CAPFOR	GNP	$\bar{R}^2$	
(1)	All developing countries	− 004*	1 46	3 09*	44 24*	448	7
		(3 76)	(1 41)	(4 76)	(3 95)		
(2)	Richer developing countries	− 005*	931	4.84*	−14 52	573	3
		(4 26)	(79)	(5 37)	(645)		
(3)	Poorer developing countries	− 003	1 35	2 76*	58 85	229	3
		(1 50)	(740)	(2 41)	(1 36)		
(4)	Latin America	− 001	960	2 54*	14 52	185	2
		(63)	(1 03)	(1 82)	(69)		
(5)	Asia	− 007*	2 83	2 15	114 52*	651	1
		(2 04)	(81)	(1 39)	(3 41)		
(6)	Africa	− 0002	1 35	3 26*	13 29	257	3
		(116)	(865)	(3 18)	(509)		

NOTE — Variables defined in text, numbers in parentheses are t-statistics, $\bar{R}^2$ is R^2 adjusted for loss of degre of freedom

* $P < 10$.

[11] The growth rate is measured as the ratio of GNP per capita in 1975 to GNP p capita in 1960 (GNP75/GNP60). The same conclusions from the analyses are reach if a compound growth rate is used.

TABLE 7

REGRESSION ESTIMATES OF THE EFFECTS OF THE STOCK OF PRIVATE FOREIGN DIRECT INVESTMENT ON ECONOMIC GROWTH, 1960–1975, FOR 76 DEVELOPING COUNTRIES

Eq.	FORCAP	FORINV	CAPFOR	GNP	FCDEV	FCLAT	FCASIA	FCAFR	$\bar{R}^2$	N
(1)......	− 004*	1 46	3.09*	44 24*		.	.	.	.448	76
	(3.76)	(1 41)	(4 76)	(3 95)						
(2)......	− 002	1.30	3.03*	28.30	−.003*		.	..	.457	76
	(1.51)	(1.26)	(4 67)	(1 37)	(1 73)					
(3)......	− 004*	1.49	2 85*	50 04*		001	..	.	.448	76
	(3 47)	(1 43)	(4 19)	(4 09)		(573)				
(4)....	− 002*	1 36	2 74*	47 23*	.		−.002	..	.498	76
	(1.93)	(1 36)	(4 34)	(4 40)			(1.03)			
(5)......	− 005*	1 33	3 11*	33 05*	.		.	.004*	.491	76
	(3 88)	(1 33)	(4 99)	(2 60)				(2 34)		
(6)......	− 002	1 26	2.60*	25 55	− 005*	005*	.	..	473	76
	(1.31)	(1.23)	(3.86)	(1 25)	(2 28)	(1.73)				
(7).......	0002	1 20	2 64*	26 82	−.004*		− 003	..	.522	76
	(129)	(1.23)	(4 27)	(1.38)	(2 29)		(1.29)			
(8)......	−.003	1.18	3.08*	17 61	−.003	.		.003	.491	76
	(1 60)	(1.16)	(4.90)	(82)	(1.17)			(1.59)		

NOTE.—Variables defined in text; see text for coding of dummy variables; the dummy variables representing the main effects of DEV, LATIN, AFR, and ASIA were included in the estimation of the eqq. (2)–(8) but not presented here, numbers in parentheses are t-statistics; $\bar{R}^2$ is R^2 adjusted for loss of degrees of freedom.

* $P < .10$.

foreign capital penetration (FORCAP), is measured as the 1967 total book value as reported by OECD (1972), weighted by the square root of the product of energy consumption and total population.[12] The recent flow of foreign investment, referred to as foreign capital investment (FORINV), is measured as the difference between the total book value of the stock of direct foreign investment between 1975 and 1967, weighted by GNP. Capital formation (CAPFOR) is measured as a percentage of GNP and averaged over the time period. The log of GNP per capita in 1960 (GNP) is included in the equations to control for the prior correlation between wealth and foreign capital penetration.

Table 6 presents the regression estimates and t-statistics for this basic equation estimated separately for richer and poorer samples and for geographical regions. Equation (1) includes all 76 countries used in the analyses. This equation represents the previous finding from table 5 that showed that in large samples unrestricted by geographical region the stock of foreign investment has a negative effect on economic growth. This is shown by the regression coefficient of —0.004 for FORCAP, with a significant t-statistic of 3.76.[13] The FORINV has a positive coefficient of 1.46, with a nonsignificant t-statistic of 1.41. This finding of a negative effect of stocks and a positive effect of flows in the same equation is further evidence of the conclusion drawn earlier that stocks and flows have opposite effects on economic growth. The CAPFOR also has its expected positive effect, as does GNP. The results of this equation, then, are totally consistent with our previous conclusions drawn from comparing the different studies.

Equations (2) and (3) present the results for the richer and poorer countries in the sample to test whether the effects of foreign capital penetration differ within richer and poorer countries. The sample of 76 countries has been divided at the mean of GNP per capita in 1960.[14] The countries included in these samples and the subsequent regional subsamples are listed in Appendix A. In equation (2), FORCAP has a very strong negative effect ($b = -0.005$, $t = 4.26$), while FORINV continues to have a small positive effect. The CAPFOR is still significantly positive and GNP has

[12] This weighting of the value of the stock of foreign capital produces results which are the same as those produced by various other weightings, e.g., GNP. For theoretical reasons, we prefer the weighting used (see, e.g., Bornschier and Ballmer-Cao 1978)

[13] All data employed in the new analyses are listed in the Data-Annex of Bornschier and Ballmer-Cao (1978) except for the GNP data, which are from the *World Bank Atlas* (IBRD 1976). These data refer to GNP in constant market prices in U.S. dollars of the base period 1973–75.

[14] The relationship between foreign capital penetration and growth has been tested for a sample of the 15 most developed countries by Bornschier and Ballmer-Cao (1978). The effect is negative. But this effect drops to zero after controlling for capital formation and personal income inequality. However, foreign investment has not been controlled. So, definite conclusions are not possible for the most developed countries.

a small negative effect. In equation (3), the effect of FORCAP is also negative ($b = -0.003$, $t = 1.50$), but weaker than the effect in equation (2). These findings, then, provide support for hypothesis 3, that the effect of foreign capital penetration varies by level of development, with much stronger negative effects in richer than in poorer less developed countries.[15]

Equations (4), (5), and (6) present the results for the regional subsamples. Equation (4) shows that the estimated effect of FORCAP in Latin America is negative but statistically not significant ($b = -0.001$, $t = 0.63$), and the effect of FORINV is positive but small. Equation (5) shows a significant negative effect of FORCAP within Asia ($b = -0.007$, $t = 2.04$) and a small positive effect of FORINV. Equation (6) shows a negative but statistically not significant effect within Africa ($b = -0.0002$, $t = 0.116$) and a small positive effect of FORINV. The results of the analyses of separate regions, then, show negative effects of FORCAP and positive effects of FORINV, as was found in equations (1)–(3). But these results remain inconclusive because of the small size of the effects. The results are also inconclusive because of the findings in equations (2) and (3) that seem to indicate that negative effects differ in size for richer and poorer countries. In the separate regional samples, region is confounded with level of development, since Latin American countries tend to be richer and African countries tend to be poorer.

In order to overcome the problem of the small sample sizes in the separate regional analyses and the problem of the confounding of regional and level of development effects, we present in table 7 an analysis of all 76 countries in which we can simultaneously examine the interactions of FORCAP with level of development and with region.

Table 7 estimates the same basic model but also includes the interaction terms, each of which is constructed by multiplying four dummy variables by the value of FORCAP. The four dummy variables used are: DEV = 1 for the more developed countries (defined as those above the mean on GNP per capita in 1960), DEV = 0 for the less developed countries; LAT = 1 for Latin American countries, LAT = 0 for others; ASIA = 1 for Asian countries, ASIA = 0 for others; and AFR = 1 for African countries, AFR = 0 for others. Europe is the excluded region. The relevant main effects of the dummy variables were included in the estimation of the equations, but for ease of presentation they are not shown in the table.

[15] This result approximately replicates an earlier finding by Bornschier and Ballmer-Cao (1978), except that their subsample analysis was based on the absolute size of the modern sector (as measured by total energy consumption) rather than on the level of development which we use. They found that FORCAP had smaller negative effects in very small countries and larger ones in larger countries The two ways of dichotomizing overlap considerably, making it difficult to tell whether it is really size or level of development which interacts with dependence on foreign capital.

For purposes of comparison equation (1) presents again the results from the basic model without any interaction terms (see table 6, eq. [1]). Equation (2) adds the interaction term for level of development, FCDEV. The results of this equation show a significant interaction effect of foreign capital penetration with level of development. The coefficient of this term is -0.003, $t = 1.73$. This result supports the implications from the separate analyses of richer and poorer countries that the effect of foreign capital penetration is significantly more negative in the richer than the poorer group of countries.

Equations (3), (4), and (5) add the interaction terms for foreign capital penetration with the different regions. The interaction terms for Latin America and Asia, FCLAT and FCASIA, are not significant, implying that there are no interactions of foreign capital penetration with presence in Latin America or Asia. The interaction term for Africa, FCAFR, is significant and positive ($b = 0.004$, $t = 2.34$), implying a possible interaction of foreign capital penetration and location in Africa. In all these equations, the main effect of FORCAP remains negative and statistically significant.

Equations (6), (7), and (8) include both FCDEV and the interaction with the different regions to test simultaneously for the two types of interactions. These three equations show that when the two interaction terms are included in the same equation, there is a significant interaction only for Latin America. The interaction term for level of development continues to be significant, except in equation (8).

Surveying all of the results in tables 6 and 7, we conclude that there is a significant interaction of the effect of foreign capital penetration with level of development. While the effect of foreign capital is negative within both richer and poorer developing countries, it is significantly more negative in richer than in poorer countries. Hypothesis 3 is supported, and we conclude that most of the discrepant results found in table 5 stem from the fact that region and level of development are confounded in the studies of separate geographical areas. There are no interaction effects of capital penetration with presence in either Asia or Africa. The only regional interaction that remains when the interaction with level of development is controlled is that of Latin America. But that interaction must be interpreted with caution. Since the interaction with level of development is controlled in equation (6), the coefficient associated with the interaction term FCLAT shows that there is a positive effect of foreign capital penetration on economic development within the poorer countries of Latin America, with poorer defined as below the mean on GNP per capita in 1960. In fact, as the composition of the subsamples in Appendix A shows, there are only two countries within this category in the analysis, Bolivia and Haiti. Thus, while this finding is statistically significant, it is not of substantive im-

portance, as it results from a comparison of only two Latin American countries with the other countries in the sample. The overall effect of foreign capital penetration within Latin America is negative.

Table 8 presents the summary calculations for the effect of foreign capital penetration within the six categories of the analysis. Table 8 shows that the effects of foreign capital penetration are negative in all regions. The effect within Latin America is —0.003, the effect within Asia is —0.004, and the effect within Africa is —0.001. The table also shows the effect within the richer and poorer samples of each region. As we concluded above, the effect within the richer group of countries in each region is always more negative than the effect within the poorer group. The table also shows that the only non-zero positive effect occurs within the poorer countries of Latin America. As we noted above, this finding is not of substantive importance. We conclude, then, that there are no regional interactions. Hypotheses 1 and 2 are not confirmed. And we can also eliminate hypothesis 4, the hypothesis of limited variation due to small sample size, because the analyses in table 7 are done with a large sample of countries. We conclude, then, that the effects of foreign capital penetration are negative within all regional samples, and the only substantively important interaction effect is that foreign capital penetration is significantly more negative in richer than in poorer developing countries.

In these analyses, the book value of the stock of foreign direct investment was measured as of 1967, while the growth rates of GNP per capita were measured from 1960 to 1975. In order to make sure that the partial overlapping of the time measurements does not affect the results, we have replicated the analyses presented in tables 6 and 7, using growth rates of GNP product per capita from 1967 to 1975 and measuring the other independent variables as of 1967. The results, presented in Appendixes B and C, show that the same patterns emerge: the overall negative effect of foreign capital penetration, the positive effect of recent foreign investment, the much stronger negative effect of foreign capital penetration in the richer half of the sample, and the lack of any significant regional inter-

TABLE 8

REGRESSION ESTIMATES OF THE EFFECTS OF PRIVATE FOREIGN DIRECT INVESTMENT ON ECONOMIC GROWTH, 1960–1975, WITHIN LEVEL OF DEVELOPMENT AND REGIONAL CATEGORIES, AS COMPUTED FROM TABLE 7

	Latin America	Asia	Africa
All countries within region	−.003	−.004	−.001
Richer countries within region	−.002	−.007	−.003
Poorer countries within region	+.003	−.003	+.000

actions. The only differences are that the Latin American interaction (which we had previously concluded was not of substantive importance) does not appear and that the magnitude of the effects is smaller, as one would expect since the period covered is eight instead of 15 years.

How might this interaction with wealth be explained? McGowan and Smith (1978) suggest that many of the least developed countries in the world have economies which are predominantly outside the world economy. Within the stratum of such countries, some countries with a greater proportion of modern economic activities (e.g., the Ivory Coast) are being compared with others in which the traditional, nonmonetized sectors are predominant (e.g., Chad). Any kind of monetized exchange, whether due to foreign investment or not, will appear as economic growth in such countries. This hypothesis could be tested by constructing a measure of integration in the world economy and seeing its effects on growth, controlling for foreign investment. Some results presented by Bornschier and Ballmer-Cao (1978) suggest a different explanation. In one analysis, they study the effects of foreign investment on growth as they vary by economic sector. They find that foreign investment in manufacturing has the largest negative effects on growth, while foreign investment in extractive industries (mining and petroleum) has positive effects. One possibility, then, is that the composition of foreign investment by economic sector differs in the lowest stratum of countries, with more investment in extractive than in manufacturing sectors. This hypothesis could be tested by including the composition of foreign investment by sector into analyses.

DISCUSSION

Our review of the evidence has produced the following assessment of the empirical relationships with which we are concerned: (1) The effect of direct foreign investment and foreign aid has been to increase economic inequality within countries. This effect holds for income inequality, land inequality, and sectoral income inequality. (2) Flows of direct foreign investment and foreign aid have had a short-term effect of increasing the relative rate of economic growth of countries. (3) Stocks of direct foreign investment and foreign aid have had the cumulative effect of decreasing the relative rate of economic growth of countries. This effect is small in the short run (1-5 years) and gets larger in the long run (5–20 years). (4) This relationship, however, has been conditional on the level of development of countries. Foreign investment and aid have had negative effects in both richer and poorer developing countries, but the effect is stronger in the richer than in the poorer countries. (5) These relationships hold independent of geographical area.

Our review of these studies has also suggested some requirements of research design factors which should be included in future studies of these relationships: (1) Such studies must be longitudinal. This requirement arises because the short-term and long-term effects differ. We have found that short lags produce small or no effects, while longer lags produce negative effects. (2) These studies must control for the initial relationship, or correlation, between foreign investment and economic wealth or growth. This is because, for a variety of reasons, foreign investment or aid is often attracted to wealthier or faster growing countries. Unless a researcher controls for this effect, the results will confound the negative effect of foreign investment or aid on economic growth with a positive effect of economic growth on attracting investment or aid. (3) Studies should also control for the initial level of savings (gross domestic investment) in a country, since savings are a major cause of economic growth and may be an inducement for the inflow of foreign capital. (4) These studies should include measures of both the stock and flow of foreign investment, since they have opposite effects on economic growth.

The confirmation of a set of empirical relationships is only a first step in understanding the effects of foreign investment and aid. Studies are needed which try to explain these relationships by explicitly designing research to uncover the structural mechanisms by which these effects occur. Few of the studies reviewed here have explicitly attempted this task. There have been some attempts, and we review them briefly here. Both Chase-Dunn (1975*b*) and Szymanski (1976) find that foreign investment tends to produce uneven growth across economic sectors. Such uneven development may be one mechanism by which foreign investment leads to a lowered rate of growth. Bornschier and Ballmer-Cao (1978) find that foreign investment leads to increasing income inequality, early monopolization, and structural underemployment, thus favoring early saturation of effective demand and lowering the rate of capital formation in a country. And since capital formation is a major cause of increasing growth, this reduction in capital formation is another mechanism by which foreign investment reduces growth. Finally, Rubinson (1977) finds that one of the ways in which foreign investment reduces growth is by reducing state power, and hence the ability of the state to undertake a policy of growth, independent of the class interests created by foreign capital. All these studies, however, should be seen as initial attempts to specify the mechanisms by which foreign investment and aid affect growth and inequality. Much more explicit research is needed in this area.[16]

[16] Two new studies have been done which we unfortunately have not been able to incorporate into our review. Evans and Timberlake (1977) have shown that foreign capital penetration is related to a distorted occupational structure in which there are

These highly aggregated studies, however, can take us only so far in understanding these processes. Three types of studies more microsociological in nature are needed. First, dependency situations are not homogeneous, and there are considerable differences in the dynamics of situations such as export enclaves, national export economies, and dependent industrialization. Controlled comparisons of the effects of foreign investment and aid in these different situations should be very fruitful. Second, studies of individual countries and of the effects of foreign investment disaggregated by composition are necessary to clarify the specific mechanisms by which these processes operate. Third, direct studies of the operations of multinational corporations and of their relationships with the state are necessary. There has already been considerable research in these three areas, and an important task is to integrate the many disparate findings into a theoretically organized scheme.

Finally, we note that the empirical relationships we have found occurred during a specific time period, from 1950 to 1970. It is possible that these relationships are conditional on features of the world economy at that time. It seems possible that the effects of foreign investment and aid on growth and inequality may be conditional on whether the world economy is in a period of relative expansion or contraction. For example, Gobalet and Diamond (1977) find some evidence that the negative effects of foreign investment on economic growth are significantly greater from 1965 to 1975 than from 1955 to 1965. Since the earlier period was one of worldwide economic expansion and the later period has been one of worldwide relative economic contraction, their study suggests that foreign investment may have more negative effects in periods of economic contraction. More generally, we also note that no relationships of the type we have analyzed should be assumed to be invariant across quite different historical periods. An important theoretical task is to understand the general conditions under which all such relationships occur.

In conclusion, this paper has reviewed and summarized the findings about a particular set of relationships which are important for understanding the process of national development. But the discovery of empirical relationships, and of the mechanisms which produce those relationships, should not be considered an end in itself. Such tasks as these are necessary because they are aids to the building of a theory of development. The findings reviewed here should be used toward this end.

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disproportionate numbers of workers in tertiary occupations This, they argue, is one of the ways in which foreign capital penetration affects economic inequality in a country. Gobalet and Diamond (1977) have demonstrated an interaction between state power and foreign capital penetration. The negative effects of foreign capital on economic growth are larger in countries with a relatively weak state than in countries with stronger states.

APPENDIX A

Countries Used in the Analyses by Subsample

1. Richer Countries*

Africa	*Asia*	*Latin America*		*Europe*
Algeria	Iran	Argentina	Honduras	Greece
Ghana	Iraq	Brazil	Jamaica	Ireland
Ivory Coast	Japan	Chile	Mexico	Italy
Morocco	Malasia	Colombia	Nicaragua	Portugal
Senegal	Taiwan	Costa Rica	Panama	Spain
South Africa	Turkey	Dominican Republic	Paraguay	Yugoslavia
Southern Rhodesia		Ecuador	Peru	
Tunisia		El Salvador	Uruguay	
Zambia		Guatemala	Venezuela	

2. Poorer Countries†

Africa		Asia	Latin America
Burundi	Mauritania	Afghanistan	Bolivia
Cameroon	Niger	Burma	Haiti
Central African Republic	Nigeria	India	
Chad	Rwanda	Indonesia	
Dahomey (Benin)	Sierra Leone	Korea, Republic of	
Ethiopia	Somalia	Nepal	
Guinea	Sudan	Pakistan	
Kenya	Tanzania	Philippines	
Liberia	Togo	Papua New Guinea	
Madagascar	Uganda	Sri Lanka	
Malawi	Upper Volta	Thailand	
Mali	Zaire		

* Log GNP per capita 1960 > 2 43
† Log GNP per capita 1960 < 2.43

APPENDIX B

Regression Estimates of the Effects of the Stock of Private Foreign Direct Investment on Economic Growth, 1967–1975, for Different Samples

Eq.	Sample	FORCAP	FORINV	CAPFOR	GNP	$\bar{R}^2$	N
1) .	All developing countries	− 002	1 32	812	12 93	136	76
		(2.68)	(1 71)	(1.44)	(1 54)		
2) . .	Richer developing countries	− 003	.021	017	− 094	406	39
		(4 23)	(2.19)	(3 01)	(729)		
3) ..	Poorer developing countries	− 001	011	.007	− 007	.056	37
		(572)	(.656)	(.694)	(021)		
4) .	Latin America	− 002	1.56	.930	−7.83	119	20
		(1 21)	(2 02)	(767)	(438)		
5) .	Asia	−.005	3.46	− 784	56.23	443	17
		(2 37)	(1.77)	(.852)	(3.30)		
6)	Africa	.0001	.674	1 07	−8.90	.045	33
		(.046)	(.410)	(.883)	(.368)		

Note.—Variables defined in text.

APPENDIX C

REGRESSION ESTIMATES OF THE EFFECTS OF THE STOCK OF PRIVATE FOREIGN DIRECT INVESTMENT ON ECONOMIC GROWTH, 1967–1975, FOR 76 DEVELOPING COUNTRIES

Eq.	FORCAP	FORINV	CAPFOR	GNP	FCDEV	FCLAT	FCASIA	FCAFR	$\bar{R}^2$	N
(1)	– 002	013	009	.114					.143	76
	(2 67)	(1.63)	(1 70)	(1 36)						
(2)	– 001	012	.011	– 049	– 002				.153	76
	(97)	(1 52)	(1 84)	(329)	(1 41)					
(3)	– 002	013	008	131		0001			.120	76
	(2 38)	(1 63)	(1.40)	(1 38)		(.095)				
(4)	– 002	.013	009	118			– 002		.136	76
	(1 64)	(1 65)	(1 56)	(1 39)			(881)			
(5)	– 003	013	.009	093				.002	.139	76
	(2 68)	(1 61)	(1.69)	(936)				(1.26)		
(6)	– 001	012	008	– 054	003	.002			.143	76
	(845)	(1 51)	(1 36)	(360)	(1.66)	(.984)				
(7)	– 0001	.012	010	– 067	– 003		–.002		.159	76
	(045)	(1 54)	(1 71)	(.456)	(1 70)		(1.14)			
(8)	– 002	012	011	– 079	–.002			001	.141	76
	(1 00)	(1 48)	(1 88)	(490)	(1.02)			(788)		

NOTE —Variables defined in text

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Toward an Improved Explanation of Income Attainment: Recalibrating Education and Occupation[1]

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In past attempts at predicting income attainment, education and occupation have been recognized as the most intuitively appealing causal factors but have yielded especially dismal empirical results. This paper attempts to demonstrate that the relatively poor success at predicting income attainment with these variables can be corrected with straightforward recalibrations of their scales. A scale of occupations is drawn from classical notions of work hierarchies as well as from recent economic theory on the power of the employing firm. A scale of educational training targeted toward the new scale of occupations is offered as a logical corollary to the theory that stands behind the scale of occupations. The combined explanatory power of the new measures substantially increases the prediction of income attainment over past work in the area. Limitations of the measures and sample are discussed, and suggestions for future improvements are offered.

Sociological models of income attainment have been markedly inferior to those of educational and occupational attainment. Sociological models of all three attainment variables are generally considered to be an outgrowth of the Blau and Duncan (1967) model of "status transmission," which was expanded and developed by the Wisconsin group (Sewell, Haller, and Portes 1969; Sewell, Haller, and Ohlendorf 1970) through the addition of a range of social psychological variables. Thus there are not really three models, one for each attainment variable, but rather one model

[1] Data presented herein were taken from a study of status attainment of Lenawee County, Michigan, men, which is or has been supported by National Science Foundation grant GS29031 (1971-74), by contract with the United States Office of Education (1958-61), and by the University of Wisconsin—Madison and Michigan State University Agricultural Experiment Stations. The project is conducted under the general supervision of Archibald O Haller and David L Featherman, Rural Sociological Research Laboratory, 617 WARF Office Building, University of Wisconsin—Madison. I am especially indebted to Haller for his personal assistance in making these data available and to Alejandro Portes, S Dale McLemore, Norval D. Glenn, Anthony Orum, Joseph Lopreato, Susan Wilson, Robert Helmreich, Parker Frisbie, and Sheldon Olson for reading and rereading an earlier form of this paper. Their ideas and suggestions form an integral part of this paper, but they bear no responsibility for any errors that it may contain.

which attempts to explain these variables as three successive stages of the same process: A prior set of determinants leads to educational attainment which in turn leads to occupational attainment which in turn leads to income attainment. The model usually explains at least 40% of the variation in educational attainment, at least 50% of the variation in occupational attainment, but usually no more than 20% of the variation in income attainment.

The major problem with explanations of income attainment has been the surprising weakness of its two main causal factors, education and occupation, for supplying an adequate explanation of income. The purpose of this paper is to demonstrate that the weakness of these two factors is not inherent. Rather, their weakness is due to the scales currently used to calibrate them. I offer recalibrations of the scales for both education and occupation. The potential of recalibrations for predicting income can be seen in a blunt comparison of the coefficient of determination (R^2) obtained from the traditional scales with the one obtained from the new scales. Using the old scales, Jencks et al. (1972) report an R^2 of .190; Sewell and Hauser (1975) report one of .074. With the Lenawee sample used in this paper, and using the old scales,[2] I estimated an R^2 of .091. With the Lenawee sample and new measures for education and occupation, I estimated an R^2 of .462. The Jencks model is stronger than both the Sewell-Hauser model and the similar model which I estimated on the Lenawee sample, but this is probably due to age differences between the two samples.[3] The Jencks data cover an age range up to 64 while the other two samples are both under 32. The most striking difference is between the fourth figure, the R^2 computed with the new scales, and the other three results. The new scales produce a coefficient of determination that is more than twice as strong as the Jencks model and at least five times the strength of the other two figures.

A useful strategy for recalibrating occupations would be to find a better measure of prestige than the Duncan SEI (Reiss et al. 1961), which has been used so extensively in the past. The limitations of this index have been noted by Haug and Sussman (1971) and Duncan's own caveats (Reiss et al. 1961, p. 139). In this paper I opt for more radical surgery on the occupational scale. The scales for both education and occupation are oriented toward a concern with power. The occupational scale I propose has roots in the "classical" heritage of Weber and Marx but is actually based upon theoretical work outside of sociology, the work of econo-

[2] The independent variables are father's occupational prestige, measured ability, academic performance, educational attainment (measured as described below), and occupational prestige (measured with the Duncan SEI).

[3] Haller and Spenner (1977) have found significant increases in R^2s for older age cohorts.

mists Carl Kaysen, John Kenneth Galbraith, and Robert Averitt, among others. Recalibrations follow from the premise that occupations carry varying and measurable amounts of power to command high monetary rewards once their organizational context is specified.[4] As a corollary to this premise, it is believed that individuals will target their educational training so as to attain positions of power and wealth. Thus an extension of the "occupational power" hypothesis leads to a recalibration of educational training, scaled in terms of the advantages or lack of advantages it provides for the attainment of powerful occupational positions.

The alternative strategy, probing for better measures of prestige, would appear to be logically prior to an excursion into the entirely new concept of power ("new" for the status attainment area). I chose to focus on power, instead of prestige, for two reasons. First, there is considerable agreement that power is the more fundamental dimension of stratification (Lenski 1966; Mills 1956; Weber 1947). Second, the concern with power furnishes a new conceptual framework for recalibrating education, and a fresh appraisal of education is especially stressed in this paper. A purely technical description of how the scales of education and occupation are recalibrated is presented below, followed by a theoretical justification and an empirical analysis.

SCALING PROCEDURES

Occupational Power

The scales for both education and occupation were provided by selected informants from high-level positions in several sectors of society. Informants were drawn from state government, the AFL-CIO, the Texas Medical Association, law, education, banking and finance, real estate, Merrill Lynch, and Radian, a large business firm. At the time of the survey, all of the informants lived and worked, at least part of the time, in Austin, the capital of Texas. They were selected through a "snowball" system of personal introductions. The first informant, a banker from Dallas who also participated in the state government, provided names and introductions to others whom he considered to be highly placed individuals. Each informant in turn provided the names, and introductions, to other influential persons in the city, and so on, until 20 informants were obtained.

[4] There is an upper limit to how much income variance can be explained from occupational categories, no matter how they are ranked, which is probably not much higher than the highest correlations reported in the past between the Duncan SEI and income. The advantage of approaching the scaling of occupations from a different theoretical concept, power, is that it suggests where to look within occupational categories for an additional source of income variance, i.e., the organizational contexts within which occupations are found.

Each informant was asked to score a list of 89 census occupational categories, on a scale from 0 to 10, according to their relative amounts of authority within an organization.[5] If the informant needed further information, it was suggested that he/she try to visualize the occupation on an organizational chart of the hierarchy of command and to score the occupation on that basis. Blalock (1975) has argued that under such conditions there is no way to ensure that other considerations such as prestige, or even income itself, are not used by raters. An attempt was made to gain some control over the validity of the rankings by comparing them to a six-category authority index constructed by Pastore, Haller, and Gomez-Buendia (1975). The rankings provided by 13 of the original 20 respondents correlated higher than .50 with the Pastore et al. index, and they were selected to fill the final informant pool. The mean of the 13 informants' scores was taken as the authority score for each occupation.[6]

The power of an occupation was expected to vary across industrial contexts, so the authority index was combined with a measure of industrial power to yield the final measure of occupational power.[7] Industrial power was measured as a four-level classification of census industry categories. The classification is derived from theoretical work of economists—Galbraith (1971) and Averitt (1968) in particular. The top category included key industries and "social overhead capital" (Averitt 1968, chap. 3) such as railroads, airlines, automobile manufacturing and sales, and computer and office accounting machine manufacturing. This sector of the economy, referred to as the "industrial system" by Galbraith and as the "center economy" by Averitt, is thought to provide the greatest contrast in industrial power vis-à-vis other sectors. Nevertheless, the other sectors were further differentiated into three categories. Religion is placed in the bottom category; education in the next to the bottom category; and all other sectors, including government, military, legal, and medical sectors, are placed third from the bottom in the category just below the "center economy." A total of 78 census industry categories were represented in the present sample.

[5] Actually, rating the authority of census occupational titles is at best an indirect method of assigning authority levels to each respondent Census categories were not constructed with authority in mind and many ambiguities are present. A more straightforward method may be to locate each respondent on an organizational chart at the time of the interview. This procedure would be less ambiguous, but a method would have to be devised that would consistently and accurately chart jobs across a wide number of types of organizational hierarchies. One possible underlying dimension useful for relating authority levels in different hierarchies is Jacques's (1976, pp. 99–100) notion of the maximum time spans during which people are required to exercise discretion. This notion holds particular promise for empirically grounding authority levels in hierarchies.

[6] The mean of the interinformant correlations was 0.58.

[7] For a detailed discussion of occupational power, see Wilson (1978).

The best combination of authority and industrial power was determined empirically from regressions of income on the two combined as additive and multiplicative indices. The multiplicative index explained a significantly greater portion of the income variance than the additive index. Furthermore, the two indices entered separately did not add significantly to the income variance already explained by the multiplicative index. Intuitively, a multiplicative combination makes sense since it creates greater power fluctuations at the upper ends of the scale. Thus the power of a manager will vary more across industrial contexts (such as between an automobile manufacturer and a local retail store) than the power of a clerk.

Targeted Education

The 13 informants ranked a number of curricula found in vocational/technical institutes, apprenticeships, and the military, as well as colleges and universities, according to their relative probabilities for "feeding into" high-level occupations (the ones they previously ranked as high). The scale for education resulted from attention to curricula of training rather than years of training. The curricula categories, ranked by the informants, were grouped into the following four categories: The top college category received a mean score of 5.83 (scores were assigned from 0 to 10), and included such categories as engineering, medicine, and business and commerce. The top curriculum category of training received elsewhere (vocational/technical institute, apprenticeship training, and the military) was scored 6.39 and included such curricula as general management, administration, and banking. The second-level college category was scored 4.21 and included architecture and law, among others. The second category of training received elsewhere was scored 4.51 and included business, accounting, design, and electronics. The third categories for both received the respective scores of 3.79 and 2.64; the former included agriculture and social science and the latter, data processing and manufacturing. The fourth category for both received the respective scores of 2.45 and 2.18; the former included literature and religion and the latter, services and clerical training. For ease of handling, the four levels of education, in all types of educational institutions, were scored simply from 4 to 1. In a few cases the respondent received education from college and also from one of the other alternatives. For these cases the respondent was assigned the college score on the justification that college, being the oldest higher-education alternative, is better articulated with labor markets. If the respondent did not have training from any of these types of institutions, he was given a score of 0. The final measure, scored from 0 to 4, is referred to as "targeted education."

The concept of educational training targeted toward positions of power and wealth follows from the hypothesis that occupational positions receive monetary rewards in direct correspondence to their economic power. The difference between this concept of education and conceptualizing education as the number of years received is that targeted education stresses the curriculum of training over and above the amount of training.[8] This concept orients us to the teleological aspects of education where individuals seek training in certain areas as opposed to others in accordance with specific occupational and economic goals.

Reed and Miller (1970) and Havemann and West (1952) both studied the effects of college curricula on income. Their results, not especially remarkable, are nevertheless encouraging. Reed and Miller, using 10 curricula categories, explained 4% of the variance in college graduates' incomes and 7.2% of the variance in incomes for those with professional degrees. Neither of these results is negligible, and considerable room is open for further returns with more detailed curricula specifications. The 10 curricula categories they employed, such as health and humanities, may have concealed significant income variations occurring within them.

The Lenawee sample I use in the following analyses offers several possibilities for improvement. First, the data include curricula designations for other types of higher-education alternatives in addition to college curricula. Second, the college curricula are broken down into 67 categories, and the other educational alternatives are broken down into 59 curricula categories. Third, the curricula categories are combined and ranked from a concept of their potential for occupational power rather than according to generic education administration categories.

METHOD

Sample

Data used in this analysis are supplied by the University of Wisconsin Lenawee County Project. These data are the results of an intense survey with relatively few cases (244 after useless cases are excluded).[9] As described above, this sample, with its detailed description of respondents' educational histories, is unique. This feature was achieved while sacrificing sample coverage, but this data deficiency is offset with a secondary analysis of nationwide census data reported below.

[8] This same distinction applies to the difference between the present conceptualizatior of education and the Human Capital tradition in economics. The Human Capital tra dition analyzes training in terms of the amount of money and time (capital invest ments) spent in obtaining it rather than dealing with the content of the training.

[9] Cases without income data were excluded from the analysis along with cases tha reported more than one occupation

The Lenawee County Project was designed as a cohort study with two interview waves; the first wave was taken from 17-year-old high school students in 1957, the second returned to this initial population 15 years later, in the spring of 1972. The sample decreased from an initial figure of 428 respondents to a final figure of 340—a follow-up response rate of almost 79%. Analyses of possible nonresponse bias were conducted by the original researchers. Their results were essentially negative; response bias is not a significant concern. However, it should be kept in mind that the further reduction in sample size down to the 244 used in this analysis could cause possible distortions. Haller and Spenner (1977) performed a comparative analysis of the 300 cases they used with comparable census data and again found no significant distortions.

Variables

Income attainment is measured as the natural log of yearly income for the income reported by the respondent in 1972.[10]

Union membership is measured as the individual's membership or nonmembership in a labor organization (assigned 1 and 0, respectively).

Occupational prestige is measured by Duncan's socioeconomic index (see Reiss et al. 1961) and was assigned to the occupation category of the respondent in 1972.

Occupational power and *targeted education* are measured as described above.

Educational attainment is measured as a seven-category scale, proceeding from lowest to highest: no post–high school education, finish vocational/technical school or some college, bachelor's degree, some graduate school but no master's, master's degree, post–master's (LLB) degree, Ph.D., or M.D.

Academic performance is the average grade received in academic courses during the 1955–56 school year.

Mental ability is measured as the scores (recorded in 1957) on the Henmon-Nelson (1942) test of mental ability.

Father's occupational prestige is measured by Duncan's socioeconomic index and was assigned to the occupational category of the respondent's father.

Father's occupational authority is measured as the respondent's indication of the occupational classification of his father. The classification is ranked according to the authority of the occupation. In cases where the informant's evaluations were not obtainable, values from the Pastore et al

[10] The natural log of income is a conventional transformation used to correct for the fact that most incomes cluster at the lower end of the income distribution while those at the upper end spread out.

(1975) index were used. The index proposed by Pastore et al. covered almost identical ranges so that minimal distortion of the informant's scale should result with inclusion of this index. There was not sufficient information on father's industry of employment to provide a classification of enough of the respondents' fathers for an analysis, and hence only the authority index was used.

RESULTS

The zero-order correlations, means, and standard deviations among variables are presented in table 1. Tests for linearity revealed the presence of nonlinear combinations of variables or interactions. A detailed consideration of the interaction effects is not considered central to the present analysis, where the purpose is to assess the contributions of recalibrated versions of education and occupation to the income equation. Nevertheless, the interaction effects appear to clarify and reinforce results with father's occupational authority, and therefore these results will be discussed together.

An in-depth probe of the causal sequences leading up to income attainment is purposefully excluded from the analysis Many of the conclusions about this process have either been made earlier (Haller and Portes 1973) or are evident in the correlation matrix (see table 1). The results that deserve elaboration are divided into two parts. In the first part we are interested in scale comparisons between the new and old scales of education and occupation, in the unique causal processes that occur in this model in contrast with past models without these scales, and in possible evidence for validating these results beyond the Lenawee sample. In the second part we turn to the effects of the father on income as a main effect and as a joint effect with other variables in the model.

Scale Comparisons and Validations

The main effects of the just-identified model are presented in table 2, and those same effects, with trivial coefficients deleted, are presented in table 3.[11] The recalibrated versions of education and occupation command the most dominant influences in the income attainment process. Their conventional counterparts, on the other hand, are irrelevant for the income attainment process. Occupational prestige (as measured by the Duncan SEI), the counterpart to occupational power, has no effect whatsoever on income attainment. The zero-order correlation between occupational prestige and income attainment is significant, but the relationship is due to occupa-

[11] For the adjusted R^2s, subtract .03 from the given R^2 values.

TABLE 1

ZERO-ORDER CORRELATIONS AMONG VARIABLES

Variables	FOccAuth	FOccPre	MA	AP	TargEd	EdAtt	OccPow	OccPre	Union	Income
FOccPre..	.495									
MA ..	.131	300								
AP.....	131	219	486							
TargEd .	075	220	.430	511						
EdAtt. ..	219	.304	.418	594	.380					
OccPow.	.112	134	326	359	482	197				
OccPre	.249	371	.422	594	582	.582	607			
Union	−.243	− 153	−.381	− 337	−.376	− 279	− 344	−.484		
Income....	225	172	155	155	427	190	.372	.289	−.051	
X ..	336	33 2	92 7	201	2 21	1 03	575	49.1	.323	9.37
SD...	131	21.6	19 9	84.1	1 45	1.61	240	25.3	.469	.492

NOTE.—FOccAuth = Father's occupational authority; FOccPre = father's occupational prestige; MA = mental ability; AP = academic performance; TargEd = targeted education; EdAtt = educational attainment, OccPow = occupational power; OccPre = occupational prestige; Union = union membership; Income = income attainment.

TABLE 2

PATH COEFFICIENTS FOR MODELS OF EDUCATION, OCCUPATION, AND INCOME

DEPENDENT VARIABLES	INDEPENDENT VARIABLES									
	FOccAuth	FOccPre	MA	AP	TargEd	EdAtt	OccPow	OccPre	Union	R^2
TargEd..	−.051	094	.218***	.319***						.311
OccPow.	085	−.034	.125	.159*	389***	−.106				.269
Income	232***	− 021	−.019	− 147	427***	.108	.280***	−.045	.211**	.306

NOTE —See table 1 for abbreviations.
* $P \leq .05$
** $P \leq .01$
*** $P \leq 001$

TABLE 3

PATH COEFFICIENTS FOR MODELS OF EDUCATION, OCCUPATION, AND INCOME, TRIVIAL COEFFICIENTS DELETED

Dependent Variables	Independent Variables: FOccAuth	MA	AP	TargEd	OccPow	Union	R^2
TargEd...		.237	.396				.304
OccPow...			.152	.404			.250
Income...	.225			.380	.242	.230	.291

NOTE.—See table 1 for abbreviations.

tional prestige's association with occupational power. Educational attainment, the counterpart to targeted education, has stronger effects on income than on occupational prestige, but these are still not strong enough to approach significance.

The effects of occupational power and union membership arise from opposite ends of the organizational hierarchy of command. Their correlation, in table 1, is —.344, which means that individuals with reduced occupational power are more likely than those with high occupational power to seek union membership to enhance their incomes.

Targeted education is the sole predictor of occupational power, which is not surprising since the prior concept was calibrated with the latter one in mind. The antecedents of targeted education are the traditional antecedents of attainment to high levels of educational training, mental ability, and academic performance. Both of these variables exert quite strong influences on targeted education. However, in contrast to models of educational attainment, the father exerts no direct effects on targeted education.

The effects of targeted education are not limited to the determination of occupational power. Targeted education exerts direct effects on income attainment beyond its effects on occupational power, and the size of this effect, .380, places this variable as the key variable in the income attainment process (although the effects of the father, discussed in the next section, are quite strong).

The attempts to validate these findings are based on direct evidence for occupational power and on indirect evidence for targeted education. The measure of occupational power was tested by Wilson (1978) on the 1970 census 1 in 1,000 5% statewide samples for all states in the United States. Among those records representing the total United States population, white male heads of households, aged 30–34, were chosen for analysis (N = 4,348). In addition, only those occupation and industry categories found in the Lenawee data (there were, respectively, 89 and 78 categories) were included in the measure of occupational power, yielding a final N of 1,207.

Thus the present results do not generalize beyond these categories. The natural log of 1969 earnings was regressed on occupational power, occupational prestige (as measured above), and education measured in years. With all three variables in the equation, occupational prestige exerted no direct effect whatsoever on income net of educational attainment and occupational power. Both educational attainment and occupational power exerted significant effects on income, with occupational power being the stronger of the two. The regression coefficients and path coefficients for occupational power and prestige were both nearly the same across the two samples. These results provide evidence for the generalizability of present findings with occupational power for other samples but limit them to the occupation and industry categories tested.

The possibility of validating targeted education with direct intersample comparisons is vitiated at present by the uniqueness of the Lenawee sample's curricula designations. Some evidence for validation can be gained by disaggregating this scale into college curricula and other curricula. Both college and noncollege curricula explain about 3% of the variance in income attainment net of occupational power. These results appear reasonable given past findings with college curricula and the present modifications. Reed and Miller (1970) explain from 4% to 7% of the variation in income with college curricula, both without other controls. Thus the present results with college curricula net of occupation power, 3%, are probably stronger than Reed and Miller's results, as should be expected with the larger number of curricula categories in use in the present results. The additional 3% explained in income attainment from noncollege curricula was left unanalyzed by Reed and Miller. The implications of these results are that if Reed and Miller had used an expanded curriculum list they also would have explained more income variance with college curricula If they had included noncollege curricula (other than high school) they may have explained still more of the income variance.

Influences of the Father

The father plays an especially dominant role in the income process in this sample. Direct effects on income from father's occupational authority indicate that fathers exert direct influences on sons' incomes even after the education years. Other effects of the father are revealed by the interaction effects presented in table 4. All of them appear to be a logical extension of the main effects of the father on income. The interactions involve either father's authority, father's prestige, or both. All of the interactions, therefore, stress the ability of fathers to raise their sons' income levels. The information that these interactions add is that if the son has advantages of his own, the father is most effective in providing help. The

TABLE 4

VARIANCE EXPLAINED IN INCOME ATTAINMENT BY OCCUPATIONAL POWER, UNION MEMBERSHIP, TARGETED EDUCATION, FATHER'S OCCUPATIONAL AUTHORITY, AND ALL SIGNIFICANT INTERACTIONS

Independent Variables	R^2	F
Four variables: OccPow, Union, TargEd, FOccAuth	.291	
Four variables +FA×FP, FA×SP, FA×MA, FP×MA.... .	.462	184*

NOTE.—See table 1 for abbreviations; also, FA = father's authority, FP = father's prestige, SP = son's prestige.
* $P < .001$.

higher the son's mental ability, and the higher his occupational prestige, the more likely will the father be able to influence the son's income. The inclusion of all four interaction terms raises the explanation of respondent's income by 17%.

The precise nature of the effects of the father cannot be determined from the present data. The question arises whether these effects are due to a direct intervention by the father or to more indirect influences such as through special encouragements, decision consultation, or modeling. This is a question of considerable importance since the prior possibility, that of a direct intervention by the father, violates the common image of our meritocratic society. We can see from the strength of the effects of the father, both singly and jointly with the other variables, that if an intervening process is to be used to explain these effects it would have to be especially strong, around the order of one-fourth of the total variance in income, in order to explain all of the effects of the father. Given past results with income data it is unlikely that an intervening process with such magnitude will be found, and a direct intervention of the father appears to be the most reasonable explanation. Nevertheless, such conclusions must await more appropriate data.

CONCLUSIONS

The major contribution of this paper is in demonstrating that education and occupation categories are effective predictors of individuals' income attainment. Recalibrated versions of these two causal factors operate with substantial force on income, raising the overall explanation to a respectable 46% of the variation in respondents' income levels. The variables are the same, but the recalibration requires a considerable theoretical adjustment. In the new scale for occupations, the notion of prestige is set aside in favor of power. Thus we move from a notion of stratification emphasizing prestige, which has functionalist support, to a notion of stratification emphasizing power, which has definite classical roots in Marx and

Weber. However, the classical emphasis on ownership of the means of production is superseded in the present measure of power by more recent economic theory from Kaysen (1959), Galbraith (1971), and Averitt (1968).

The recalibration of educational training is formulated as a logical corollary to the new scale for occupations. Given a ranking of occupations in terms of economic power, individuals target their education to attain specific levels in the power hierarchy.

The recalibrated measures of education and occupation have room for improvement, but the strength with which they nevertheless operate to predict income levels is compelling evidence that the theoretical adjustments that stand behind them are sound. Better measures, larger samples, and more detailed analyses in the future should clarify more effectively the usefulness of these notions in particular and the image of the status attainment process in general.

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Desegregation Rulings and Public Attitude Changes: White Resistance or Resignation?[1]

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Two opposing theoretical arguments about the effect of court rulings on public attitudes are developed and tested using data collected before and after a desegregation ruling in Milwaukee. The social-adjustment argument is supported quite strongly. Parents of children in the public schools became more supportive of integration and busing on several different measures. On the other hand, people without children in the schools became more resistant, and parents of children in parochial schools showed the most negative attitude change. The proximity-resistance argument predicts that more resistance will occur in specific situations than in general situations and that most resistance will occur among those most affected by the decision. No support was found for this argument. The implications of the results are discussed briefly.

What effect does a court ruling which orders a city to desegregate its schools have on the attitudes and feelings of residents of the city? Are attitudes about racial issues hardened or eased? Is there increased resistance or resigned acceptance?

Two alternative reactions to a court desegregation order might be predicted from social theory and research. On the one hand, considerable research shows that greater prejudice and resistance to integration emerge as situations become increasingly specific, concrete, intimate, and proximate (see, e.g., Kramer 1950, and Westie 1965). Moreover, many who "favor" integration as an abstract principle are nevertheless opposed to busing as a means of achieving it. These studies suggest that whites will become more resistant to integration after an important desegregation decision because imminent racial contact has become significantly more likely. According to this view, one would also expect whites to resist and change more on specific items such as the desirability of busing as a means to achieve racial balance than on more general items such as the desirability

[1] The following made substantial contributions to the preparation of this paper, and I extend grateful appreciation to them. The *Milwaukee Journal* collected and made the data available. Donald L. Noel helped me clarify and formulate the research, and he and two anonymous reviewers made helpful comments on an earlier draft. An earlier version of this paper was read at the Southern Sociological Society meetings in New Orleans, spring, 1978.

of integration. This argument also predicts that parents with children in the public schools are more likely to change than either those who do not have children in the schools or those who have children in parochial schools, since parents of public school children are the only ones directly affected by the court order. We shall call this theoretical orientation the proximity-resistance argument.

At the same time another body of research literature suggests that at least some people will respond positively or show resigned acceptance of a desegregation order. Numerous studies suggest that attitudes conform to an accomplished fact. (For a more thorough review of this literature, see Pettigrew [1971, chap. 11].) Hyman and Sheatsley (1964) have shown that public acceptance of integration accompanied integration of the public schools in the South. The continuance of this trend is indicated as late as February 1976, with the Gallup Poll reporting that only 15% of white southern parents objected to sending their children to a school where a few of the children were black, a decrease from 61% in 1963 (Gallup Opinion Index 1976, p. 9).

In more specific research on the law and attitude changes, Colombotos (1969) found that the percentage of physicians in New York State who favored the Medicare hospitalization program for the elderly jumped from 38 before the law was passed to 70 in the 10 months after it was passed (but still before it had been implemented). Muir (1967) also found that the Supreme Court decision banning prayer in public classrooms resulted in general attitudinal compliance by officials and teachers in one public school system, though the changes were facilitated or impeded by various other groups to which they belonged. Thus it is clear that at least some legal rulings can affect attitudes before the law is actually implemented. We shall refer to this attitude-conformity literature and its theoretical orientation as the social-adjustment argument.

The social-adjustment argument also predicts a "negative" reaction to integration on the part of parents whose children attend parochial schools. Whether intentionally or not, these parents have made a decision that has exempted their children from the integration order and guaranteed their attendance in predominantly white schools. Thus negative reactions to an integration order would help justify their children's attendance at a parochial school.

METHOD

The data reported here are from two telephone surveys conducted by the *Milwaukee Journal* research staff for a series of articles on a desegregation case against the Milwaukee Public Schools. The first survey was conducted in May and early June of 1975. The case had been heard by the federal

court more than a year earlier. On January 19, 1976, the judge ruled tha the school system was illegally segregated and appointed a special maste to oversee the formation and implementation of a desegregation plan Busing was not mentioned in the decision though it was not ruled out The ruling involved only the City of Milwaukee; the suburbs were no affected. The second survey was begun one week after the ruling (anc some eight months after the earlier survey) and, though shorter than the first, included several items from the earlier survey.

The samples were obtained through random digit dialing. This procedure eliminates most of the biases usually present in telephone surveys, including unlisted numbers (8% in Milwaukee), unpublished numbers (an additional 4%), and out-of-date directory numbers. Professional interviewers conducted the interviews. The refusal rate was a low 7%, and complete data were obtained for almost all respondents (only 7% of the respondents refused to answer such sensitive questions as personal income).

The first survey elicited complete response data from 563 white and 122 black city residents. The second, smaller survey obtained complete data from 317 whites and 65 blacks. Owing to the small number of blacks in the samples the analysis focuses only on the changes in white attitudes. A comparison of the two samples with each other and with known census characteristics was conducted and presented by the *Milwaukee Journal* (Sunday, March 7, 1976, p. 6.). The characteristics of the sample matched closely those found in the special city census taken in 1975.

The substantive items from the surveys included both general race relations questions and specific items about integration and busing (see table 1). Answers allowed for items 1, 2, and 5 were: yes, don't know, and no. Item 3 could be answered: a lot, a little, or not at all. Item 4 was scored on a four-point scale: objection to some, half, or more than half minority students, or no objection. The last two specific items on busing were scored on a five-point scale from strongly approve to strongly disapprove

It should be noted that the predictions are not concerned with differences between the three groups per se but rather with the direction and differences in the amount of change in each of the three groups. To assess these differences the interaction component of an analysis of variance using an unweighted means solution was examined. A separate analysis of variance was conducted for each item using the full range of responses. (The table reports grouped data for clarity.) Since the analysis of variance does not tell which groups changed, a further test, Duncan's multiple-range test—an a posteriori test from the analysis of variance—was conducted for the appropriate comparisons (not all possible comparisons are of interest). Note, however, that the test of the two theoretical orientations is based on the direction of the results as well as the statistical significance of the comparisons.

RESULTS

The direction of the results for the general item of desirability of integration as a goal (1) provides weak support for the social-adjustment argument (see table 1). The interaction between time and groups was not statistically significant, however, nor were the individual tests for the two groups of primary interest, parents with children in public schools and parents with children in parochial schools. On the somewhat peripheral items of perceived discrimination (2) and neighborhood integration (3) the results were mixed. The smaller percentage of parents with children in the public schools who say there is discrimination supports the proximity-resistance argument, but the even larger negative reaction by parents of children in parochial schools lends support to the social-adjustment theory. The interaction effect was also significant. The fact that all three groups became more negative may reflect changed attitudes about affirmative action programs more than changes in attitudes about discrimination. Likewise, the decrease in item 3 may reflect larger attitudinal changes in society. Neither item 2 nor 3 provides strong support for either theoretical argument.

The results on the specific school issues are more clear-cut (items 4 through 7). The interaction is significant on three of the four items. Furthermore, on each of the four items the changes for the parents of students in the public schools are all in the direction that supports the social-adjustment argument. Fewer parents of public school children object to their children attending school with minority students, more approve of busing from other neighborhoods into their own, and slightly more approve of busing from their neighborhood to other neighborhoods. The changes on some items are quite small, but the differences in the direction of attitudinal change by the three different groups as well as the magnitude of the changes are of theoretical interest. Thus it is important that parents with children in the public schools, on each item, became more supportive of integration and busing, while the others either showed no change or became less supportive.

The results from parents of the parochial school students on three of the four items also support the social-adjustment theory. There was no change on item 5, but on the other three specific school items these parents exhibited strong negative change.[2] A much larger percentage of these par-

[2] Alternatively, these changes may reflect changes in the enrollment patterns of public and parochial schools. However, two factors argue against such an interpretation of the results First, the second survey was completed soon after the ruling (within two weeks) Thus parents did not have time to withdraw their students from the public schools and enroll them in parochial schools before they were interviewed. Second, the enrollment patterns in the Milwaukee Public School system show no dramatic change in the years preceding the decision. Like most urban public school systems, Milwaukee had experienced declining enrollments for a number of years, but the patterns show

TABLE 1

ATTITUDINAL CHANGES OF WHITE PARENTS (by Presence or Absence of Children) TO DESEGREGATION RULING

Survey Items	Percentages May 1975	January 1976	Change	Theoretical Orientation Supported
	General Racial Issues			
1. In general, do you believe that racial integration of the schools is a desirable goal? (Yes):				
Children in public schools[a]	43.3	46.4	+3.1	Social adjustment (weak)
Without children in schools[b]	49.6	40.1	−9.5**	. .
Children in parochial schools[c]	48.3	40.6	−7.7	Social adjustment (weak)
Total	47.9	41.5	−6.4	. . .
2. In general, do you think that minorities are discriminated against in obtaining jobs? (Yes).*				
Children in public schools	31.9	20.3	−11.6**	Proximity-resistance
Without children in schools	28.3	25.1	−3.2	.
Children in parochial schools	39.3	9.1	−30.2**	Social adjustment
Total	30.4	22.4	−8.0	.
3. If a family of another race with about the same income and education as you moved next door, would you mind it a lot, a little or not at all? (A little or a lot)				
Children in public schools	24.4	11.5	−13.1**	Social adjustment
Without children in schools	21.2	15.8	−5.4	.
Children in parochial schools	26.7	18.2	−8.5	Neither
Total	22.6	15.1	−7.5	. .

[a] N = 142 in the first survey, 69 in the second survey

[b] N = 360 in the first survey, 215 in the second survey

[c] N = 61 in the first survey, 33 in the second survey.

* $P < .05$ for interaction of group and time (analysis of variance).

** $P < .05$ based on Duncan's multiple-range test, an a posteriori contrast test from the analysis of variance. Note, however, that the test of the theories is based on direction of the results, as well as the statistical significance of these individual tests. Further, as one reviewer rightfully noted, the analysis of variance tests were not completely independent and thus the experiment-wide level of significance is something higher than the .05 reported for each individual test.

TABLE 1 (*Continued*)

Survey Items	Percentages: May 1975	Percentages: January 1976	Percentages: Change	Theoretical Orientation Supported
		Specific School Issues		
4. Do you or would you object to your children attending a school where *some* (*half, more than half*) of the students are of another race? (Some or half):*				
Children in public schools	34.6	26.5	−8.1	Social adjustment
Without children in schools	Not asked of parents without children in school			
Children in parochial schools	28.3	45.5	+17.2**	.
Total	32.7	32.6	−0.1	Social adjustment
5. Would you approve or disapprove of busing children to a school outside their neighborhood to achieve racial integration in schools? (Approve):				
Children in public schools	22.0	29.0	+7.0	Social adjustment
Without children in schools	22.8	23.0	+0.2	.
Children in parochial schools	16.9	18.2	+1.3	Neither
Total	22.0	23.8	+1.8	
6. How do you feel about busing of children *from other neighborhoods into your neighborhood* so that schools will be racially integrated? (strongly approve to strongly disapprove). (Approve or strongly approve):*				
Children in public schools	21.9	27.5	+5.6	Social adjustment
Without children in schools	20.8	18.6	−2.2	. .
Children in parochial schools	22.0	9.1	−12.9**	Social adjustment
Total	21.2	19.6	−1.6	. . .
7. How do you feel about busing children *from your neighborhood to schools in other neighborhoods* to achieve racial integration? (Approve or strongly approve):*				
Children in public schools	12.0	14.4	+2.4	Social adjustment
Without children in schools	18.3	16.3	−2.0	. . .
Children in parochial schools	14.8	3.0	−11.8**	Social adjustment
Total	16.3	14.5	−1.8	. .

ents would object to their child being in a school where some or half of the students were minority students, and fewer said they would approve of busing either into or from their neighborhood. The change on these last two items was even more dramatic among the parents of the parochial students who strongly disapproved. The percentage of parents who strongly disapproved of busing into their neighborhood increased from 45.8 to 75.8, while on the bus-out question the percentage rose from 52.5 to 81.8.

We had predicted that there would be more change on the specific items than on the more general items. A close examination of the changes reported in table 1 reveals that this did not occur, except for parents of parochial school students. There is, however, much more consistency on the specific items than on the general ones. In fact, if one considers only items 1, 4, 5, 6, and 7, which deal specifically with school issues, there is strong support for the social-adjustment theoretical orientation. The expected differences in direction appear in all five cases for the parents of students in the public schools and are present in four of the five cases for the parochial school students (the binomial probability of obtaining results supportive of one theory nine times out of 10 is .02).

It is important to note that the positive changes among parents with students in the public schools occurred during a time when some national leaders and social scientists questioned whether busing, at least in large cities, is an appropriate means of achieving integration. Furthermore, these changes occurred despite negative attitudinal changes in the total community (see totals for the items in table 1).

IMPLICATIONS

The results have important implications. A sizable literature has emphasized negative reactions to court-ordered integration and busing. However, in this case the judge's ruling resulted in favorable attitude changes among those most affected, parents of the students in the public schools. Furthermore, though busing was not mentioned by the judge, the favorable attitude changes included attitudes toward busing, not just integration.

The history of integration in the United States certainly shows that most Americans will accept integration—although the stance taken by the community leaders is a critical variable affecting community response to integration. In Milwaukee most leaders, including the mayor and city council, were generally supportive of the desegregation ruling. The majority of the school board members, the defendants, were not. Under these conditions

no change that can be attributed to anticipation of the ruling. In fact, the decline from 1974–75 to 1975–76 (the year the case was heard) appeared to be leveling off from the previous declines (the enrollment figures were as follows: 1972–73—128,488; 1973–74—123,452; 1974–75—118,584; and 1975–76—115,708).

we have found some acceptance of the desegregation ruling. The positive changes by the parents of the public school children are perhaps small, but they loom large considering what caused them (and considering how deeply entrenched racial prejudice is in American society). There were no increased intergroup contacts, no changes in school feeder patterns, no increased neighborhood integration. There was only the ruling. Like several earlier investigators of law and attitudes we have found that the law can change public opinion. Other perhaps larger changes may occur when the integration is undertaken, depending, of course, on the way in which it is implemented. Nevertheless, the relationship between attitudes and law, especially the conditions under which law results in attitudinal compliance, needs further investigation.

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The Location of Change in the Sexual Structure of Occupations, 1950–1970: Insights from Labor Market Segmentation Theory[1]

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Empirical analyses of inequality trends indicate that gender differentials have remained highly stable in the United States since World War II. We challenge such conclusions of extreme stability with respect to the sexual structure of occupations. The primary focus is on shifts in gender composition, because different occupational distributions of men and women are now regarded as the major source of differential work rewards. We demonstrate that conventional measures of shifts in occupational sexual structure ignore the location and direction of change and that substantively important variations have occurred since 1950. But our primary objective is to explain those changes. After indicating that other perspectives fail to account for observed patterns of sex composition change, we suggest complementarities between economists' labor market segmentation approaches and occupational sex segregation. Although our empirical representation of market segments is heuristic, results of analyses of U.S. census data from 1950 to 1970 are largely consistent with a dual labor market interpretation of changes in occupational sexual structure. We conclude with brief mention of substantive and policy implications of these findings.

Recent empirical research on trends in gender inequality consistently points toward stability since World War II. For example, Featherman and Hauser (1976) analyze data on men and women in the 1962 and 1973 Occupational Changes in a Generation (OCG) surveys and find for each year a virtually identical (85%) proportion of the gender earnings gap that cannot be accounted for by specified explanatory variables. Aggregate investigations are characterized by similar results, for instance, the absence of significant shifts in levels of occupational sex segregation (Treiman and Terrell 1975)

[1] This is a shortened version of an unpublished manuscript dealing with similar issues Readers interested in elaboration and further detail are encouraged to write for the longer paper (cited here as Snyder, Hayward, and Hudis 1978). Portions of this material are based upon work supported by the National Science Foundation under grant SOC77-01062 and the National Institute of Mental Health under grant MH 30136-01 The authors are grateful to Larry J. Griffin, Lowell L. Hargens, Arne L. Kalleberg, Barbara F. Reskin, and anonymous referees for valuable comments but retain full responsibility for all views expressed here.

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or parameters that represent processes underlying distributions of women and income levels across occupations (Snyder and Hudis 1976).

We reexamine postwar gender differentials and for several reasons focus especially on changes in the sexual structure of occupations. It is now generally recognized that achievements occur in the context of "opportunity structures," which represent the distribution and characteristics of positions in the marketplace (Sørenson 1975; Stolzenberg 1975). The limits of solely individual level models are underscored by the Featherman-Hauser findings and their assessment that "sexual inequalities of achievement . . . stem mainly from the processes which generate sex-specific occupation distributions" (1976, p. 463). We consider these to be largely aggregate processes that require occupational level study before they can be linked to gender achievement differentials and changes therein (see Stolzenberg 1975). Moreover, many analyses (Bergmann and Adelman 1973; Ferber and Lowry 1976; Hodge and Hodge 1965) identify female concentration per se as the major occupational level influence on earnings differences between men and women.[2] Despite the importance of compositional shifts and their sources, little quantitative work addresses them. Our treatment first demonstrates that significant changes have in fact occurred since 1950 and then brings economists' labor market segmentation perspectives to bear on them.

MEASURING STABILITY AND CHANGE IN THE SEXUAL STRUCTURE OF OCCUPATIONS

Conclusions concerning sex composition changes are based on (*a*) temporal comparisons of indices of dissimilarity, *D*, by gender across occupations (e.g., Gross 1968) or (*b*) panel models in which the autoregression coefficient of percent female represents temporal stability (Treiman and Terrell 1975; Snyder and Hudis 1976). Fluctuations in *D* reflect only "net" change, that is, in extent of differentiation by gender. Substantial male-to-female shifts in some occupations, offset by opposite changes in others, may be mistaken for extreme stasis. Conversely, the regression procedure indicates, via lower values of the stability coefficient, extent of gross change or "permeability" of occupational sexual structure.

However, both aggregate (computed over all occupations) measures necessarily ignore the direction and location of change: for example, to what extent do male-to-female movements predominate over female-to-male movements? Does the observed small amount of aggregate "instability"

[2] This issue is not identical with that discussed directly above. Differential occupational distributions could account for lesser rewards for women even if female concentration has no independent influence on earnings. This would occur if gender composition were not an important component of the effects captured by Stolzenberg's (1975) use of the census nominal classification, which is a proxy for many substantive dimensions of interoccupational differentiation.

reflect merely trivial fluctuations within most occupations or substantial numbers that undergo relatively large changes? To investigate these questions, we use all detailed occupational titles that were comparable in adjacent census years (417 categories for 1950–60, 212 for 1960–70; see Snyder and Hudis [1976] for data sources and matching criteria). Mean changes in percent female for 1950–60 and 1960–70 are —0.19 and 1.69, respectively, with standard deviations of 5.77 and 5.16. The mean shifts are quite small given secular increases in women's labor force participation. However, the standard deviations indicate considerable interoccupational variation during each period. We further treat changes greater than 5% in an occupation's gender composition over a single decade as large enough to be substantively important.[3] By that criterion, large shifts were experienced by 77 of 417 occupations in 1950–60 (of which 30 were increases in proportion female) and 53 of 212 categories in the 1960–70 sample (37 female gains). Many were of considerable magnitude.[4]

Such changes are not inconsistent with conclusions of overall stability. Even if many occupations experience shifts of 5% or 10%, heavily male or female categories are still preponderantly male or female a decade later. But our "disaggregated" approach indicates that significant locations of change remain to be explained. Identifying the characteristics of stable occupations versus "changers" (in either direction) should help to explain (1) how gender-specific occupational labor market boundaries operate and (2) where policy efforts to reduce segregation in the labor force should be focused.

LABOR MARKET SEGMENTATION AND OCCUPATIONAL SEX SEGREGATION

Previous multivariate analyses of shifts in occupational sex composition (Treiman and Terrell 1975, pp. 180–88; Snyder and Hudis 1976) are largely atheoretical. However, these studies' linear models assume implicitly that fluctuations in proportions female are negatively related to indicators of occupational desirability. And they typically fail to isolate any substantively important predictors of changes in female concentration (see Snyder and Hudis 1976, p. 225).

We believe that market segmentation perspectives, particularly dual labor market theory (one major variant), suggest more informed expla-

[3] This criterion is admittedly arbitrary but differs only slightly in magnitude from alternative measures, such as one standard deviation of the change in percent female. It is also far larger than any minor variation due to sampling fluctuations.

[4] For instance, "hucksters and peddlers" (42.3%), "watchmen (crossing) and bridge tenders" (42.2%), "midwives" (—27.6%), "bank tellers" (24.6%), and "counter and fountain workers" (21.0%) evidenced the most extreme fluctuations between 1950 and 1960. See Snyder et al. (1978) for a complete listing of occupations and their characteristics.

nations of changes in sex composition.[5] Dual theory specifies a differentiation of labor markets into two largely nonoverlapping sectors: a primary market consisting of jobs that offer "high wages, good working conditions, employment stability and job security, . . . and chances for advancement" (Piore 1970, p. 55), and a secondary market with opposite types of jobs. Several factors underlie the separation of these market types and differential placement and behavior of workers within them (Piore 1970). Primary sector jobs require employment stability, and some workers are therefore excluded because of unstable behavior. There is a "fit" between job and worker characteristics in both segments. However, many persons objectively qualified for primary employment are confined to secondary jobs because of attributes (especially race and gender) that employers associate with unstable work behavior ("statistical discrimination": Phelps 1972). Primary/secondary sector separation may also have become increasingly rigidified over time due to factors such as unionization and social welfare legislation.

Consequently, workers' entry point into the occupational system is crucial. Those who locate primary market employment typically proceed along well-defined "career ladders" (Doeringer and Piore 1971). Workers relegated to the secondary sector form a largely undifferentiated and low skilled labor pool whose job mobility is rarely upward. Movement into the primary sector is difficult even for workers who meet primary job requirements. They often adopt unstable employment patterns as a function of the positions into which they are recruited (Piore 1970; see Kohn and Schooler 1973 for evidence).

While dual market theory is oriented more toward explaining stability than toward explaining change, it contains several implications for the location of shifts in occupational gender composition.[6] Fluctuations should be unlikely in primary sector occupations, as statistical discrimination and other structural controls over entry points operate to maintain stability. These stable categories should also be heavily male, but as a consequence rather than as a defining characteristic of primary segment location. Conversely, higher turnover rates, lower skill levels, and lack of job ladders in

[5] See Wachter (1974) for a thorough discussion of segmentation, including dual, perspectives Although Wachter also criticizes dual market approaches, partly on grounds that they can be subsumed within broadly conceived neoclassical models, we are not convinced by those arguments. An earlier version of this paper attempted to specify neoclassical implications for occupational sex segregation, but we could not develop falsifiable hypotheses that satisfied ourselves or any of several readers.

[6] We want first to dismiss an issue that might vitiate the applicability of dual market approaches in this context (detailed justification is provided by Snyder et al. 1978). While "jobs" and not occupations are the units of analysis in segmentation theory, Temme (1975) demonstrates that 70%–75% of the variance in several job-level characteristics is captured by the 1960 and 1970 census occupational classifications employed here.

the secondary segment should generate greater "permeability" of sex composition in these occupations. If this pool of workers is largely low skilled and undifferentiated, then employers in the secondary segment may not have any compelling preference for male or female labor (though women and minorities will be overrepresented in this pool). Although other preference patterns remain plausible,[7] the location of sizable gains or decrements in percent female will in any case be a function of idiosyncratic factors that affect the gender-specific labor supply available to particular secondary occupations. In summary, dual labor market theory implies that unattractive secondary locations are more likely to experience both male and female increases, while desirable primary-sector occupations generally maintain stable, and heavily male, gender composition.

This is not to claim that dual theory is the only relevant treatment. Oppenheimer's (1970) "sex labelling" process constitutes a segmentation variant in which gender, rather than the characteristics specified by Piore, determines the fit between job requisites and workers' attributes. Gender therefore also defines occupational labor market boundaries. Oppenheimer (1970, pp. 138–42) specifies that sexual segregation is maintained over time by absorption of women into occupations already defined, both statistically and socially, as "female" work. Tradition, employer preferences, alleged sex-linked skills, etc., underlie that process. The sex-labeling perspective does not purport to be a complete explanation of recent changes in gender composition (our data cannot address its historical hypotheses). However, it unambiguously indicates a "tipping effect" model: already heavily female occupations are most likely to further increase their concentration of women. While increases in proportions of men are usually ignored, for reasons of internal consistency occupational sex labels must apply to "male" as well as "female" categories. If so, then male gains should occur in disproportionately male occupations.[8]

DATA AND VARIABLES

Our data are characteristics of detailed occupations for 1950, 1960, and 1970 that are reported in published census tabulations (see Snyder and

[7] For instance, secondary-sector employers (like those in the primary sector) may prefer male labor but be forced to fill positions with women because men are underrepresented in this sector. However, we provide evidence elsewhere that tends to rule out this and related alternatives (Snyder et al. 1978).

[8] If "female" and "male" occupations, respectively, become more female and male, we should observe temporal increases in *D* (which is not the case). Although fluctuations in "unlabeled" categories may cancel this expected trend, sex-labeling arguments are unclear on this issue. With respect to the tipping hypothesis, there will be a "ceiling effect" insofar as already heavily female occupations (e.g., over 80%) cannot experience further large gains in female concentration. However, few occupations (20 of 417 categories in 1950; 14 of 212 in 1960) exceed 80%

Hudis 1976 for a full description). We also employ a measure of the number of years of required training: specific vocational preparation (SVP) (Temme 1975; Stolzenberg 1975, p. 647). Temme (1975) aggregates job-level SVP scores to the census detailed categories for 1960 and 1970. We imputed the 1960 values to 1950, which introduces a small bias in the 1950–60 analyses only (see Snyder et al. 1978).

In addition to percent female, census-derived measures include weighted averages of gender-specific annual income, median education, and full-time (percent working 35–48 hours) and full-year (percent employed 50 or more weeks) work. Occupational properties, without regard to gender, are the relevant explanatory variables here. Except for SVP, however, they must be represented indirectly by aggregating incumbents' characteristics. This makes little empirical difference, because correlations of male and female characteristics across occupations are very high (Treiman and Terrell 1975).

While these measures capture some primary/secondary distinctions (e.g., wages, training, employment security) specified by Piore, they are heuristic devices in several respects. We necessarily assume that other dimensions, such as job ladders and turnover, are also reflected in the measures. Moreover, the dichotomous formulation is a conceptual orientation rather than an empirical mandate since no procedure for classifying occupations into one segment or the other has been developed (Cain 1975). Given that segmentation criteria data are almost invariably continuous, occupations must be described as more or less reflecting primary- or secondary-sector location. However, for the present purposes it does not matter greatly if available measures imply anything more than "desirability" insofar as a segmentation interpretation rests on a given pattern of association with observed shifts in gender composition.

FINDINGS

Table 1 presents means and standard deviations of changes in percent female between 1960 and 1970 for high and low categories of occupational characteristics in 1960. Results for 1950–60 are not shown because of space limitations, but are nearly identical. Excepting the initial level of female concentration, each explanatory variable is divided at its median since there are no superior classification criteria (see above). There is no support for the specification that changes in female concentration are negatively related to desirability. Differences in mean changes across categories of each indicator of segmentation (top five lines, col. 1) are trivial, generally less than 1%. Conversely, findings in columns 2 (standard deviations) and 3 (means of the absolute values of the change, which score increases *and* decreases in percent female positively) are consistent with patterns ex-

TABLE 1

MEANS AND STANDARD DEVIATIONS OF CHANGES IN % FEMALE 1960–70, REPORTED SEPARATELY FOR CATEGORIES OF SELECTED OCCUPATIONAL CHARACTERISTICS IN 1960

Selected Occupational Characteristics Measured in 1960 ($N = 212$ Occupations)	Mean Change in % Female (1)	SD of Change in % Female (2)	Mean of Absolute Value of Change in % Female (3)
Income (median = $4,950):			
High	1.92	2.55	2.22
Low	1.46	6.85	4.82
Education (median = 11.85 years):			
High	1.59	4.24	3.14
Low	1.79	5.95	3.89
% full week (median = 62.2%):			
High	1.88	3.76	3.02
Low	1.51	6.22	4.00
% full year (median = 73.15%):			
High	2.47	3.29	2.81
Low	0.88	6.49	4.27
SVP (median = 6.29):			
High	1.78	3.28	2.49
Low	1.60	6.52	4.53
% female:			
High ($\geq 33\%$; $N = 42$)	−1.66	8.50	6.20
Low ($< 33\%$; $N = 170$)	2.51	3.51	2.86

pected from dual market formulations. Both measures show much greater variability within the low categories that crudely represent secondary location.[9] These results indicate that primary- and secondary-sector occupations are characterized, respectively, by relative stability and permeability in gender composition. Though far from definitive, this evidence tends to support our contention that primary occupations generally maintain stability because their stringent employment requirements (e.g., higher SVP) and statistical discrimination provide tight control over entry points.[10] In secondary-sector locations, characterized by absence of job ladders, lower skill requirements, etc., there are higher probabilities of compositional

[9] The absolute value means differ significantly ($P < .05$, one-tailed test) in the expected direction for all except the education indicator. Conversely, none of the raw mean differences in percent female in the upper panel are significant.

[10] Since there is no direct evidence for this interpretation, one reader suggested that the results might be due to omitted factors, such as demand, technological change, or upgrading through unionization, that operate differentially across occupations. Data limitations preclude examination of technological changes, but we undertook supplementary analyses of the effects of size or changes therein (a crude proxy for demand) and unionization (estimated from individual level data in the Occupational Changes in Generation II survey). Although each variable had significant effects (which varied depending on the equation and decade) on composition changes, our reported findings are not materially affected by their inclusion.

shifts. With a largely undifferentiated labor pool employers in this sector appear to hire what labor is available.

Finally, the results in the last line of table 1 (containing only initial percent female) seriously challenge the sex labeling/tipping effect expectation that heavily female occupations are generally more likely to experience increases in concentrations of women. The reverse pattern (col. 1)—which should not be isolated if employers in female occupations have strong preferences for hiring women workers—characterizes the observed shifts. Instead, both larger male and larger female gains occur more frequently in female locations (cols. 2 and 3). Such findings are again in line with dual market explanations (though no substantive importance is attributed to female concentration per se) because percent female is positively correlated with characteristics indicative of secondary-sector location. This argument suggests that percent female acts as a proxy, but by no means a perfect one, for secondary-segment occupations.

Since the results in table 1 are not independent and pertain only to a single period, we estimate multivariate models for each decade. Change in percent female is regressed on the initial level of female concentration and other predictors of substantive import. This specification is equivalent to the more familiar lagged dependent variable model in the parameters of primary interest (Treiman and Terrell 1975, p. 184n).

The upper and lower panels of table 2 present results of three alternative models for 1950–60 and 1960–70, respectively. Equations (1) and (4) again indicate the inadequacy of a simple but conventional model which assumes that increases in female concentration are negatively related to occupational desirability. The reason for this model's failure is more clear in figure 1, which represents the association between occupational characteristics (assuming higher values reflect primary location or greater desirability) and changes in percent female. That nonlinear relationship is not easily tractable via multiple regression because it is not a function. We therefore transformed the dependent variable to the absolute value of the change in percent female. This procedure scores negative values (gains in proportions of men) positively and reflects them onto the line above the X-axis in figure 1. Equations (2) and (5) in table 2 report estimates of this model, which specifies extent of composition change in either direction as a negative linear function of the predictors (except for percent female, because lower values of this variable are associated with primary location). All coefficients but those for full-year employment are in the expected direction for both decades, and this model performs significantly better than the first (eqq. [1] and [4]).[11]

[11] The SVP variable further increments the adjusted R^2 but is deleted from these models because its high correlation with income makes it difficult to apportion independent effects.

TABLE 2

REGRESSION EQUATIONS ESTIMATED OVER DETAILED OCCUPATIONS, FOR DETERMINANTS OF CHANGES IN % FEMALE: U.S., 1950–60 AND 1960–70

Decade	Equation	Dependent Variable	Partial Regression Coefficients: % Female	% Full Week	% Full Year	Income ($ Thousands)	Regression Constant	$\bar{R}^2$
1950–60 ...	(1)	Change in % female	– 025 (013)	– 031 (016)	042 (022)	–.560 (369)	1.14	.015
1950–60 .	(2)	Absolute value of change in % female	058 (010)*	– 003 (124)	.021 (.018)	–.731 (292)*	2 81	.132
1950–60 ..	(3)	Change in % female	046 (013)*	–.013 (012)	060 (022)*	– 886 (396)*	1.13	379
Interaction terms .			– 138 (019)*		– 103 (027)*	1 294 (.581)*	. .	...
1960–70	(4)	Change in % female	– 080 (014)*	004 (018)	050 (023)*	– 410 (217)	1.52	.159
1960–70.	(5)	Absolute value of change in % female	044 (011)*	– 018 (.014)	013 (018)	– 480 (169)*	5.24	.194
1960–70.	(6)	Change in % female	066 (021)*	– 012 (013)	078 (024)*	– 632 (.242)*	1.09	.558
Interaction terms		.	– 158 (023)*		– 092 (025)*	669 (.320)*	.	.

NOTE —SEs in parentheses.
* Coefficient at least twice its standard error.
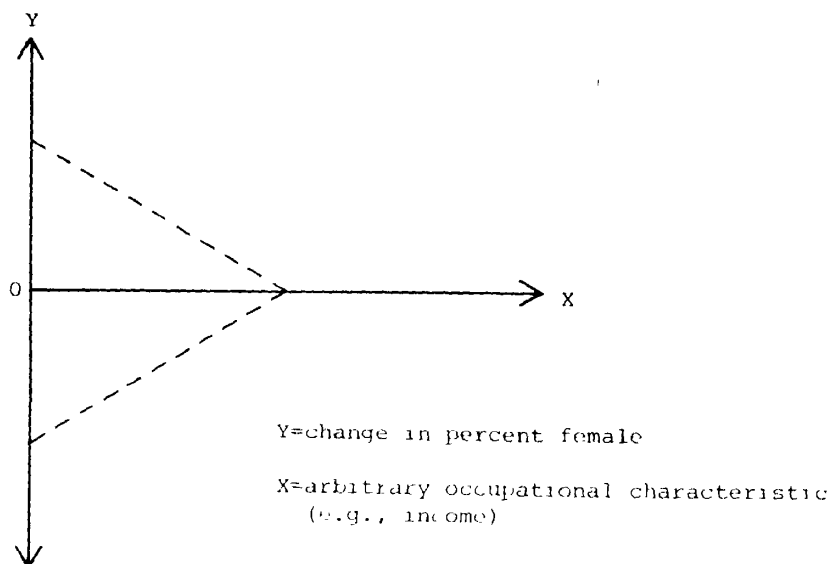
FIG. 1.—Heuristic diagram of the relationship between occupational characteristics and changes in percent female.

However, the absolute value representation makes the unrealistic assumption that an increment in any given occupational characteristic has an identical effect on the probability of a gain in percent female and in percent male. A more tenable treatment would consider figure 1 as two separate linear functions and allow the slopes to differ. We created a dummy variable, scored one if the change in percent female was below its mean and zero otherwise. We then computed its interaction (product) with each occupational characteristic.[12] Following figure 1, the difference in slopes should be reflected in positive main effects and negative interaction effect coefficients (again reversed for percent female only). With the continued exception of full-year employment, every coefficient in equations (3) and (6) conforms to these expectations, and this model's explanatory power is far superior to the other specifications.

CONCLUSION

Although market sectors can be represented only indirectly, our results offer support for dual labor market explanations of shifts in occupational gender composition between 1950 and 1970. Put another way, the patterns

[12] While using the dependent variable as the covariate criterion is an unusual procedure, it facilitates comparisons with the other models. Separate estimation of two linear functions generates nearly identical results.

of observed changes are not even roughly consistent with any plausible alternative interpretation. These conclusions suggest that segmentation theory may be extended to occupational segregation and that gender composition may be merely a consequence of other sources of labor market boundaries. Two further implications can only be mentioned. First, conventional assumptions concerning employers' gender-specific hiring preferences (e.g., women are preferred in heavily female occupations, but tastes for male labor otherwise predominate [Oppenheimer 1970; Ferber and Lowry 1976, p. 383n]) may be mistaken. Second, policy recommendations typically encourage uniform reductions in occupational sex segregation (e.g., Zellner 1972). Instead, greater movement of women into occupations with primary-segment characteristics, regardless of their current sex composition, is more likely to have an impact on gender inequality. Of course, identifying appropriate targets does not promise that such shifts will occur, since primary-sector locations appear most resistant to changes in sexual structures.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

THE APPLICATION OF LOG-LINEAR TECHNIQUES TO RECURSIVE MODELS: COMMENT ON REYNOLDS[1]

In "Some Comments on the Causal Analysis of Surveys with Log-linear Models" (*AJS* 83 [July 1977]: 127–43), H. T. Reynolds uses Bishop, Fienberg, and Holland's (1975, pp. 39–41) theorem on collapsing multiway tables to show how collapsing the categories of the independent and control variables in a log-linear analysis can yield misleading results. These points are well taken and should be kept in mind by researchers who intend to use log-linear techniques to analyze data sets with continuous or discrete-polytomous variables. Reynolds also uses the collapsibility theorem to criticize Goodman's (1973*a;* 1973*b*, pp. 1164–68) application of log-linear techniques to the analysis of recursive systems of qualitative variables. I contend that this criticism is groundless and that researchers who take it seriously could be misled by it. Moreover, in making this criticism, Reynolds perpetuates an error made by Knoke (1975) and Burke and Turk (1975) in their respective applications of log-linear techniques to recursive models. An attempt to set the matter straight seems in order. In this comment, I describe briefly Goodman's method for the analysis of recursive systems of

[1] I refer to the recently developed techniques for the analysis of multiway contingency tables as "log-linear" techniques even though some researchers may prefer to work with the equivalent multiplicative models.

qualitative variables, summarize Reynolds's objections to this method, and point out the flaws in Reynolds's argument.

Assume that a researcher posits a recursive model for four qualitative variables *A, B, C,* and *D* in which the causal ordering of the variables is the same as their alphabetical ordering. Basically, Goodman's method involves the separate analysis of successively smaller-dimensioned tables that are obtained by collapsing the categories of the dependent variable in the immediately prior higher-order table. For example, using Goodman's method, the researcher would analyze first the four-way table, next the three-way table obtained by collapsing the categories of *D,* and, finally, the two-way table obtained by collapsing the categories of *C* in the three-way table. An important feature of this method is that in each table the researcher would examine only the effects of the independent variables on the dependent variable. The joint distribution of the independent variables would be treated as given by the data, so that the marginal that corresponds to this distribution would be "fitted," that is, included in the model, regardless of the contribution of this marginal to the fit of the model for the table. Thus, the model for the four-way table would always include the *ABC* marginal, the model for the three-way table would always include the *AB* marginal, and the model for the two-way table would always include the *A* marginal.

Reynolds objects to this method on two related grounds. First, he argues that automatically fitting the marginal that corresponds to the joint distribution of the independent variables in the analysis of a particular table may introduce unnecessary parameters into the model. Second, he argues that collapsing tables can yield ambiguous results and invokes the collapsibility theorem to show that a statistically significant relationship can appear in a reduced table even though the relationship is not present in the full array. He illustrates both points by presenting a log-linear analysis of data that he claims were generated by a model in which variables *A, B,* and *C* affect *D, A* affects *B,* but *B* and *C* are conditionally independent, given *A*. The results of his analysis show that *B* and *C* are significantly related in the three-way table even though including the relationship in the model for the four-way table does not significantly improve the fit of the model to the data. The ambiguity of Goodman's method, according to Reynolds, is that the effect of *B* on *C* would be included in the model for the recursive system even though neither it nor the joint effect of *A* and *B* on *C* is required by the model for the four-way table.

Perhaps the best way to see the flaws in Reynolds's argument is to compare Goodman's method with the use of multiple regression ordinary least squares (OLS) to estimate the parameters of a recursive system of quantitative variables. Although the similarity between the equation-by-equation method of OLS estimation and Goodman's method of separately analyzing

tables of successively smaller dimensions is obvious, two points of similarity warrant discussion.

First, in the OLS estimation of a particular equation, the covariances between the independent variables are taken as given. Although these covariances are part of the information used to estimate the effects of the independent variables on the dependent variable, there is no attempt to test their statistical significance, nor is there any attempt to estimate the parameters of the causal structure that underlies them. Such analysis, if it is undertaken, is done in conjunction with other equations—equations in which one of the independent variables now appears as a dependent variable.[2] With respect to Reynolds's first objection to Goodman's method, then, fitting the marginal that corresponds to the joint distribution of the independent variables in the log-linear analysis of a multiway contingency table is analogous to taking the covariances between the independent variables as given in OLS estimation. (The two procedures would be equivalent, except that in four-way and larger-dimensioned tables it is the joint distribution among the independent variables rather than the set of bivariate relationships that is taken as a given.)

Second, in the OLS estimation of the parameters of a recursive model, the covariances of the independent variables with the dependent variable in one equation and the variance of the dependent variable are irrelevant in estimating the parameters of the causal structure that underlies the covariances between the independent variables. For example, if A, B, C, and D were treated as quantitative variables, neither the covariances of A, B, and C with D nor the variance of D would enter into the estimation of the effects of A and B on C; and neither the covariances of A and B with C nor the variance of C would enter into the estimation of the effect of A on B Similarly, collapsing the categories of D removes the distribution of D and its relationships with A, B, and C from the log-linear analysis of the effects of A and B on C; and collapsing the categories of C (in the three-way table) removes the distribution of C and its relationships with A and B from the log-linear analysis of the effect of A on B. The response to Reynolds's second objection, then, is that to attempt to determine the causal structure that links the independent variables in the log-linear analysis of

[2] With regard to this feature of OLS regression, Knoke (1975, p 423) argues that an advantage of log-linear techniques over dummy dependent-variable regression is the fact that log-linear techniques allow the researcher to test the significance of the relationships among the independent variables while testing the effects of these variables on the dependent variable. I agree that log-linear techniques possess this property but attempt to show in this comment that the results of such analyses are meaningless. Consequently, I regard as fruitless the discussion by Burke and Turk (1975, p. 319) of whether to include in their analysis of a five-way table the marginal defined by the joint relationship among the variables age, race, and prior incarceration—three independent variables in a presumably recursive model of the effects of four independent variables on court dispositions.

a four-way table would be equivalent to controlling for D in the OLS estimation of the effects of A and B on C.

An appreciation of this last point provides a basis of resolving the "ambiguity" that Reynolds sees in the results of the log-linear analysis of his contrived data. The error he makes in interpreting his results stems from his assumption that the model in his figure 1 implies the independence of B and C in the four-way table. The proper interpretation of Reynolds's data, given the causal ordering of his variables, is that both A and B affect C. This interpretation is borne out by a dummy dependent-variable regression analysis (OLS) of Reynolds's data. Reported in equations (1)–(4) of table 1, these results are consistent with the results of Reynolds's log-linear analysis. Of particular interest is the fact that in agreement with Reynolds's analysis of his three-way table, but in contrast to the model in his figure 1, the second equation in table 1 also shows a significant effect of B on C, controlling for A.

The "ambiguity" in both these results and Reynolds's arises from the fact that Reynolds did not generate his data according to the model in his figure 1. Had he done so, (i) B and C would be conditionally independent, given A, in the three-way table; (ii) consistent with the collapsibility theorem, the conditional relationship between B and C in the four-way table would be negative; and (iii) the regression of C on B, controlling for A, would be zero. Instead, B and C are conditionally independent, given A and D, in the four-way table. As shown in equation (4) of table 1, a consequence of this conditional independence is that the partial regression coefficient of C on B, controlling for A and D, is negligible ($b_{CB \cdot AD} = -0.014$).[3] The

TABLE 1

DUMMY DEPENDENT-VARIABLE REGRESSION OF REYNOLDS DATA

Equation					
(1)	$B =$	.304 +	374A		
			(029)		
(2)	$C =$	.209 +	447A +	089B	
			(029)	(.029)	
(3)	$D =$	.085 +	220A +	.442B +	178C
			(028)	(.026)	(.027)
(4)	$C =$	.181 +	379A −	.014B +	.226D
			(030)	(.032)	(034)

NOTE —The first coefficients on the right-hand side of the equations are intercepts, the remaining coefficients are unstandardized regression coefficients The terms in parentheses are standard errors

[3] The model of the conditional independence of B and C, given A and D, is represented by the log-linear model that includes the marginals (AB) (AC) (AD) (BD) (DC)—a model, as Reynolds correctly points out, that fits the data in his four-way table ex-

significance of this outcome, of course, is that just as the researcher who uses OLS regression should ignore the fact that $b_{CB \cdot AD}$ is negligible, so the researcher who uses log-linear analysis should ignore the fact that B and C are conditionally independent, given A and D, in the four-way table. A partial relationship between two variables, controlling for a variable that is causally subsequent to the two, is meaningless, whether the relationship is estimated by means of OLS regression or log-linear techniques.

In sum, the ambiguity that Reynolds sees in Goodman's method for analyzing recursive systems of qualitative variables does not exist. Instead, the two points that Reynolds fails to appreciate are (*a*) in the log-linear analysis of a particular table, the log-linear models that are analogous to OLS regression equations are those models that fit the marginal corresponding to the joint distribution of the independent variables and (*b*) the analysis of the reduced tables provides a more valid picture of the causal structure linking the independent variables than the analysis of the relationships among these variables in the full multiway table.

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REPLY TO GILLESPIE

Since the analysis of categorical data has become such an important part of social science research, continued developments in this area are welcome I am, therefore, grateful to Professor Gillespie for his thoughtful comments on my paper and for pointing out the error in the data simulation.

I have been struck by efforts to place the analysis of nominal and ordinal

ceedingly well. The log-linear model that corresponds to eq. (4), on the other hand, includes the marginals (*ABD*) (*AC*) (*DC*). Since the latter model is hierarchical with respect to the former, the fit of the former model implies the fit of the latter (The model that corresponds to eq. [4] simply adds the *ABD* interaction to the former model)

data on the same basis as that of quantitative variables. What is noteworthy about many of these undertakings is that their proponents often emphasize the formal equivalence or at least analogy between their approaches and "internal-level" methods. Consequently, one might be tempted to think that any type of model could be analyzed at any level of measurement simply by using an appropriate technique. The point of my article was to show how misleading this view can be. Gillespie's comments, however well taken, have not dissuaded me from this position.

Regarding recursive models, Gillespie is correct in noting that the data in my table 4 do not illustrate the point as well as I intended. (The model was tested with the wrong expected frequencies.) He is also correct in noting the analogies between Goodman's approach and the usual analysis of recursive models.

The difficulty, in my mind, lies in the interpretation of the results of the method. Whenever political or social variables are collapsed, one will, as often as not, find significant associations measured by λ terms that are not present in the original array. (The collapsibility theorem shows which of them will be affected. For example, collapsing D in my fig. 1 to create a three-way table among A, B, and C would affect, among others, the BC relationship.) The question then becomes, How does one interpret such relationships? Should they be "ignored" as artifacts of a specific method, or do they have substantive significance? The difficulty is compounded by noting that a simpler model will normally fit the full table.

In short, my disagreement with Gillespie stems not from the equivalence of Goodman's approach to regression analysis but from the question how one is to understand the results of such analyses. Of course, this is not the only instance in which equivalences of this nature can be found. Goodman (1972) supplies analogies for multiple and partial regression coefficients, all of which are based on log-linear models. Others have pointed to the similarities between measures of association for ordinal variables and "product-moment" coefficients (see, e.g., Hawkes 1971). In most of these cases, however, the affinities break down once the methods are applied to concrete problems and data mainly because they lead to different substantive conclusions. My article was an effort to show in a particular situation why this happens.

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COMMENT ON "REVOLUTION AND THE REBIRTH OF INEQUALITY" BY KELLEY AND KLEIN[1]

In "Revolution and the Rebirth of Inequality: A Theory of Stratification in Postrevolutionary Society" (*AJS* 83 [July 1977]: 78–99), Jonathan Kelley and Herbert S. Klein claim to offer a theory and an explicit mathematical model of a peasant revolution's effects on inequality and status inheritance. However, their theory does not adequately account for the ways in which revolutions alter forces shaping stratification, in either the long or the short run, and their approach leads to a mistaken understanding of the societies they profess to describe. But because the theory is cloaked in apparently sophisticated scientific formulas and because it appears to complement prevailing U.S. conceptions of stratification and mobility, if uncriticized it may be blindly accepted, at least by people unfamiliar with revolutionary societies.

The problems with their model are numerous. First, they argue that the long- and short-run consequences of revolutions for stratification differ without specifying the time periods that they have in mind. Yet revolutions which broke out at roughly the same time, such as the Mexican and Russian revolutions in the 1910s, and the Bolivian and Cuban revolutions in the 1950s, have had notably different effects on patterns of stratification. There may be certain similarities in the postrevolutionary trends in these societies, but any complete model must take into account the full range of consequences. Time per se is a misleading variable.

Second, Kelley and Klein present a highly generalized and idealized model of the generation and inheritance of inequalities over time within a hypothetically pure market system. They claim that "economic growth, a goal of almost all modern revolutions, expands the market economy," and that therefore market competition, based on individual differences in physical and human capital endowments, will shape postrevolutionary patterns of inequality. Because of the assumptions on which their model rests, the authors are unable to explain, theoretically, the different effects that capitalist and socialist revolutions (defined in terms of the type of political economy instituted as a result of the upheavals) may have on patterns of development in general and stratification in particular. Occasionally, they allude to countries which have experienced socialist transformations, but only in an ad hoc and noncategoric fashion. Yet these different types of revolutions are, by definition, associated with different class structures, structures which, in turn, may affect governmental allocations of scarce societal resources. Peasants in Cuba, for example, have access to many more social benefits (tantamount to income subsidies) and a larger share of their nation's wealth than do, for example, peasants in Mexico and Bolivia.

[1] I thank Paul Osterman and Theda Skocpol for their comments on an earlier draft of this comment.

But their model is not even appropriate for revolutions which usher in market economies. Human capital theory, on which they base their model, accounts for only some of the forces affecting stratification, even in the purest market economies such as the United States. Therefore, it omits a variety of forces operating in capitalist societies which have and have not had revolutions. Human capital theory assumes a labor market for skills approximating the neoclassical model. This ideal-typical model is unlikely to be applicable in any society, but especially in societies which have experienced revolutions. Revolutions in capitalist societies tend to restrict the play of market forces in the agrarian sectors affected by land reforms: While market forces may penetrate the countries in ways that they did not prior to the upheavals, land reforms generally limit the free play of these forces. Such institutionalized restrictions often are absent in comparably developed societies which have never experienced violent social transformations. The 1910 Mexican revolution, for example, resulted in a land reform which was communal, restorative, and anticapitalist in inspiration, and peasants (mainly Zapata followers) were largely responsible for the specific shape the reform took. The distributed land (known as *ejidos*) is inalienable and may not be used as collateral for securing working capital from financial institutions; beneficiaries of the land reform cannot legally move between the *ejido* and other sectors of the economy without forsaking their land rights. Similarly, the 1952 Bolivian revolution involved a land reform which has impeded the free mobility of land, labor, and capital. While it is true that market forces penetrate the agrarian reform sectors in both countries in ways that they did not prior to the respective revolutions, any complete theory or model of postrevolutionary stratification must take the restricting structural forces into account.

In employing a model based on market mechanisms, Kelley and Klein also omit adequate consideration of other important forces shaping postrevolutionary societies, forces which account for differing developments in societies at roughly similar levels of development which have and have not experienced revolutions. In particular, they underestimate, misunderstand, and exclude from their mathematical formulas political and governmental processes and institutions that shape bases of inequality in postrevolutionary settings. They argue that the state can alter stratification processes only where all vestiges of private property and individual and family differences in human capital are rooted out. But all revolutions centralize power, enlarge the state apparatus, and strengthen the state's ability to intervene in society, even in societies where private property prevails. The greater and more active presence of the state in society after revolutions means that differential connections to the state, distributive policies of the state, and the structured capacities of various groups to influence state policies will all affect who benefits and how. Through political mobilization

and recruitment, through access to state offices and patronage, and through differential access to state-sponsored education and economic programs, some people are likely to experience more mobility than they would on the basis of preexisting individual and group differences in wealth and skills. The exact effects of political forces on mobility depend upon the type of political system to which a revolution gives rise. However, there is reason to believe that the state assumes a more important role in shaping the stratification system in all societies which undergo major social upheavals than it does in comparable societies which experience no such upheavals.

The authors err also in focusing on a level of analysis that is too limited in scope to be useful analytically. They do not consider, as part of their model, ways in which international political and economic forces shape postrevolutionary patterns of stratification. The extent and nature of foreign impingement on the domestic political economies of postrevolutionary societies have varied at different points in time, but these impingements must be taken systematically into account. Since World War II, international public lending institutions—such as the IMF, the World Bank, and USAID (and its predecessors)—and commercial banks have shaped agrarian developments in countries that have experienced peasant-fought capitalist revolutions, and not merely or even mainly through investment in human capital. These institutions have concentrated largely on extending credit and some technical assistance. Often the lending agencies lend primarily to persons possessing human capital skills, but this is a political decision; they need not invest accordingly. The U.S. government, for example, heavily subsidized the Bolivian revolution, in part to prevent it from turning communist during the Cold War. The U.S. aid projects influenced the development of the Bolivian hinterland and, in so doing, exerted an impact on rural stratification. Furthermore, four years after the Bolivian upheaval the IMF—with U.S. government backing—sponsored a monetary stabilization program which affected peasants' economic prospects, in part by undermining their political strength. The military, rebuilt with U.S. aid at the time of the stabilization project, was used against "disruptive" peasants. Organized and politicized peasants had won major concessions before the stabilization program was put into effect. The experience of Mexico actually suggests that, when international powers are weak, revolutionary governments may best be able to institute policies constricting the expansion of capitalist forces in the countryside: during the Depression Lázaro Cárdenas expanded the political power of the peasantry, allocated inalienable land to peasants, and promoted collectively organized production units to an extent unparalleled by other postrevolutionary presidents.

In sum, stratification in postrevolutionary societies is shaped by many forces besides market mechanisms and individual access to human capital

resources. Social forces which antedate revolutions and postrevolutionary international relations and transnational forces, state policies, and pressures from organized domestic groups all leave their stamp on emergent patterns of inequality and status inheritance after societal transformations, in different ways under different historical conditions. To deny the importance of these forces may facilitate mathematical formalizations, but it obscures our understanding of major historical events and processes. It is possible to arrive at theoretical generalizations about the impact revolutions have on stratification, but the generalizations must be based on an adequate and accurate analysis of the full range of forces at play in specific empirical settings.[2]

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[2] Kelley and Klein have been invited to reply to this comment.—Ed.

Review Essay: Dependency in the World Economy

Unequal Development: An Essay on the Social Formations of Peripheral Capitalism. By Samir Amin. Translated by Brian Pearce. New York: Monthly Review Press, 1976. Pp. 440. $17.50.

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For more than a decade, numerous Latin American and African political economists, sociologists, and historians have been carrying on a lively discourse on the nature of the "capitalist world system" and the place of their countries within it. The task of these so-called "dependency theorists" (including Samir Amin, Giovanni Arrighi, Arghiri Emmanuel, André Gunder Frank, and Pierre Jalée, inter alia) has been to specify the "mechanisms" by which the Third World[1] has been integrated into the world's political-economic system. These writers have "unmasked" the myth that in due course "developing nations" will join the First World in global prosperity. Through their work a neo-Marxist approach to world capitalism has become a believable and encompassing analytic mode, convincing even to those who otherwise repudiate Marxist analysis.

Samir Amin has been laying the theoretical and empirical groundwork for his magnum opus, *Unequal Development* (the French edition appeared in 1973), since completing his dissertation in 1957. Since then he has published nine books in French, all of them dealing with ways in which socioeconomic conditions of certain African countries have been shaped by international (and intracontinental) trade and investment practices. His earlier monographs on Mali, Ghana, Guinea, the Maghreb (Tunisia, Morocco, Algeria, and Mauritania), the Ivory Coast, Senegal, Togo, Upper Volta, Dahomey, Niger, and the Congo richly inform his two general theoretical statements, *Accumulation on a World Scale: A Critique of the Theory of Underdevelopment* ([1970] 1974; hereafter cited as *Accumulation*), and the book here under review.[2] *Unequal Development* elaborates and emends the theoretical and empirical substance of *Accumulation*, and at half the length seems twice as dense. The purpose of the book is to offer a complete explanation for the failure of development in the Third World

[1] I use this term for stylistic convenience, but advisedly. "The false concepts of 'underdevelopment,' 'Third World,' and so forth ought to be swept away and replaced by the concept of *capitalist formations on the periphery*," as Amin argues in *Accumulation on a World Scale: A Critique of the Theory of Underdevelopment* ([1970] 1974, 1:22; see also n. 29).

[2] It is unfortunate that the former awaited translation for four years, the latter three With the world economy in turmoil, it would be useful to get Amin into English as rapidly as possible.

since World War II with special emphasis on countries in Africa and Latin America, and, through economic analyses, to demonstrate that development of a positive sort cannot be accomplished by these nations until they either extricate themselves from the world capitalist system or insist upon its reconstitution on new terms (a process not necessarily homologous with recent OPEC strategy). This, then, is a major statement and, apparently, the culmination for Amin of 20 years of work.

Amin begins by charging that conventional social science in the West (especially economics, but also sociology, psychology, history, political science, and geography), when it turns its attention to the Third World, often produces idiotic results. Such concepts as "harmonic" relationships, "marginal propensities," "perverse labor relations," "general equilibrium systems," and "interdependent markets" capture the mystifying flavor of the conventional, neo-Keynesian mentality among development economists and their allies in other fields. This approach, according to Amin, bespeaks "economism," the attempt by ideologues of "central capital" to soften rhetorically the systematic exploitation of the "periphery."

In reading Amin, one walks into the midst of a long-standing intellectual guerrilla war, and thus it is essential to identify the factions. Yet even this is not enough, since it is often the case that the dominant forces refuse to recognize the legitimacy of their opposition. Marginalist development economists (e.g., Arthur Lewis, Roy Harrod, John Hicks) and the monetarists (Milton Friedman) at this late date consider the dozen or so major studies and theoretical statements by dependency theorists now translated irrelevant or hopelessly incorrect owing to faulty assumptions. The universal attack upon Emmanuel's *Unequal Exchange* (1972), which is central to Amin, is the best example. In the few instances when the book gained notice, it was claimed that unequal exchange is, by definition, impossible (according to the classical theory of comparative advantage): no "actor" would enter into transactions which did not maximize its own welfare, and therefore any actor would "naturally" be dealing from a position of economic parity with other traders. Furthermore, it was claimed that Emmanuel's recourse to a revised labor theory of value made no sense in a (theoretical) economic universe tied to price theory and not to value theory. An examination of the latest mainstream American texts or monographs on aspects of development economics (e.g., Morgan 1975; Yotopoulos and Nugent 1976) reveals that this willful neglect of Third World voices continues. The naive reader, external to the conflict among schools of economic thought, can appreciate the indignation felt by Amin and his peers. This is yet another case of the West claiming ultimate competence for the interpretation of events occurring in other cultures. The quiet assumption behind it all is that native political economists, by virtue of their existential position and assumed automatic Marxism, lack the objectivity to understand properly international economic relations. It is typical of this syndrome that when American academics wish to be lectured on development economics they bring in a "postimperialist" expert from Great Britain, France, or Germany (when they look beyond their own borders),

rather than those who have been living amid the data for years and would it might be thought, appreciate the vicissitudes of the world system more immediately.

It is curious that the same reality, the same data, lie beneath and have given rise simultaneously to mainstream development economics (with its "production functions," "marginal opportunity costs," and "demand curves") and dependency theory with its lexicon ("peripheral formations," "comprador elites," and "unequal exchange"). This is truly a case of two language universes which cannot speak with one another. It is, I believe, very important to those interested in the field to decide where the explanatory limits of each camp are located. It is hard to believe, initially at least, that the legions who work within the canons of development economics and modernization theory are absolutely incorrect in their explanatory models and policy suggestions regarding the Third World and, by implication, that only Amin and his colleagues know what is actually going on. Yet this proposition becomes less improbable as one progresses methodically through *Unequal Development*.

Nothing short of methodical progression is called for in coming to terms with this book. Included are a concise history of precapitalist and capitalist formations throughout the world with an analytic typology of different formations and their evolution over the past two millennia (chap. 1); a thorough exposition of the neo-Marxist understanding of the "laws" of capitalist accumulation in its current phase with emphasis on the role of money and international balance of payments in "autocentric accumulation" (chap. 2); an elaboration of Emmanuel's theory of unequal exchange and an examination of the nature of the international flow of capital and how this determines "extraverted accumulation" (chap. 3); a technically detailed theory of the transition to peripheral capitalism as a permanent and necessary structure of the world economy with emphasis upon "sectoral unevenness," "marginalization," and the "blocking of transition" (chap. 4); and finally, a dense history of peripheralization in Africa and the Middle East linked to a concise portrayal of the principal features of peripheral formations and their maintenance by the West (chap. 5).

Obviously, an adequate critique presupposes a knowledge the breadth of which must approach Amin's. I do not know of any North American social scientist writing about the Third World, and especially Africa, who can claim command of enough history, economic theory, sociology, and international relations to challenge Amin in toto. This is the first dilemma facing the book as it awaits incorporation into current discourse. Amin is truly in a class by himself, which probably ensures that criticism will be leveled only at those small portions of his work which irritate various specialists. I am familiar with the common complaints against dependency theory lodged by sociologists and political scientists, and it is not difficult to imagine which features of the formal economic argument would upset development economists. But rather than run through the likely criticisms (most of which Amin has been circumspect enough to anticipate and neutralize), it would be more useful to present several key sets of

propositions within Amin's theory. However, it should not be thought that the book boils down to nothing more than these selected remarks. As may have been gathered by now, a point by point critical examination of *Unequal Development* would call for considerably more space than is available at the moment; what follows represents a partial exposition.

Amin delineates five modes of production, each of which gives rise to a specific social formation ("concrete, organized structures that are marked by a dominant mode of production and the articulation around this of a complex group of modes of production that are subordinate to it" [p. 16]): primitive communal; tribute paying in its early and late (feudal) forms; slaveowning; simple petty commodity; and capitalist—with the caveat that none of these modes has existed in pure form. Any analysis of a social formation must discover how "surplus" (in Baran's sense) is produced, how transfers of surplus from one formation to another are effected, and what share of the surplus goes to various social classes internal to the formation (p. 18). "Profit" is the form of surplus peculiar to the capitalist mode, surplus being the excess beyond mere subsistence which the society is capable of producing. Precapitalist formations (see also *Accumulation*, pp. 137–68) require that surplus be paid in other forms: in rent, in kind, and so on. When all products are transformed into commodities, the capitalist mode is dominant, and while in precapitalist formations various modes can coexist in relative harmony, the capitalist mode is "exclusive, destroying all the others" (p. 22). This view is based on the law that capitalist development demands continuous widening and deepening of the internal market for its survival, even though that process obtains only in the center of the world system and not in the periphery (also *Accumulation*, pp. 170–222) where capital allows the existence of the so-called dual economy, that is, the persistence of modified precapitalist modes. Within each mode is a pair of classes matched according to the generation, division, and circulation of the surplus. Thus, while the tribute-paying mode has a "state class" and its peasants, capitalism has bourgeoisie and proletariat. But for Amin sheer ownership of the means of production is insufficient to locate a class's place in the allocation of surpluses. Rather, its "place in the production process" determines its reward and power (as in the case of the state class which does not own the peasant's land that produces the exacted tribute).

Because no society is limited to a single social formation, any society will contain more than two classes, for the administrative support infrastructures are called upon to maintain and legitimize the relationship between the actual producers of the surplus and its major recipients. While the distribution of surplus is clear enough in precapitalist modes, "under the capitalist mode of production . . . generation of the surplus takes place obscurely, opaquely. As Marx said, the main thing in *Capital* is its demonstration of how surplus value is transformed into profit" (p. 25).

Amin attacks those who are subject to "economistic alienation" in their claim that a logical contradiction obtains between volumes 1 and 3 of *Capital*, concerning the famous transformation problem. To Amin it is clear

that what Marx was getting at is valid as such: that when surplus value is changed into profit, the proper root of value—work—is lost and the locus of theorizing, turning instead upon process, profits, and "factors," becomes instantly alienated. "This is why 'economic science' emerges as an ideology—the ideology of 'universal harmonies'—reducing the laws of society to the status of laws of nature that are independent of social organization" (p. 26). This is the beginning of Amin's polemic against conventional economics and the root of the difficulty in fully appreciating sections of his work. He makes casual references to ongoing debates with his enemies, and unless one is well versed in the fine points of the arguments involved (to the point of knowing the sources since Amin does not give them) and is willing to trace the conflict through endless mathematical formulations and contradictory "data-sets," the impact of the Marxist critique can remain blunted.[3]

What Amin has done in chapters 2 through 5, however, more than compensates for his failure to disrupt conventional economics via an elegant mathematical counterstatement. He has breathed life into the study of development (and our knowledge of the process of underdevelopment itself), which is sorely lacking in so much of what is cranked out by the "central capital's" economic establishment. But do not assume from this that Amin's analyses are "soft." It is clear that his economics are such that, did he think it legitimate or worthwhile, he could fence well enough with the "alienated empiricists" (as has his colleague Emmanuel) through formulaic complaint. It is a measure of his humanity that he refuses to debate tangents on curves while exploitation—a component of reality thus far resistant as such to empirical analysis—continues as it has for several hundred years as the central ingredient in the operation of the world economy.

Amin is very familiar with the history of ancient and medieval trade routes and the place these had in later formations. He is careful to point out, as have other Marxists, that sheer concentration of money-wealth, the fruit of early trade, alone did not account for the origin of capitalism

[3] For instance, Amin's disgust with mainstream discussions via matrices of "intersectoral capital flows" (*Unequal Development*, pp. 215 ff, and *Accumulation*, pp. 286 ff) and "astructuration" or "disarticulation" can be pursued only through the use of an input-output model and the series of equations which are a feature of such models. Since Amin uses neither the equations nor the I-O format, and does not supply suitable data, his refutation must be accepted either on faith, on the basis of cogent historical argument, or on formal logical grounds. One may sympathize with Amin's disapproval of the conventional approach, but as long as he rests content with hypothetical data to prove his points (a practice which is similar in simplistic design to Marx's in *Capital*) and mere percentages when he does supply actual data—of which by this time he must have endless amounts—comparing his work with that of conventionalists, with their structural equation models of equilibrium, will remain impossible. In other words, if critique stops at epistemology, the opposition can keep up its activities unperturbed, since, as far as I know, very few paradigms or methodologies have been shelved in the social sciences because of epistemological attack. They are shelved when they are shown to be wrong or trivial on their own terms. This, for the most part, Amin has not done.

The proletarianization of the peasants and nomads was also required. This occurred only in Europe, not in China or in the Arab world. The role of the first capitalist periphery—the Americas after the 16th century—in permanently breaking up precapitalist social formations has been documented by Wallerstein and others. Precisely this process, proletarianization of unenthusiastic peasants and villagers, was to occur again in the 19th century when European imperialism took over Africa with the same gusto with which it had stolen the Americas from the native population.

Amin offers an involved, fascinating account of trade relations among Egypt, the Arab world, and black Africa long before European penetration. While it is impossible to summarize the entire account here, one interesting set of facts may stand as an example of the way he uses intimate historical knowledge when forming broadscale hypotheses, a technique practiced by few in his field. It is commonly thought in the West that Egypt (Amin's birthplace, incidentally) is part of the Arab world, a belief encouraged by the recent wars in that area. Amin makes the point that Egypt and China were different from all other ancient cultures, especially that of the nomadic Arabians, because of their long-standing and durable form of social organization—the tribute-paying formation based on (relatively) highly productive agrarian peasants. This social formation, contrary to the slaveholding, feudal, or trading formations, possesses "its own inner dynamism," and as such "is autocentric [i.e., accumplation occurs without external expansion of the system] and constitutes its own path of evolution" (p. 58). The peripheral formations, in contrast, were derivative modes and often parasitic to the Egyptian-Chinese model. In Europe, one of these peripheral formations—capitalism—was born, since the equilibrium achieved and maintained by massive tribute-paying formations could not be produced by the rapacious protocapitalists. In brief, Amin discredits the Westerner's indiscriminate grouping of Third World cultures and nations, either in the past or in the present. In fact, he finds the use of African nations as analytical units unsatisfactory due to the arbitrariness of their contemporary boundaries. He insists that the structural uniqueness and cultural idiosyncrasies of each area be allowed to speak prior to theoretical formulation. This demand on his part makes possible an undercutting of "static" models and the creation in their place of dialectical, dynamic, and diachronic analyses of current worldwide political-economic arrangements.

Within chapter 2, "The Fundamental Laws of the Capitalist Mode of Production," a multitude of dense and, in some cases, elliptical discussions appear, most of them begun at much greater length in *Accumulation* (pp. 395-534). The topics include (1) the law of the equalization of the rate of profit and the mobility of capitals, as modified into the falling rate of profit and the intercession of multinational (transnational) corporations (MNCs); (2) "commodification" within the capitalist mode; (3) the bête noire of advanced capitalism—the fact that its capacity to produce inevitably surpasses the capacity to consume—and the expansionism thus necessitated; (4) a truncated replay of the "transformation of value" prob-

lem; (5) "conjunctural fluctuations" of economic activity and unemployment and their effects on developing nations; (6) "discounting the future" as ideology; (7) the functions of money in autocentric accumulation; (8) Marx's problem of "expanded reproduction" revised; (9) the trade cycle as distorted through the "monetarist illusion" and the "pure theory" of the Keynesians, with special attention to the failure of "multiplier" and "accelerator" effects in the Third World; (10) unmasking of the theories of "price-effects," "exchange-effects," and "income-effects"; (11) the problem of convertibility and inconvertibility into gold as the basis for monetary disaster in the developing countries and the relation this has to price structure as opposed to value structure; (12) the falseness of "automatic equilibrium" theory; (13) the nature of the international liquidity crisis and the problem of credit; and (14) the mélange of difficulties and conflicting opinions regarding the IMF, multinationals, and the balance-of-payments crisis.

As a sample of Amin's conciseness, we may consider his summary regarding the foolhardiness of expecting to find a "naturally occurring equilibrium rate of exchange," and the cyclically disheartening position of developing countries: "The devaluation of an inconvertible currency gives rise in its turn to a wave of inflation that brings the situation to where it was before. Once again it becomes clear that chronic disequilibrium cannot be avoided except by way of control over external trade and capital movements, by direct influence on real movements. When the currency has become inconvertible, the system no longer possesses the solidity it needs in order to wait for the income-effects to exhaust its consequences and for equilibrium to be restored. The tendency to disequilibrium entails permanent instability" (p. 114). This sort of direct, declarative statement coming at the end of an incisive discussion and the presentation of conflicting claims, typifies the book, especially chapters 2 and 3 ("From Specialization to Dependence"). There are dozens of pithy reflections such as this, and surely the hardiest empiricist could find within these chapters eminently "testable" hypotheses to work upon should he so choose.

In chapter 3, Amin suggests the need for "value-transformation models" in analyzing world trade. He estimates that "hidden transfers of value from the periphery to the center, due to the mechanism of unequal exchange, are on the order of $22 billion, that is to say, twice the amount of the 'aid' and the private capital that the periphery receives" (p. 144; see also *Accumulation*, pp. 37–136). With this in mind, Amin proposes that plundering of the Third World is as alive as ever, but is carried out through more efficient means than previously.

The picture that becomes undeniable, as Amin amasses data and outlines his theory regarding the international flow of capital and unequal exchanges, is one of a trap (the "debt-trap," to use Charyl Payer's phrase) which holds within its teeth a failing animal. If it moves at all, pain results, yet if it refuses to move, it will slowly die by the loss of its lifeblood The limits of the relationship between the periphery and central capital. as worked out by Amin, seem almost set by "fate." Yet the central point

of disagreement between First World analysts and Amin lies in the allocation of blame—for the former, underdevelopment is a function of certain "unfortunate" qualities, mostly sociocultural, inherent to developing nations. In Amin's view the underdeveloped countries have tried time and again to achieve modernity, and a change of values, attitudes, or mores will not break the political-economic chains that lock them into the world system. The voluntarism that suffuses bourgeois economic and sociological theory, and the bogus rationalism that services marginalism, are, in fact, the precise opposites to the determined quality of international economic relations.

Or so one believes, as one penetrates Amin's understanding of the balance of payments and the "inherent tendency of capitalism to expand markets." Extraverted accumulation, the formation foisted upon the periphery, is the result of three structural changes being experienced by the center: (1) the growth of transnationals; (2) advanced technology sending classical industry into the periphery with the center keeping for itself the most profitable, "cleanest" modes of accumulation; and (3) concentration of technological knowledge in the transnationals (p. 189). These changes mean that the periphery is undergoing tremendous restructuring, all of it initiated from and managed by the center. Repatriated profits from MNCs will grow vastly in proportion to overseas investment, as technological domination involving high technology becomes capable, unassisted, of transferring enormous amounts of relative and absolute value from the periphery to the center.

It is difficult to imagine why this trend should reverse itself, and, as Amin points out, it will do radical analysis little good to imagine a coming global apocalypse that will end this imbalance when all political-economic indicators point in the other direction. This is not to say that Amin does not recognize the crisis in world capitalism. He agrees that the superboom of the 1960s will not reappear. But import-substitution industrialization in the developing countries has failed completely, and Amin foresees one or two new arrangements of formations that will preserve the MNC's empire: either Eastern Europe will be modernized and fully integrated into the world system, or those portions of the Third World which already possess sufficient infrastructure (e.g., Mexico, Brazil, Argentina, and parts of India) will be forced through balance-of-payments pressure to begin producing manufactured goods of the "classical" capitalist type. For Amin all of this denotes a "new form of unequal specialization" (p. 190; see also *Accumulation*, pp. 170–222). This "increasing divergence between sectoral productivities in the periphery heralds another expansionist phase of capitalism: postimperialism" (p. 191).

At this point it is important to remember, however, that the peripheralization of any given country becomes feasible for central capital only if it can buy compliance from indigenous forces. This factor cannot be minimized, especially as world mores make it increasingly problematic to employ coercion for economic ends. Whether they be "local parasitic social groups which function as transmission belts: latifundia-owners, kulaks,

comprador trading bourgeoisie, state bureaucracy, etc.," or others (installed dictators), the fact remains that for the past 30 years, genuine, evenly distributed development has been sabotaged by the "luxury consumption" habits of the elites. And the lack of a market for consumer durables in developing countries is directly attributable to the fact that tiny elites absorb massive proportions of what little surplus is left after the center has taken its cut.

These and other phenomena guarantee "marginalization of the masses," in other words, "mechanisms of impoverishment: proletarianizing of the small agricultural and craft producers, rural semiproletarianization, and impoverishment without proletarianization of the peasants organized in village communities, urbanization, and massive growth in both open unemployment and underemployment in the towns, etc." (p. 194). Even more frightening is the fact that the extraverted economy, once "properly" arranged, becomes a self-reproducing configuration, requiring very little systematic attention from the outside (except for quantities of capital)—a situation which did not prevail in classical European imperialism. This process becomes, then, the ultimate in carefree, rationalized imperial domination. Yet for Amin the resulting condition is not all bad, since "the constitution of the world system [in this way] . . . has not only made possible the development of socialist trends at the periphery, but has also shifted the principal nucleus of the forces of socialism from the center to the periphery" (pp. 196–97). Whether this change in the allocation of world historical socialist importance is sufficient reward from the point of view of peripheral inhabitants is a question Amin does not address.

Chapter 4, "The Origin of the Development of Underdevelopment," is given over to an elaboration and explanation of Amin's "theory of the transition to peripheral capitalism." Since this is the most sustained, systematically presented part of *Unequal Development*, perhaps its "nine theses," to be found on pp. 200–203, bear reproducing here in abbreviated form. (1) Central capital's onslaught upon the periphery causes crucial retrogressions in the indigenous economy (such as the "ruin of the crafts without their being replaced with local industrial production," and the current peripheral agrarian crisis) of a sort never experienced by the center, its genesis having followed substantially different paths from those of the periphery. Peripheral development does not correct these retrogressions because of the nature of extraverted accumulation. (2) Unequal international specialization brings with it several distortions. It forces the periphery to produce exotic goods for which it has natural advantages since the center can more easily produce more profitable, more elastic goods. Wages fall, and, therefore, import substitution fails. (3) This initial distortion causes another, hypertrophy of the tertiary sector (Marx's "unproductive laborers"), which is artificially and unorganically related to, and imposed upon, the indigenous economy. Administrative costs drain still more surplus and help perpetuate the perennial crisis of government finance throughout the Third World. (4) Light industry develops to a limited extent in the periphery; still another distortion that develops in league with special problems origi-

nating abroad. This represents another form of disarticulation. (5) The leak that abrogates the multiplier effects that fail to operate in the periphery is not the result of hoarding, which Keynes thought to be at the center, but consists of repatriated profits from foreign investments. Because of imbalanced growth, accelerator effects are transferred to the center along with nearly all the other benefits of industrialization, instead of working as they theoretically should (see also *Accumulation*, pp. 535–88). (6) Unless policies of the peripheral governments change vis-à-vis foreign monopolies, distorted integration will continue apace. (7) Underdevelopment is not restricted to GDP figures but is a structural arrangement operating within certain important parameters which, when examined, demonstrate the essential asymmetry between the history of the West as it modernized and the development of the periphery. The asymmetry is apparent in (*a*) intersectoral unevenness of productivity and the "transmission of price structure from center to periphery" which accounts for the inequality of income distribution; (*b*) gross disarticulation due to productive goals being set by the demands of the center, with the loss of the usual benefits accruing from industrialization; (*c*) economic domination by the center expressed by the inherent tendencies of foreign capital accumulation at the periphery. (8) These center-periphery arrangements guarantee a blocking of transition and a move from dependent to autocentric, autodynamic growth (*Accumulation*, pp. 299 ff). (9) Unlike central capitalism, peripheral capitalism is not exclusive; it is more differentiated. The form of differentiation depends upon "the nature of precapitalist formations that were there previously" and the point in the history of central capitalism at which they were integrated into the world system. Thus the young central formations (which move in time from a young borrower to a young lender position) share nothing, in the long run, with the peripheral formations which all tend toward a single model "characterized by the dominance of agrarian capital and ancillary [comprador] commercial capital." This model, in all its severe limitations, rests ultimately upon political support and beneficiaries from abroad. This dependence mutilates the national community which Amin thinks is vital to autocentric growth, as technocratic and bureaucratic elites at the periphery ape those at the center. Accompanying factors are the rise of the petty-bourgeois strata and the tendency to state capitalism. Finally, all these arrangements do not promise, in themselves, a basis for the development of socialism in the Third World, but rather assure the continuation of the center-periphery relation that is as vital to central capital's health as to that of the export elites who service it.

A number of other related topics covered in chapter 4 cannot be treated here. Chapter 5, "The Contemporary Social Formations of the Periphery," is, in a sense, a return to the first part of the book since Amin presents a detailed history of the move, especially in Africa, from precapitalist to peripheral capitalist formations. There is considerable repetition, but not without the introduction of still more detail (also *Accumulation*, pp. 261–98).

The concluding pages introduce policy suggestions for a strategy of tran-

sition which, perhaps not too surprisingly, tend toward some reification of concepts (e.g., society) and, in general, do not sustain the tone of realism common to previous parts of the book. It is clear that Amin is much more comfortable in specifying the "three symptoms of underdevelopment" (sectoral inequality of productivities, disarticulation, and domination; p. 381) than in providing easily implemented solutions for blocked transition. I do not think this reduces the value of a general theoretical statement such as this in the slightest. As Amin points out, each country, and each region within those countries, faces specific problems than transcend his general model of peripheral development. To expect broad suggestions to fit any given locale would be to demand more than could sensibly be delivered. What is needed now is detailed study of particular areas in an effort to discover, as I believe will be done, whether Amin's multitude of propositions—which, it need be repeated, exceed those listed above—do justice to what is occurring to the Third World.

I want to conclude with some negative remarks, some trivial, others more critical. (1) The translator systematically fails to honor the distinction between the English prepositions "between" and "among." (2) On page 341, Amin, in an uncommon lapse, attributes the concept "lumpenbourgeoisie" to G. Arrighi, the actual originator, of course, being A. G. Frank. (3) The table of contents is totally inadequate. In making up my own, I discovered no less than 76 clearly marked, titled subdivisions within the five chapter divisions. Having these assembled helps greatly in making the argument hang together since so very much is included. (4) Amin has used no footnotes (a peculiarity of the English edition?), going to the other extreme from his practice in *Accumulation* where he used 639. This makes checking his argument almost impossible at times. In place of footnoting, he has divided the lengthy bibliography into sections which correspond roughly to major divisions in the book. Yet on numerous occasions, books and articles mentioned (without details) in the text are not to be found in their proper place in the bibliography. This situation is most inconvenient as one tries to follow the lineaments of an argument as complex as Amin's, especially with an eye to contrasting it with conventional statements, always scrupulously referenced. (5) Amin's habit of including mimeographed items in his bibliography is irritating. A paper delivered in Delhi 10 years ago by someone unlikely to be known in the West is not the best vehicle for popularizing an idea. In one instance (pp. 405–6), in a subbibliography of eight items, all are listed as "mimeo." (6) Very seldom does Amin use data newer than 1967. Given that the French edition came out in 1973, nevertheless this makes it difficult to take some of his statements at face value. The post-1973 OPEC phenomenon, of course, is untreated. A new edition with much newer data is called for. (7) Praise be to Monthly Review Press for making available in the United States about 90% of the dependency literature that is now in translation. But at such a high price, it would be nice if some of the pieces of wood floating around in the paper could be removed before the smudgy ink is applied.

And last, in paradox, if Amin's work is at all flawed, it is because of his

rigorous adherence to an enlightened Marxist analytical model, which, for all its power of explanation, is a tool most adept at post-factum analysis. A great deal of the book is anchored in the late 19th and early 20th centuries. Surely, with Amin's experience, he could now produce a study exclusively treating the past decade or so which would be of more immediate analytic use to students of dependency in the First World. The point is not that Amin should be asked to participate in the "prediction fetish" of bourgeois social science, but that he should now be willing to push ahead, having done such a masterful job of describing and analyzing the origins of underdevelopment.

Certainly it is clear that competent discussions and studies of the world economy must begin with *Unequal Development*.

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Book Reviews

Universities in the Western World. Edited by Paul Seabury. New York: Free Press, 1975. Pp. xi+303. $11.95.

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To assess this book it is necessary to know how it came to be written. The worldwide disturbances in universities in the 1960s, euphemistically called "student unrest," had abated by 1973; but they had left their marks upon universities. Some people call the marks scars; others call them reforms. The contributors to this book, while conceding that the disturbances did cause some reforms, are preoccupied with the scars.

All but two of the essays are papers presented at a conference held in Venice in 1973, organized by a body called the International Council on the Future of the University, formerly known as the International Committee on the University Emergency. This council is composed of people who are deeply concerned about the future of universities because—to put the theme very briefly—they believe that the disturbances have irreversibly changed universities, and changed them for the worse. They have been weakened by an uncontrolled expansion in numbers, a dilution in quality of teaching staff, pollution from politics, a betrayal of responsibility, a surrender of the academic ethos. The 21 contributors to this book include people of great academic distinction, drawn from 10 different countries They are all university teachers or administrators; this is a pity, because two other "estates" have a vital interest in the health of universities—students and employers of graduates—and any attempt to evaluate the events of the 1960s might have been better balanced if it had included the opinions of representatives of these other two estates.

It is, of course, far too early to evaluate these events. But the book is not premature, for it is important to put on record, during what may be a temporary lull in student unrest, the considered views of academics who lived through the 1960s and the tentative conclusions they draw about the lessons to be learned and the strategies to be pursued in the future. The plan of the book is as follows. There are 11 essays recording national experiences from Germany, the United States, Canada, Britain, the Netherlands, France, Italy, Japan, Denmark, Australia, and Sweden. There are seven essays which discuss universal perspectives, and there is a summing up (with the melancholy title "Reflections on a Worn-out Model") by the distinguished philosopher Charles Frankel, who is chairman of the board of trustees of the International Council on the Future of the University.

Testimony from the 11 countries recounts the familar story. There is

a frightening summary of the takeover in many German universities by persons who can only be called anti-intellectuals, the chaos caused by the establishment of *Drittelparität* by governments of the *Länder,* and the decay of an academic tradition which, 50 years ago, was the envy of the world and the model for universities in America and Britain. There are chapters describing how similar gestures to democratize university government have taken place in the Netherlands and Denmark. Seymour Martin Lipset analyzes the cycle from revolution to apathy on the campuses of American universities, with its residue of collective bargaining on the part of the faculty but (surprisingly, for a British observer) very little concession to the demands of students to participate in university government. In Japan, too, where by all accounts the student protests were more vicious and destructive than elsewhere, the "reforms" have left students with little more control than they had before the troubles began. Canada has come out of the cycle of unrest with comparatively little damage; so has Australia; and Britain, notwithstanding a lively outburst of antinomianism and the lamentations of the reactionaries who cry that "more means worse" (when in fact more means different), has even profited from the experience. Overdue reforms have been quietly carried out, and what James McAuley calls "the doctrine of the university" has not been eroded by heresy. The epidemic in Britain was a very mild one.

This is not to say that British universities have not been weakened by other influences; they have been weakened by the inevitable consequences of increase in size, greater dependence upon the state, deeper involvement in bureaucratic controls. And this raises the prime question: Is what Charles Frankel and his colleagues call the University Emergency something inherent in the model—the "worn-out model" as he calls it? Or is it just that the Western world is going through a great upheaval, and universities, being centers of liberalism and dedicated to constructive dissent, are peculiarly vulnerable to the turbulence? In a search for answers to these questions one turns to the seven analytical essays in the book.

Experts in military strategy say that one of the most serious unsolved problems in policies for defense is how to deal with urban guerrilla warfare. On the campus the analogous unsolved problem is how to deal with the sit-in. Approaches to a solution have been suggested (e.g., by E. Ashby and M. Anderson in *The Rise of the Student Estate in Britain* [London: Macmillan, 1970], pp. 129–55), but nothing can avoid the polarization of faculty and the confusion which this brings to the academic ethos. The analytical essays do contain some viewpoints which help to illuminate the perplexities of the last cycle of student unrest and which may guide academics into wise decisions before the next cycle begins. In depth and originality of thought the essay by Edward Shils (reprinted from *Minerva* 12 [1974]: 16) is head and shoulders above all other essays in the book. Shils sees as clearly as anyone the dangers of an academic profession suffering from "despondency of spirit." He would agree with Ralf Dahrendorf, who in another essay in the book writes: "There is no way back to the old university which existed before mass education, participatory demands, and the emer

gence of an educational class" (p. 56). But he faces courageously the practical decisions which universities will have to make if they are to preserve their traditional role in society. His strictures will hurt, for they indict some of the reforms and improvements which are commonly regarded as examples of enlightenment. For example, lifelong education is enthusiastically embraced as one of the new duties of universities to society. No one in his senses disputes the virtues of lifelong education, but if universities are expected to take responsibility for it, resources may be attenuated which would be better devoted to the prime function of universities for their students, namely, to give the students such a rigorous training in thinking that they are able to conduct their own lifelong education with a minimum of help from institutions. Another example. Many universities have embarked upon training in the creative arts, with poets in residence, courses in creative writing, and the like. Creativity, in this sense, is a precious and rare gift, but it is unlikely that universities provide the best environment for its cultivation. And then there is the uncritical welcome given to innovation. A capacity to innovate is another precious and rare gift, but, as Shils writes, great mischief is done in its name. The concomitant to successful innovation is a critical examination of the value of the thing which the innovation will replace. It is no accident that violins have not changed shape since the time of Stradivarius; all attempts to improve on the shape have failed. Some aspects of the academic ethos are equally worthy of preservation.

Is Charles Frankel right, then, when he writes about a worn-out model? Do we need in the pattern of universities a fresh paradigm, analogous to the fresh paradigms which seep into scientific thought? Doubtless the university has been badly shaken; societies which once revered it now ridicule it. But this has happened before. In the mid-eighteenth century, for instance, Oxford and Cambridge suffered a dramatic slump in admissions. There was an annual intake of little more than 100 at Cambridge and 200 at Oxford, according to the scholarly study by Lawrence Stone in a volume edited by him (*The University in Society* [Princeton, N.J.: Princeton University Press, 1974], 1:3–110). These universities—and there were no others in England—were bypassed, and there were similar slumps on the continent of Europe. It is, in my view, too soon to decide whether the model is worn out. It is not, however, too soon to reflect on the changes which Frankel proposes as his conclusions to the conference on the future of the university. Here, in synoptic form, are his proposals:

1. Phase out the assumption that there should be uninterrupted formal full-time education from age five to age 22; arrange life chances so that people can enter upon higher education at a later stage in their careers. This is not a recipe for lifelong education; it is a deferment of the intense mental effort associated with undergraduate work (if it is done properly) to a stage of life when the student desires to have it.

2. Phase out the "civic function" of higher education, not on the ground that secular moral understanding is no longer necessary but on the ground

that the citizen has other ways to get that sort of education—through the media and the dissemination of humanistic studies in books and films.

3. Reassert that higher education, by its very nature, is not for everyone any more than a place in the football team or in the symphony orchestra is a democratic right.

4. Dispel (and it will be difficult) the misconception that all of the world army of university teachers (and it must run to millions) must do research and hold Ph.D. degrees in order to earn recognition as teachers and to gain promotion. For if this were done the idealism of teachers could be concentrated upon students, and if this were done students would not protest, and if *this* were to happen the University Emergency would be over.

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Much of what has been written in the philosophy of science since about 1960 has had an implicit sociological content or relevance. Philosophers exasperated by the difficulties of accounting for the logic of scientific knowledge, shifted their attention to the history of scientific thought. They tended to interpret this history in terms of cognitive structures, called "paradigms" or "research programs," which contain ontological, methodological, and substantive elements. Scientific theories are supposed to be parts of these structures and can be correctly understood only on the basis of the ontological and methodological assumptions of them. The history of science is seen as either a succession of intellectual revolutions in which new paradigms periodically overthrow the old and decaying ones, or a permanent competition between research programs in which the more vigorous programs gradually eliminate the weaker ones.

Some of these views of the development of science seemed to bring science quite close to metaphysical, theological, and ideological systems of thought. To some sociologists this seemed a golden opportunity to extend the field of the sociology of knowledge—previously limited to the realm of nonscientific thought—to scientific thought as well. To others it seemed a threat to the very existence of sociology as a scientific discipline, since if the natural sciences do not differ from ideological and religious systems in their logical structure but only in their subject matter, the social sciences cease to have anything to distinguish them from religion and ideology. In principle, of course, the natural sciences were equally threatened, since they could be equated with magic, but few natural scientists, or others for that matter, took this seriously.

Therefore, such questions as whether the beliefs which we call scientific are more rational than others and whether there is progress in science are —in a way—more crucial for social than for natural scientists. ***Progress and Its Problems*** deals with these questions and attempts to show that it is possible to distinguish between scientific and other theories according to a criterion of rationality. "Rationality consists in making the most progressive choices," progressive referring to "problem solving effectiveness." There are difficulties about the operational definition of this effectiveness, and the book makes interesting, but not quite satisfactory, suggestions about how to overcome them.

Furthermore, the definition of "problem solving effectiveness" requires clarification of what constitutes a problem and what is an acceptable solution. This brings the author to the discussion of the kind of cognitive structures called "paradigms" and "research programs." He is dissatisfied with these concepts and suggests the term "research tradition" to describe a "set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain." This definition does not sufficiently distinguish "research tradition" from Kuhn's "paradigm" or Lakatos's "research program," but Laudan's concept is different. Unlike paradigms and research programs, research traditions can contain incompatible theories and adopt theories from other research traditions. Furthermore, theories belonging to different research traditions are comparable (in spite of the difficulty of perfectly translating the language of one tradition into that of another), and some scientists can work in different traditions at one and the same time and can, in fact, accept one tradition as more adequate from the point of view of existing knowledge, yet prefer to do their research in another, more risky but more promising tradition. Finally, the author points out that there are different kinds of research traditions, including some "nonstandard" ones which may cut across several metaphysical and methodological traditions or which may have a distinct methodology, but no ontology shared by all those following the tradition.

This concept of research tradition, which allows the existence of overlaps in personnel, theories, and problems and admits the existence of nonstandard traditions, is a great improvement on the concepts of "paradigm" and "research program." It fits the observations of the behavior of scientists far better, and it reduces to a manageable size such problems as incommensurability and anomalies. However the treatment of the concept is still unfinished. Laudan's examples of research traditions include such schools as Newtonianism and behaviorism, and one can easily accept that these are comparable phenomena. But then the term is also applied to Marxism and, in the expression that "the view that science is quasi-independent of such disciplines [as theology and metaphysics] is itself a research tradition," to science in general. These usages, which extend the term to everything from scientific schools to science in general, tend to obscure and

diminish its usefulness. Evidently, there are cognitive structures in science other than theories and research traditions. At the very least there are disciplines which would be, according to the present terminology, non-standard traditions, because they cut across a great many metaphysical and methodological traditions; and there is also such a thing as science in general. But, like most of the recent literature on the cognitive structure of science, the book does not come to grips with these phenomena.

This neglect means that the book does not really deal with science as an intellectual enterprise of a particular kind, but with the questions of rationality and progressiveness. Its main purpose is to show that in spite of the existence of metaphysical assumptions in the research traditions, science is still a rational and progressive enterprise, because judgments can be based on the criterion of problem-solving efficiency. This is an important aspect of science, but it is not all there is to it. Rationality is not limited to science, and common sense, theology, metaphysics, or even magic, can be—on occasion—as rational as science. This is abundantly shown in this book, yet it is nowhere made clear what in addition to rationality distinguishes science from other forms of rational thought.

Of course, explaining correctly what is common to science and to other fields of cognitive and practical behavior is not a mean contribution. In this respect the book continues the tradition of the 1960s and 1970s of diminishing the difference between science and other things. But while most of the recent literature has tried to show that science is as irrational as other fields, Laudan shows that those other fields are rational too. This view has important consequences for the sociology of knowledge. The chapter entitled "Rationality and the Sociology of Knowledge" shows that much of the muddle in this branch of sociology derives from an absence of an adequate theory of rationality. Thus attempts are made to find sociological explanations of events in science and other cognitive fields, when a proper theory of rationality could account for them satisfactorily without any need for sociological explanations. Some satisfaction for sociologists can be derived from the fact that none of those shown to have committed this error in recent years are professional sociologists; they are historians of science adopting a neo-Mannheimian, pre-Mertonian sociological approach.

On the whole, this is a welcome and timely book. One wishes that some of its definitions and suggestions were more precise and that the examples were more detailed and more carefully handled. But these shortcomings are compensated for by the clarity and freshness of the discussion of rationality and progress which is probably the best published in recent years. Alongside a recent paper by Stephen Toulmin, "From Form to Function: Philosophy and History of Science in the 1950s and Now" (*Daedalus* [Summer 1977], pp. 143–62), this book can be of considerable use in dissipating the confusion about science created by some of the philosophies of the sixties.

The Social Production of Scientific Knowledge. Edited by Everett Mendelsohn, Peter Weingart, and Richard Whitley. Sociology of the Sciences, a Yearbook, vol. 1. Boston: D. Reidel, 1977. Pp. vi+294. $26.00 (cloth); $13.50 (paper).

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This is the first in a series of annual yearbooks which are to appear under the aegis of an international editorial board of 13 members, with Richard Whitley of the Manchester Business School as managing editor. Everett Mendelsohn of the history of science department at Harvard is the sole U.S. member. Each volume will have its own editors, and the next books scheduled in the series are *The Dynamics of Science and Technology,* to be edited by W. Krohn, E. Layton, and P. Weingart in 1978, and *Countermovements and the Sciences,* to be edited by H. Nowotny, H. Rose, and J. R. Ravetz in 1979.

As is proper in opening such a series, the present volume begins with a clear editorial statement and a first section of four papers which discuss changing concepts and approaches in the history and sociology of science. It is made perfectly clear at the outset that this is not a yearbook of that sort of sociology of science, or even of combined sociology and history of science, with which most U.S. scholars will be familiar. It does not derive from the work of such pioneers as Robert Merton and Bernard Barber except by a process of polemical opposition, and it contains absolutely no hard methodological content or quantification at any place. The history of science found here is not that, for example, of *Isis:* It presents neither internal substantive studies of scientific content nor external historical studies of scientific institutions. It is perhaps closer to that sort of writing on social studies of science that is concerned more with the social consciousness and political principles of the participants than with dispassionate attempts at objective analysis.

In the field of sociology of science which has long been striving in various ways toward the professionalization of its own society and journal, neither of which has happened yet in spite of the recent formation of a Society for Social Studies of Science, one might have expected that a yearbook would give the much-needed opportunity for general publication in the area. It is therefore to be regretted that the present publication does not fill that role. Instead it constitutes a sort of manifesto for a new faith amongst a group of practitioners. A reviewer is not entitled to comment in terms of a book that he wishes an author had produced instead of the one at hand. By this token I can only take the manifesto in its own right and judge it by its effect on one who patently, though of some initial good will and much respect for the scholarship and integrity of the editors, cannot place himself amongst those who, converted to a disinclination to find absolute and positive values in science, seek instead almost entirely social artifacts. I had hoped that a radical social viewpoint would make

interesting and exciting reading, perhaps even shocking to traditionalists. It did not have that effect on me, merely one of tedium due to a lack of substance in all of part 1 and most of part 2. Frankly I was rather disappointed and found that the second part, where the authors should get down to business following the manifesto section, seemed to be a philosophic and to me obscure treatment of cognitive structures. The one exception is a good paper by Trevor Pinch of Bath University with the explicit title "What Does a Proof Do If It Does Not Prove? A Study of the Social Conditions and Metaphysical Divisions Leading to David Bohm and John von Neumann Failing to Communicate in Quantum Physics."

The third part of this volume contains three excellent papers under the rubric "Social Goals, Political Programmes and Scientific Norms" which contribute workmanlike scholarship in fairly conventional and stimulating analyses of science policy issues. I hazard a guess that it is much more realistic to see the dominance of social constructs in the political issues of new high technologies than in matters of basic science. A paper by Wolfgang van den Daele, Wolfgang Krohn, and Peter Weingart analyzes this particular problem and considers the various stages that constitute the attempts to direct scientific development socially in a way different from its traditional autonomy. Helga Nowotny treats the case of risk assessment in "Scientific Purity and Nuclear Danger." In the last and best chapter Dorothy Nelkin deals in a lively fashion, familiar from her previous publications, with science education in "Creation versus Evolution: The Politics of Science Education." In general, however, the first volume of this yearbook seems rather a damp firework, and we can only hope that subsequent numbers will provide more substantial pyrotechnics or perhaps seek for more contributors from the stuffier but more cumulative traditions in sociology and political science of science.

Science, Class and Society: On the Formation of Sociology and Historical Materialism. By Göran Therborn. Atlantic Highlands, N.J.: Humanities Press, 1976. Pp. 461. $19.00.

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Professor Therborn's book is a study of sociology, a critical confrontation with and suggested reconstruction of the discipline. The terrain on which the critique takes place is bounded by two sets of issues. The first set has to do with the manner and extent to which the different disciplines that study social phenomena become sciences—achieve "scientificity," in Therborn's vocabulary. The second set involves the relationships between sociology and Marxism. Of course, these two sets of issues define the terrain for most studies of sociology, and the first part of Therborn's book reviews some of the better-known or recent ones, including those by Parsons, Mills, Gouldner, and Frederich. The point of these reviews is to make clear the distinctive features of Therborn's own study.

I believe that what the author considers to be the distinctive elements of his presentation can be expressed in terms of four ideas. First, he argues that a discipline becomes a science to the extent that it discovers a *regular pattern of determination within its subject matter* and produces this discovery as a theory or conceptual object. The second idea is that only three social disciplines have attained scientific status. Those three and their conceptual objects are: (*a*) classical political economy and market regularity as a theory of economic behavior; (*b*) sociology and what the author refers to as "the ideological community," which is defined as "a community of shared values and norms" (p. 224), as a theory of society; and (*c*) historical materialism and the pattern of determination caused by the forces and relations of production as a theory of sociohistorical structure and process.

The third idea, central to Therborn's analysis, is that each of the three social sciences represents the result of a substantively and temporally different social class problematic that occurs during the history of developing capitalism. Thus market regularity as a theory of behavior is the product of an entrepreneurial capitalist class that emerges triumphant from the initial bourgeois revolutions. The ideological community as a theory of society is developed by sociologists as a "theory of politics," that is, a theory of social control, in response to problems of maintaining the legitimacy of a capitalist class during the period that is bounded by bourgeois revolutions on one side and by the threat of proletarian revolutions on the other side. The third social science, historical materialism, is the result of Marx's and Engels's discovery of the pattern of determination brought into being by the capitalist organization of the forces and relations of production and their necessary internal contradictions. Although sociology and historical materialism are historically contemporary, the latter, since it came into being only when Marx and Engels recast their ideas in terms of an identification with the working class, discovered the pattern of determination produced by a more mature and contradictory stage of capitalism.

The fourth idea is Therborn's insistence that sociology and historical materialism are quite different social sciences with distinctive subject matters. "It is strictly speaking indefensible and merely confusing to speak of a Marxian sociology, of Marxism as sociology . . . or of a convergence between Marxism and either sociology or economics" (p. 424).

These ideas and supportive arguments are developed and presented via a reading of historical social scientists, a reading which concentrates on how these founders perceived and presented their disciplines. Thus, the idea that sociology begins as a post–bourgeois revolution theory of politics is demonstrated by analyzing the works of Spencer, St. Simon, Comte, et al. And the argument that contemporary sociology is concerned with maintaining and strengthening the ideological community, in the face of attacks from a growing proletarian class, is demonstrated through a reading of Pareto, Mosca, Michels, and especially Durkheim and Weber. Other writers are included, especially in the chapter dealing with economics and histor-

ical materialism. The latter, of course, begins with Hegel and more or less follows the Althusserian reading of Marx's development.

I found the analysis of the classical sociologists Durkheim and Weber most stimulating. Durkheim is presented as sharing (but never transcending) the same problematic as the young Marx, and the comparison is interesting. The analysis of Weber is characterized by the greatest ambivalence, as is often the case for Marxists. To begin, Weber is praised as a "bourgeois Lenin" (p. 271), and Weber's erudition and powers of historical synthesis are highly valued. But then, Weber is viewed as being "antitheory" (p. 287), and the sociology of domination is perceived as being limited to how a "ruling minority" exerts domination over the ruled masses. This not only equates types of legitimacy with kinds of ideological communities but also ignores the central concern of Weber with the legitimacy of ruler vis-à-vis the staff or cadre. Finally, Weber's "inattention to illegitimacy" is severely criticized. I like the reading of Weber insofar as it cuts through and dismisses the reaction to Marx as the motive force of Weber's work. However, I think Therborn misreads the legitimacy problematic. I think a critique of Weber for his "inattention to illegitimacy" is as dubious as a critique of Marx for his inattention to theft.

I presume that Therborn wants his ideas about the distinctive content of the "three social sciences," and especially the difference between sociology and historical materialism, to be read as more than idiosyncratic and hortatory definition. But this requires confirmation from and thus confrontation with contemporary research practice. Such a confrontation is absent. Indeed the book often reads like those giant-sized texts on the history of social thought that previous generations of graduate students used in preparation for preliminary examinations. Thus, the failure to show that the variables studied by sociology include only those concerned with the ideological community and exclude the organization of the forces and relations of production and the failure to show a research praxis that is distinctive to contemporary historical materialism severely limit this book. Although the title of the book uses the word "formation," the analysis is about present condition as well. And the relationship between these two states may well involve its own dialectic.

Demands for Social Knowledge: The Role of Research Organisations. Edited by Elisabeth Crawford and Norman Perry. London and Beverly Hills, Calif.: Sage Publications, 1976. Pp. 276. $15.00.

Quality, Careers and Training in Educational and Social Research. By Caroline Hodges Persell. Bayside, N.Y.: General Hall, 1976. Pp. xix+321.

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Sociologists have a specially trained capacity and a growing but not yet perverse taste for examining themselves. Using sociology to study sociol-

ogists is by now a comfortably familiar enterprise, and most studies have focused on the social and cognitive roots of a certain metasociological position or theoretical orientation. These books exemplify a different but related line of inquiry: the sociological analysis of the organization and evaluation of empirical social research.

Each book examines a problem widely assumed to afflict social research. For Persell, the problem is the perceived poor quality of much educational and social inquiry. How can the quality and utility of empirical studies in sociology and education be improved? For Crawford and Perry, it is dwindling opportunities for university teaching positions in which one can also carry out research. Can organizations not affiliated with university teaching departments provide environments for successful social research?

The eight papers gathered by Crawford and Perry were inspired by a 1974 seminar held at Cambridge University to review the European Survey of Social Science Research Organizations. The survey was created in 1971 to compare the organization of empirical social research in several European countries. The material in this volume suggests that the survey is somewhere in the middle of its course. The discussions are largely contextual, demographic summaries of organizations in Denmark, France, and England. Few analytic comparisons are drawn among them, and little attempt is made to evaluate the relative effectiveness of the diverse arrangements.

The editors begin with an observation possibly more true of Europe than of the United States: "social science research has changed from its traditional setting—the university teaching department—to a variety of locations within and outside the university" (p. 9). Increasing numbers of social scientists are employed by independent research institutes, government bureaus, and consulting firms. The chapters in varying fashion discuss the sources and consequences of this trend.

The title of the volume suggests one source for the diversification of organizations for social research: demands for social knowledge, it is said, have in the past come largely from other social scientists, while lately they have come more frequently from government officials or business executives. Several contributors forecast that, as the audience for social research moves beyond academy walls, there will be palpable changes in the kinds of research undertaken and in the organizational settings in which it is carried out. Haurum and Friis, examining Danish social science, conclude that "a greater part of the research effort will be channeled into subject fields within which there is a special societal demand" (p. 112). Norman Perry (whose chapter stands above the rest in drawing out analytic implications from batches of numbers) predicts for England a similar decline of autonomy in problem selection. Still, it is not clear from the discussion how much decline will be due to the "bureaucratization" of social research in organizations detached from university departments and how much will be due to shifts in government funding priorities that will equally affect problem selection by social scientists in and outside teaching departments.

Expanding audiences for social research can sometimes lead to the emer-

gence of new areas of sociological inquiry. Cherns and Perry mention technology assessment and environmental studies as research areas developed in response to pressures for solutions to practical social problems. As it happens, these problems required efforts of scientists from several disciplines, and their identification led to the creation of new institutes designed specifically for this sort of work. New organizations for social research, especially those which facilitate cross-disciplinary communication and collaboration, may provide a more effective context for certain types of research than university settings, where informal barriers between departments have become institutionalized. Once created, these new institutes can in turn help to reshape the cognitive structures of disciplines. Formation of the Social Science Research Council in England led to a "holistic concept of the social sciences, as a counter to the fissiparous tendencies of individual disciplines and departments" (p. 75).

Harold Orlans provides the most far-reaching discussion of the consequences of different organizational settings for empirical social inquiry. Suggesting that fraud and dishonesty occur "too often" in sociological studies, Orlans calls for a "restoration of trust in the research community" (p. 56). Increasing amounts of research carried out for government or business clients in nonuniversity settings may make this more difficult to attain in the future. Submission of reports only to government officials who may not comprehend the technical machinations leading to recommendations increases chances that fraudulent research will pass undetected. Survival of some independent research institutes may depend (to a greater extent than for university teaching departments) on recommending what government agencies want to hear. That possibility was suggested in a memorandum from speechwriter Patrick Buchanan to Richard Nixon in 1970 reported by Orlans: "We should direct future funds away from the hostile foundations like Brookings. However, I think there is a clear national need for a Republican Conservative counterpart" (p. 50).

The eight chapters are rather insecurely held together by an effort to illuminate the sociology of science with models developed in the sociology of organizations. The effort is largely subverted by the ambiguity of goals for organizations for social research. Should priority be given to contributing to the stock of scientific knowledge, producing studies aimed at solving social problems, or training new researchers? Without the functional equivalent of "maximizing profit," the contributors find it difficult to evaluate the effectiveness of this or that form of research organization. One conclusion shines through: Several organizational settings—university teaching departments, independent research institutes, and government bureaus—have provided adequate environments for successful social research. Clearly, factors other than the formal organization of inquiry are needed to account for the wide variation in quality of social research. To enumerate these is Persell's chosen task.

The theme of the Persell book is ingenuous: Execution of educational and social research is, in the language of her evaluation forms, "not up to average standards, less than mediocre." Three hundred and ninety empiri-

cal papers in educational research were sent to 39 raters, each an outstanding scholar in educational research or in the behavioral sciences. Raters were asked to evaluate selected papers on a one-to-five scale in terms of their contribution to theoretical knowledge, contribution to educational practice, and quality of research methods. The distribution of papers for each question was slightly skewed toward the "incompetent" end of the scale. The most striking finding for Persell is that the quality of papers written by authors trained in departments of behavioral science is higher than the quality of papers by those trained in schools of education. Most of the analysis tries to explain this discrepancy by sequentially entering several dozen variables into a regression equation, a task stretched to fill over 100 pages.

Persell concludes that quality of research is positively related to previous work in a research bureau of a prestigious academic department and negatively related to the number of professional education courses taken in graduate school and to the number of years of schoolteaching experience. Research quality is also positively associated with being a staff member in a research bureau, spending more rather than less time on research, and preferring research to other professional activities.

This is scarcely cause for surprise. It is quite reasonable to expect a better grade of research from those explicitly trained to do mainly this, who spend most of their time doing it (and who like it that way), and for whom production of high-quality research is a principal factor in career advancement. For many of those who receive training in schools of education (who might at times do research), empirical investigations are a peripheral professional activity, perhaps less important for career advancement than are administrative or teaching accomplishments. To find that they do poorer empirical research is as surprising as finding that a tuba virtuoso plays a pathetic piccolo.

Twice Persell places a higher value on statistical reliability than on exploring interesting questions. She computes a correlation of .23 between raters' evaluations of quality and the number of citations a paper received. The coefficient is found "statistically significant," and perhaps because of this, characteristics of papers cited often in the literature but rated poor in quality (and vice versa) are not determined. In the same way, Persell assures us that, statistically speaking, raters are almost interchangeable, and residual discrepancies among raters are not examined. Two judges "were much more severe than the others" (p. 16), but this becomes only a caveat instead of the basis for examining their other distinctive qualities.

Many readers will come away from these books feeling cheated. The Crawford and Perry collection lacks a unified framework to guide readers through statistics that are often incomparable from one country to another. The contributors often talk past each other in slightly different conceptual languages that disguise their concern for essentially the same issues. Exactly one-quarter of Persell's book is taken up by eight papers of educational research, reprinted without annotation only to show the diversity of problems and research techniques. The names of the authors are included, elim-

inating the possibility of the kind of analysis that would have justified their inclusion: systematic examination of actual examples of papers that were rated high or low. A book on the quality of social research should be able to demonstrate points by its own example. This one does not.

Research on Human Subjects: Problems of Social Control in Medical Experimentation. By Bernard Barber, John J. Lally, Julia Loughlin Makarushka, and Daniel F. Sullivan. New York: Russell Sage Foundation, 1973. Pp. viii+263. $10.50.

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This study is proclaimed to be the first "to obtain systematic empirical estimates on both expressed ethical standards and actual behavioral practices with regard to the use of human subjects in biomedical experimentation" (p. 5). The research was built upon two issues with ethical significance: "informed voluntary consent" (p. 6) of subjects and consideration of "proper balance between risk and benefit" (p. 6) to the subjects by the researchers. These issues highlight "the dilemma of science and therapy" (p. 7).

Data were collected from six areas: (1) expressed standards and self-reported behavior of researchers; (2) some formal characteristics of institutions in which biomedical research is conducted; (3) some sociological attributes of researchers, including the quality and quantity of their research; (4) patterns of socialization of the researchers in matters of research ethics; (5) the structure of the social networks in which research is conducted; and (6) the actual process of formal peer review mandated by the Public Health Service.

The study is concerned with the social origins of satisfactory and unsatisfactory standards of concern and practice with human subjects. The data reveal that scientific values are instilled at medical schools but that concern for the protection of human subjects is sorely deficient in medical education.

The questionnaires elicited from respondents their estimates of (1) risks to subjects involved in research projects, (2) desired benefits to subjects and to future patients, and (3) the importance of scientific knowledge expected to be obtained. The authors explain: "An analysis of these data will allow us to describe the actual ethical practices of our respondents and to see how their practices are related to their expressed ethical standards" (p. 30).

The findings show that a significant minority of the biomedical respondents displays "unawareness of the importance of, or concern with, consent; willingness to take undue risks; and actually doing studies that involve unfavorable risk-benefit ratios" (p. 7).

Yet it would appear that the sociological researchers for this study have violated the very principle of voluntary consent which was a key issue in determining permissiveness, or ethical deficiences, among the biomedical researchers. One of the questions quoted Public Health Service policy: "An individual should generally be accepted as a research subject only after he . . . has consented to his participation. . . . Such consent is valid . . . only if the individual is first given a fair explanation of the procedures to be followed, their possible benefits and attendant hazards and discomforts, and the reasons for pursuing the research and its general objectives" (p. 248). Neither in the schedules themselves nor in descriptions of their administration are there indications that the subjects were made aware, for example, that their responses to hypothetical cases would be used to construct indices of their own ethical standards. Although there are indeed some questions which alert the subjects to the fact that there are ethical issues under review, several deal primarily with procedures of research committees, and most are inserted among questions on diverse subjects.

There is a likelihood that many of the respondents placed the hypothetical cases in scientific, not ethical, contexts. The reported decision-making processes, including the dominant role of the senior biomedical researcher, suggest that junior researchers may not be expected to be concerned primarily with ethical issues in commenting on research proposals. That is, their responses may have reflected the priority they were expected to give to science but need not necessarily have reflected an abandonment of concern for ethics: deference to the senior researcher and higher monitoring boards may have been in operation. Questions would need to be put in direct ethical context for that to be determined.

If it had been made clear to respondents that they were being subjected to investigation of their personal ethics, it is not likely that they would easily subject themselves to the hazards of being labeled unethical.

In addition, the schedule called upon respondents to list collaborators on research projects, which resulted in colleagues being associated with projects which may be judged to include unethical practices, without the consent, or even knowledge, of the collaborators. The data showed that the ethically permissive typically work with others like themselves. Thus, researchers who gave no consent could readily be linked to practices labeled unethical.

Ethical standards are presented as absolutes by the authors. The study would have benefited if respondents had been asked whether behaviors which the authors subsequently labeled as permissive, or deviant, or unethical, were seen in the same light by the respondents. In one data summary it was reported that only 39% of the respondents were classified as ethically strict on the risk-benefit issue; the other 61% were permissive to some degree. The authors' designation of the ethical is hardly the norm.

The authors found that those who have been unfairly treated in research careers are those most likely to deviate from strict ethical standards. It would appear that the unfair treatment emanates from those who are identified as the more successful researchers and who also are presented as the most ethical. There is irony here.

In spite of some ethical flaws, this study reports a bold and imaginative social research venture which shows that the science-therapy dilemma has not been resolved. There are, however, reasons for concern that neither biomedical nor sociological researchers give ethical principles the consideration that both the subjects and the general public deserve.

[EDITOR'S NOTE.—A comment by Barber and his coauthors and a reply by Cain will appear in the next issue.]

The Enduring Effects of Education. By Herbert H. Hyman, Charles R. Wright, and John Shelton Reed. Chicago: University of Chicago Press, 1975. Pp. ix+313. $12.50.

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Australian National University

The investigation reported in this book focuses on the long-term effects of schooling on knowledge. Two kinds of knowledge are considered: knowledge that is a direct outcome of schooling—"simple matters of fact in geography, history, the humanities, and the sciences" (p. 18), and knowledge that could not have been learned in school—knowledge of popular culture and public affairs—but which results, instead, from the knowledge-seeking habits and skills acquired during schooling. In this context two basic questions are asked: first, whether schooling imparts both knowledge and a receptivity to further knowledge such that adults with more schooling know more than do those with less schooling; and second, whether the overall level of knowledge and the knowledge gap between those with more and less schooling persist to old age.

Hyman et al. address these questions with data on 76,671 noninstitutionalized white adults of both sexes aged between 25 and 72 years. Data were obtained from 54 nationwide surveys carried out by Gallup, NORC, and SRC during the period from 1947 to 1971 and are supplemented by Canadian data from Gallup polls conducted between 1953 and 1957. The surveys selected are concentrated around four periods—the early and late 1950s and 1960s—so that the authors are able to look also at "the variations that represent special circumstances in the schooling and life course of a cohort" (p. 7).

The major mode of data analysis is age-by-education-by-cohort breakdowns of the knowledge/receptivity measures, along with some ad hoc indices, χ^2 significance levels, and γ as a measure of association. The authors present their analyses and interpretations in 142 pages of text and footnotes and in 123 tables contained in three appendices of 147 pages in all. An additional appendix details the surveys used. The overall conclusion is "that education produces large, pervasive, and enduring effects on knowledge and receptivity to knowledge" (p. 109).

Apart from a few lapses into outright waffle (e.g., the final paragraph

on p. 41 and n. 17 on p. 309) and some slippery phrases of the "numerous authorities agree" (p. 80) type, the book is well written in a simple, nontechnical style. This, together with the simple nontechnical treatment of the data, should make the book accessible to a wide audience. However, this is both an attraction and a detraction. The simplicity is attractive in the sense that one does not need to know much about sociology or statistics to understand the material presented. It is detractive in two senses: first, because of the statistical simplicity of the analysis, many of the conclusions are based on ad hoc rather than statistical criteria; and second, the presentation of the data is overwhelming in its detail and is likely to be appreciated more by trivia buffs than by the earnest reader seeking some knowledge about the enduring effects of education.

The mode of data analysis is problematic in another sense. It has led to the exclusion of nonwhites from the analyses on the basis that cell *N*'s would be too small to be meaningful (p. 10). Given that the question of racial differences in educational opportunities and outcomes remains one of the most important and contentious issues around, I believe the authors erred in restricting their analyses to whites. There *are* other modes of data analysis which do not require one to split up the sample.

However, the authors could do little about the major flaw in their study, their inability to control for the effects of intellectual ability as a logical antecedent to both education and knowledge. As a result, the enduring effects of education demonstrated in this work are attributable in part to the enduring effects of intellectual differences among those surveyed. Unfortunately, rather than simply acknowledge this, the authors choose to engage in a somewhat slippery argument designed to establish that controls over socioeconomic origins amount to indirect controls over most of the effect of intelligence (pp. 21–30). For example, on page 25 they cite, somewhat selectively, W. H. Sewell and R. M. Hauser's "Causes and Consequences of Higher Education: Models of the Status Attainment Process" (*American Journal of Agricultural Economics* 54 [December 1972]: 851–61) to support their general contention that "the controls imposed on social origins eliminate considerable variation in intelligence . . . because of the substantial correlation between class and intelligence" (p. 26), but they neglect the observation made by Sewell and Hauser later on in the same article that "less than one-fifth of the association of ability with educational attainment may be attributed to its association with socioeconomic origins. Clearly, the effect of measured ability on schooling is not merely a reflection of one's socioeconomic status, the assumptions of some social scientists to the contrary" (p. 856).

In short, the investigation seems to have been motivated by two considerations, a substantive one which is captured in the title and a methodological one aimed at promoting the merits of secondary analyses. Although the substantive conclusions must be seen as somewhat uncertain, the book does provide a compelling illustration of what could be achieved through

intelligent and creative work with existing data sources, and therein lies its major contribution to both sociology and education.

Education, Inequality, and National Policy. Edited by Nelson F. Ashline, Thomas R. Pezzullo, and Charles I. Norris. Lexington, Mass.: Lexington Books, 1976. Pp. xxii+199. $14.00.

Mary Jo Bane
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Education, Inequality, and National Policy is a compilation of papers from an invitational conference held in Newport, Rhode Island, in June 1975. The purpose of the conference was to discuss in "a relatively tranquil climate" (p. xii) the intersection of two trends—one in educational research, the other in federal educational policy—which appeared to contradict each other.

From 1965, when major federal funding for elementary and secondary education was first authorized by Congress, to 1975, the federal contribution to the national education budget quadrupled; by 1975 it accounted for 10% of all spending on education. The largest federal sums in 1975 went for assistance to educationally deprived children, for loans and grants to students in higher education, and for vocational, technical, and work training—all programs which originated in the 1960s War on Poverty. Lyndon Johnson had rejected cash assistance and job creation as major weapons against poverty; his preferred strategy was for the poor "to learn their way out of poverty." By 1975 the federal government was heavily invested in this strategy, at least to the extent of fairly large budget allocations for compensatory education, for job training, and for tuition aid to low-income families.

An emerging body of educational research, however, challenged the efficacy of the federal strategy. The first questions were raised in 1966, when James Coleman et al.'s massive survey, *Equality of Educational Opportunity* (Washington, D.C.: Government Printing Office, 1966), found no significant relationships between the level of district spending on schools and student achievement. Christopher Jencks et al.'s *Inequality* (New York: Basic Books, 1972) reported the same finding on spending and a further finding that additional years of education were relatively ineffective in equalizing adult economic success. In 1969, Arthur Jensen published "How Much Can We Boost IQ and Scholastic Achievement?" (*Harvard Educational Review* 39 [Winter 1969]: 1–123), a long technical article on IQ and achievement which began with the statement that "compensatory education has been tried and apparently has failed" and argued that IQ differences could be attributed largely to genetic differences. These studies could

be—and were being—used to argue for discontinuing or decreasing federa spending on compensatory education, training, and student aid.

Another federal strategy, also challenged by the educational research was directed at racial integration in the schools. In 1964, with the passag of the Civil Rights Act, the Justice Department and HEW joined the fed eral court system in demanding that Southern school districts take effectiv measures to end racial segregation. The Republican administration whicl took office in 1969 withdrew executive branch support for busing, but th federal courts continued to issue strong desegregation orders, often requir ing extensive busing, against both Southern and Northern cities. Coleman's research had suggested that blacks in class- and race-integrated schools hac somewhat higher levels of achievement than blacks in segregated schools But the differences were small, and some other studies found no gain at al from integration. A later piece of research, also by Coleman, suggestec that court-ordered busing led to "white flight" from cities to suburbs. Th research as a whole gave little support to those arguing for a strong federal desegregation effort.

These discouraging findings of educational research were widely publicized and discussed more or less rationally in a variety of forums. The conference at Newport which led to this volume was an attempt to explore seriously and calmly the implications of the research for federal educational policy. Of the 20 listed participants (who included Jensen, but not Jencks or Coleman), 11 were academics and nine were connected in some way with the federal policymaking process. The conference apparently succeeded in its aims. Those who wrote papers, while representing various approaches to and stances toward the issues, managed to clarify issues and carve out fairly wide areas of agreement.

The authors varied in the extent to which they challenged specific research findings. Papers by Fred Hechinger, S. M. Miller, and Torsten Husen recounted the development of ideas about education and opportunity and offered some clarifications and distinctions for interpreting the research. The genetics issue was discreetly avoided by all, even Arthur Jensen. The white flight hypothesis was challenged by Gary Orfield in a paper which argued that no conclusions about cause and effect could be drawn from the existing data. Henry Levin summarized the evidence on the links between educational strategies and improved life chances for disadvantaged students and concluded that it was inconclusive. Herbert Gans offered historical evidence that for 19th-century immigrants economic success preceded investment in education rather than vice versa. Lester Thurow's paper was also pessimistic about the role which education could play in reducing economic inequality. His review of time series data on income distribution suggested that reductions in income inequality did not occur gradually and continuously as education became more equal but were precipitated instead by two events—the thirties depression and World War II —which changed public attitudes about the fairness of wage disparities.

Despite their differences the authors of the papers all seemed to agree, more or less, with Torsten Husen's summary statement that "educational

reform cannot be a substitute for social reform." None expressed a Lyndon Johnson–style optimism about the possibility of educating the poor out of poverty. At the same time, the participants did not call, at least in the published record, for reducing the amount of federal money and energy spent on education. (Herbert Gans came the closest, in the most radical proposal in the book. He called for lowering the age of compulsory schooling and providing jobs for poor adolescents. Lester Thurow, also cynical about education, made no proposals for schooling.)

Some writers proposed, with varying degrees of ingenuity, new directions for the federal effort, especially for new federal research—not surprising, given the occupations of the participants. Edmund Gordon called for recognition of diversity and the need for diverse educational programs. Arthur Jensen called for matching teaching techniques with the characteristics of children. Carl Bereiter, in an imaginative and interesting piece, had a number of ideas about raising the overall level of intelligence in the population, among them teaching basic academic subjects with materials and techniques which are less intellectually demanding than the present emphasis on learning how to think. ("As a result of noble efforts supported by the National Science Foundation, we now know a lot about how to make mathematics and science more difficult; it remains to find ways to make them easier" [p. 140]). Marshall Smith noted some promising findings in evaluations of compensatory education programs and called for more.

All in all, the conference must have been a civilized and satisfying affair for its participants. Considerable agreement on facts and issues emerged, and major disagreements were muted. The academics found new justifications for education and new areas for needed research. The government people were assured that the existing research was not really conclusive and that their efforts in education were worthwhile. All agreed that equal opportunity and poverty were mostly somebody else's problems: The conference participants would support full employment and income transfers.

This civility, rationality, and modesty should no doubt be applauded. The problems *are* complicated, and the contributions which educational institutions can make to economic equality are limited. It is surely better for academics and policy analysts to explore areas of agreement than to shout at each other. Nonetheless, reading these conference papers left me with a vague feeling of unease. I have a lurking suspicion that conferences on job creation and income transfer strategies against poverty also conclude that the problem is complicated and the effectiveness of the strategy limited. I would not be surprised if they emphasized the importance of education.

The introduction to the volume concludes, "Perhaps if our expectations are more realistic, we will not be disappointed as often. More importantly, we can be more effective in achieving a more just and equitable society" (p. xx). I agree that realistic expectations are a good thing. But I continue to wonder whether there is any relationship between conferences and volumes like this one and "a more just and equitable society."

Stress and Contradiction in Modern Capitalism: Public Policy and the Theory of the State. Edited by Leon N. Lindberg, Robert Alford, Colin Crouch, and Claus Offe. Lexington, Mass.: D. C. Heath, 1975. Pp. xiv+450. $23.00.

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This book is a collection of essays written by some of the participants at an international conference on "Patterns of Change in Advanced Industrial Society: Priorities for Social Science Research in the 1970's and 1980's," which was held in November 1973 at Monterosso-al-mare, near Genoa, Italy. Sponsored by the Council for European Studies and the Giovanni Agnelli Foundation, this conference brought together 34 participants from Western Europe and the United States. It was also the first in a number of separate activities initiated by the Council of European Studies and "designed to mobilize resources, to focus research and to encourage an interdisciplinary and European-American dialogue on the implications of contemporary changes in advanced societies and on the kind of future that may be emerging" (p. vii).

In his "General Introduction," Leon Lindberg emphasizes the timeliness of this volume's publication. For the first time since the 1940s, we are told, "important transformations, indeed convulsions, in political arrangements seem in the offing" (p. ix). The resurgence of "great ideological debates" in the past half decade points to the limitations of those social scientists who too smugly and too blithely predicted continued secular growth, stability, and consensus within "advanced industrial societies." In the face of an impending economic downturn the editors intend this set of essays as a miniature prolegomena, or a sort of practical introduction, to comparative research which combines the analysis of macrosocietal change processes with a focus on critical political structures (viz., the "state"), public policy priorities, and the "strategies of dominant and counter elites" (p. ix).

The book itself is divided into four sections, each with a summary-*cum*-critique by one of the four coeditors. These four sections center on particular topics (or "contradictions" as they are sometimes called): planning versus the market, inequality versus opportunity, efficiency versus legitimacy, and dominance versus vulnerability. Despite these seemingly different concerns, the one underlying—and unifying—theme of the book is an examination of the nature of "the state" and the determination (and impact) of "public policies."

In part one ("Planning versus the Market"), six authors confront the various theoretical and methodological problems associated with the outcomes of market competition and the efforts of government boards, commissions, and agencies to regulate what are regarded as "adverse" effects. The chapter by Martin and the one by Cohen and Goldfinger are similar in the sense that they begin with substantive analyses of the strategies of government elites and political parties and go on to press for certain social

policies. In particular, Cohen and Goldfinger are concerned with social security policies in France. Martin is concerned with full employment, income policies, pensions, and inflation in the United States, Britain, and Sweden. The Solo, Lowi, Offe, and Alford chapters are more concerned with theoretical questions about the nature of paradigms.

Both the Martin and the Cohen and Goldfinger chapters provide evidence for a critique of the pluralist conceptions of the distribution of social services. However, they fail to fully develop alternative theoretical positions. While Solo emphasizes the distinctive role of knowledge as a new source of social and political power, Lowi recognizes that large-scale multinational firms actually become the chief beneficiaries of state policies. Offe uses the class perspective to argue that the capitalist state operates along a number of divergent lines to promote capital accumulation while state managers attempt at the same time to hide the class character of state policies.

In part two, the authors take up the question of patterns of inequality generated within three separate spheres: economic, political, and ideological. Manuel Castells, for example, concerns himself with the question of collective consumption. He argues that extant market inequalities in fact reproduce themselves in the allocation of collective resources. In addition, he demonstrates that the so-called public sector represents those arenas where private profitable production has become difficult—if not impossible—to maintain. Rein and Marris center on the relationship between government wage policies and the concepts of justice and equity. They conclude that wage policies cannot succeed without at least the appearance of justice based on the principle of equality. In the concluding chapter of part two, Crouch argues that, at least in Great Britain, there is little likelihood of an egalitarian mass movement reducing inequality to a minimum. In fact, he believes that the most likely eventuality in a period of economic decline would be state action against organized labor and a political move to the right.

In part three the authors turn their attention to the examination of the relationship between legitimacy and efficiency. They are concerned with such diverse issues as belief systems, mass loyalty, and ideology. This section contains perhaps the most varied chapters of the book, and in his introduction to the section Offe takes issue with three of the four contributors.

Part four is devoted to the broader questions of internationalization and interdependence. Kurth argues that "internationalization" and "transnational organizations" are not new phenomena. He contends that some form of regulation is the only means by which multinational corporations can be made to serve the interests of a more general public. Marxists, like Hymer and Martinelli, emphasize the contradiction between the internationalization of capital and the interests of labor, petit bourgeois, and small-scale capital.

In general, this volume of collected essays offers the reader a wide variety of perspectives on the nature of modern capitalism. Either explicitly

or implicitly, all the major debates concerning contemporary capitalism can be found in these essays. The strength of the book lies in its scope and imagination. The weakness is its occasional uneven quality and the lack of comparability between the concepts. Nevertheless, in any volume of collected essays—particularly papers delivered at a conference—it is difficult if not impossible to overcome these types of shortcomings. This book is a valuable source of information for readers becoming seriously interested in the conceptual problems associated with the study of modern capitalism.

Economics and Sociology: Towards an Integration. Edited by T. Huppes. Leiden: Martinus Nijhoff, 1976. Pp. 178. Fl 29.00.

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The preface to this collection of papers, presented at a symposium organized in 1975 by the Department of Economics of Groningen University, suggests that the book aims to help overcome the failure of economics "both to predict and counteract the economic crisis now afflicting nearly the whole Western World" (p. v). The argument is that the "manifest influence of sociological variables" on the course of economic processes remains hidden by the "sophistication" of economics as a social science because the latter abstracts from social phenomena like norms, institutions, power conflict, and social change. Two economic sociologists, N. J. Smelser and T. Huppes, and two institutionalists, A. G. Gruchy and K. W. Kapp, present papers seeking to integrate economics and sociology. I. Gadourek examines problems of integrating the two from a methodological viewpoint. Finally, Huppes attempts an interdisciplinary analysis of a concrete economic problem: inflation.

It is almost impossible, in the brief space here available, to provide an adequate critique of a series of papers, each of which formulates a completed argument from a different perspective. This review is therefore restricted to a few general observations.

First, for the most part the authors seem to have been immersed in depicting and contrasting the frameworks and methodologies of the disciplines. Although they agree that real-life problems do not appear neatly labeled "sociology" or "economics," they do not begin by analyzing the historically shaped characteristics and causes of such problems. Yet it is precisely because of this failure systematically to expose and analyze the causes of problems that the disciplines have been unable to contribute significantly to the formulation of policies to overcome them. Neoclassical economics assumes what is to be proven: that competitive profit-maximizing activities of "economically rational" individuals will lead to the best allocation of resources. As some commentators at the Groningen conference

note, however, institutionalists identify and describe the way institutions hamper realization of this assumed order of things; economic sociologists examine and categorize additional variables. Neither formulate a coherent theory to explain why these phenomena have emerged from and are integral features of the interacting contradictory characteristics of capitalism.

Yet the primary contribution social scientists can make to effective policy is to formulate and test explanations of why things happen as they do. The identification of the key constraints and resources, both physical and institutional, that must be changed to overcome the problems permits the formulation of the range of possible solutions. Evaluation of the consequences of implementing these changes will deepen theory as well as lead to improved practice.

Description, categorization, and analysis of institutions and sociological variables in isolation from explanations is inadequate for determining possible effective policies. Thus Smelser maintains that neither economics nor sociology can generate knowledge that is adequate for assessing or predicting the consequences of a given policy. The kinds of gains and costs, he says, are not really comparable, and there is no consensus on what they really are. As a result, he resorts to speculation on the probable directions and consequences of social and political changes likely to arise from high unemployment and serious inflation.

Second, the criterion for integrating the disciplines should not be the internal similarities or differences of their methodologies, which Gadourek attempts to assess—insightful though his comparison may be. It would be more fruitful to examine their methodologies in terms of their contribution to testing explanations of the causes of problems and to evaluate the consequences of policies. Given the complexity of problems, both quantitative and qualitative methods are required. It would be desirable, too, to devise new methods, not yet considered by the conventional wisdom of either discipline, to facilitate participation by those affected by the problems in creative efforts to explain and solve them.

Third, the dominant approaches introduced at the Groningen conference seem based on the assumption that analysis and proposals for change must be confined within the prevailing political economic system, despite its inability to cope with the current crisis. Smelser speculates that conflicts may emerge and prevailing cultural values may be questioned. Gruchy concludes that planning must be accepted. Kapp holds that unequal exchanges between dominating and dominated groups cause unequal distribution of income, growing disparities, and polarization, and he calls for new principles for determining social goals and public policies. But their proposals, like their analyses, remain vague, speculative. They all tend to ignore or play down the interacting, historically shaped contradictory features of the capitalist system which have culminated in the current difficulties.

Huppes admits Marx's influence on sociology but maintains that among economists he inspired mainly dissidents. (Can he really be convinced there are no economists in the socialist third of the world?) Huppes him-

self does not analyze the underlying structure of classes or the fundamental conflicts which have emerged as transnational corporations have deepened and extended the capitalist mode of production within and across national boundaries.

Gruchy rejects Marxist analysis because, he claims, data cannot be found to support it. This, of course, raises the question of methodology in another way: Will it ever be possible to develop a theory to explain real problems which can be proven true in toto with quantitative data? Does this imply that it is impossible to build such a theory? Gruchy apparently concludes that it is—leaving his arguments on a speculative basis which inevitably yields critical decisions to the dominant class wielding state power.

The *reductio ad absurdum* of the attempt to integrate disciplines without a critical examination of the historically shaped causes of real-world problems is Huppes's paper on anomie and inflation. The argument appears to be that inflation results from an irrational demand for more goods than can be provided, given scarce resources, and anomie reflects the disintegration of the ability of social norms to exert control as a result of rapidly changing complex society. In both cases, previously existing restrictions on the status quo (in the former case the function of price mechanisms, in the latter one of norms) are increasingly disregarded. Huppes argues that "from an interdisciplinary point of view, current inflation is perhaps best described by terms like anomic inflation or social structural inflation" (p. 159). He buttresses a vague hypothesis about causal relationships by presenting data showing a correlation between anomie (measured by indicators like suicide, divorce, and crime, which have been questioned elsewhere) and inflation, both of which increased at a growing rate in the post-World War II era. His policy conclusion is that, to combat inflation, "there would perhaps be more sense in fighting anomie," which calls for "highly developed systems of conflict management" (p. 160). But he neglects the critical question: In whose interest, by what political economic class, should conflict be managed? Is this anything more than another rationalization of the exercise of state power to maintain an increasingly inequitable status quo?

The conference commentator, A. Szirmai, declares Huppes's piece is "the most successful attempt at integration during the symposium" (p. 175). It certainly seems to illustrate the futility, perhaps even the dangers, of trying to integrate the traditional disciplines without a critical analysis of the inadequacies of their underlying frameworks and methodologies for explaining the objective contradictory conditions which shape real-world problems.

Ambiguity and Choice in Organizations. By James G. March and Johan P. Olsen. With contributions by S. Christensen et al. Oslo: Universitetsforlaget, 1976. Pp. 408. $20.00 (paper).

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University of Michigan

Arresting phrases appear in the table of contents: technology of foolishness, organized anarchy, drift of decisions, reorganization as a garbage can, arithmetic of attention, ambiguity of relevance. The mood thus set is enhanced in the first paragraphs of the first chapter in which appear such expressions as "curious paradox," "dramatic complexity," "what is standard in the interpretation of organizations frequently becomes irrelevant to experience," and "the [prevailing] imagery of decision making confounds an understanding of organizational phenomena." The authors invoke a world of the absurd. They aim to be unsettling, and they succeed. For good measure, they include phrases with double meanings, which are intentional, and a couple of bilingual puns which I think were inadvertent.

While the language is, in part, playful, the purpose of *Ambiguity and Choice in Organizations* is very serious indeed. It is a scholarly, inventive, and searching effort. This book carries forward the development of the "garbage can" model of organizational decision making, first so named in the authors' article "A Garbage Can Model of Organizational Choice," written with Michael Cohen (*Administrative Science Quarterly* 17 [March 1972]: 1–25). This extension consists, first, in the systematic treatment of certain issues merely mentioned in the earlier publication, and second, in the testing of the model by its use in the explication of cases.

The case materials, all from educational organizations, describe situations in which decision making of some sort appeared to be expected, or in progress, or perhaps to have taken place. Those qualifying terms, "of some sort," "appeared to be," and "perhaps" are inserted, not casually, but to emphasize the qualities of the cases, all of which involve uncertain intentions, ambiguous conditions, cognitive discrepancies, and contexts of confusion. The case analyses deal with events of kinds familiar to all of us but here interpreted in unfamiliar manner. Decisions are made that have only a distantly rational relationship to the initiating problem. Issues get redefined to fit a preferred or feasible solution. Participants in decision making may enter and leave the scene for reasons unrelated to our usual rules of role, rationality, and relevance. In some cases the decision processes take precedence over substance to the extent that no one seems interested in the decision once it is reached or in its implications. All this can be read as comic or tragic aberration arising because organizations rely on imperfect people with imperfect knowledge and unlike interests, but March, Olsen, and colleagues see it with different eyes. The prevalent condition of the context for both organizational and individual action is

ambiguity—ambiguity of purpose, of possibilities, and of preferences. The authors search for order within seeming anarchy, and they find some. What they describe is not pathology but fluid complexity. Some of the cases are rich with detail of observed events; some are analyses of roles, institutions, or organizations treated as a class.

The protocol of book reviewing does not encourage the citation of other reviews, but in this case it is justified. Prior to this writing, two notable pieces have appeared on the March and Olsen book: a commentary by Michael K. Moch and Louis R. Pondy ("The Structure of Chaos: Organized Anarchy as a Response to Ambiguity," *Administrative Science Quarterly* 22 [June 1977]: 351–62) and a review by Charles Perrow (*Contemporary Sociology* 6 [May 1977]: 294–98). They are not the usual brief descriptive notices; they are lengthy essays that go beyond the book and extend the discussion in substantive ways. I wish that I had read these essays before reading the book, and I advise that you do so. Moch and Pondy embrace the proposed lines of theoretical development, further explicate some features of the garbage can model, and then speculate about the deliberate accommodation of ambiguity in future organizational design. Perrow, in a more acerbic and critical style, notes some limitations of the garbage can model, observes that there will be other similarly untidy intrusions into the domain of rational theories of organizational choice, and suggests that we must come to terms with the March and Olsen ideas. By "come to terms" he means accept, submit, improve.

Those unfamiliar with the garbage can model should be assured that it is not as silly as its unfortunate name suggests. It is a model which in any situation of organizational choice allows for the conjunction of multiple actors, multiple goals and values, multiple action options, and the like, plus a component of chance as to which combination may take effect and how the process is changed in its composition of elements with the passage of time. Both in the more formal theoretical chapters and in the context of case explication, the metaphor of the drama recurs. First acts introduce the characters and a setting, but these usually turn out to be other than as first perceived. Coincidences occur that transform the dramatic structure. Hidden roles, personalities, and external forces come into play. The outcomes are plural, subject to several different levels of interpretation. Some of the March and Olsen cases are even provided, in retrospect, with alternative plots, just as theatergoers play the intermission game of designing alternative last acts.

The potentials of the garbage can model for linkage with other developing lines of theory are considerable. I mention only two: (1) Most decision-making models aim to exclude individuality and personality, but the garbage can model invites the person into the process, with implications about the relevance of theories of cognition, perception, learning. (2) Most decision-making models have no allowance for any in-process change in the identity of the focal organization, but the garbage can model invites the

employment of open systems theory with respect to the potential of complex, nested, partially organized systems to display continuous change in boundaries, merging of parts, and movement of the action from one part of an organization to another.

The eight collaborators, it should be mentioned, appear to form a continuing seminar group based in Scandinavia. Their joint work with the main authors has been very productive.

Organizations as Bargaining and Influence Systems. Edited by Peter Abell. New York: Halsted Press, 1975. Pp. 189. $14.95.

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Bucknell University

Since a truly sociological study of power in organizations appears infrequently, the temptation is strong to applaud any foray into this murky field. Moreover, since the work of Peter Abell and his associates is so clearly at a "working-paper" stage, one is tempted to mute criticisms still further. Nonetheless, *Organizations as Bargaining and Influence Systems* comes across as extraordinarily uneven and at the very least in need of several months of revision under a disciplined editorial hand. In addition, the theoretical structures introduced, though by far the strongest areas of the book, serve to undermine in a number of crucial ways the tentative empirical work put forward.

That individual chapters are strong in themselves is beyond question. Abell's first chapter criticizes quite effectively the two major frameworks for dealing with power phenomena. The case for imputing power by observing which resources various organizational actors control strikes Abell as "fundamentally unsound. . . . [I]t is manifestly clear that very different resources confer power in different organizations or in different parts of the same organization. . . . Furthermore, it is *a priori* problematic which sorts of resources are operative in a given context" (p. 13). Although he is less straightforward in debunking the "reputational" framework for detecting power relations (for reasons which become obvious below), sufficient critical artillery exists in the political science literature (here, for example, see Dahl, "A Critique of the Ruling Elite Model" in *American Political Science Review*, vol. 52 [June 1958], and more relatedly, Thompson's comment on Perrow in Zald, *Power in Organizations* [Nashville, Tenn.: Vanderbilt University Press, 1970], pp. 90–92). In effect, a reputation for power may not be cognate with what we "really" mean by "power," that is, the ability to gain one's preferred outcomes over resistance. In any case, the reputational approach is in scant accord with Abell's formulation. Abell operationalizes this latter view of power (the ability to gain ends over resistance) quite cleverly. Given a set of actors,

a set of decisions, a set of preferences and saliences each actor associates with each decision, and an unambiguous outcome for each decision, one can analytically separate power processes into two phases. The first, the influence phase, results from each actor attempting to influence other actors' initial preference schedules in accordance with his own preferences and saliences. Symbolically, the influence phase ends with a set of modified preferences of each actor j on each issue i (Y_{ji}) which are a linear function of the products of the influence of each actor on actor j (I_{jm}), ($m = 1, 2, 3, \ldots, n$), the salience each actor associates with this issue (S_{ji}), and the initial preferences of each actor on the issues (X_{ji}). Or, $Y_{ji} = A_j + I_{j1} S_{1i}X_{1i} + I_{j2}S_{2i}X_{2i} + \ldots + I_{jn}S_{ni}X_{ni} + U_{ji}$ for n actors and $i = 1, 2, 3, \ldots, k$ issues. Since in general the Y_{ji}, S_{ji}, and X_{ji} are known, we can solve for the I matrix using standard least-squares techniques. If the matrix of independent variables (here an $n \times k$ matrix composed of the products $S_{ji}{*}X_{ji}$) has no inverse, the implication is that the linearly dependent preference schedules form a coalition and are reduced to a single actor prior to solution. Similarly, given the modified preference schedules (Y_{ji}), we can conceive of a second phase of the power process, where these are converted into outcomes (O_i), given saliences (S_{ji}) and the power of each actor over all decisions (P_j). Symbolically, $O_i = A + P_1S_{1i}Y_{1i} + P_2S_{2i}Y_{2i} + \ldots + P_nS_nY_{ni} + U_i$. Here again, linearly dependent rows containing modified preference schedule–salience products are aggregated prior to solving for the P vector, given the known O_i.

This formulation captures the essential character of power: Power is seen as the relationship between an actor's intentions and valued outcomes. Power is, in essence, a relationship. However, this insight is not carried far enough. First, even after citing carefully the fact that different resources may be activated more or less effectively in different parts of the same organization, and, by implication, that different actors may have different amounts of influence or power over different decisions, the formulation insists on one non-contingent index of each actor's power, the P_j. It would be more sensible and consistent to consider the power of each actor over each decision, a P_{ji}, to be the main result. Further, as should be clear, this formulation bears conspicuous resemblance to Coleman's argument for rational actors (in *Quantitative Sociology*, ed. H. M. Blalock et al. [London: Academic Press, 1975], pp. 529–56) and various other network analyses. Yet network analyses in general are most effective when they contain not only relational data but also data on attributes concerning actors in the network and attributes concerning the network itself. Despite Abell's self-conscious attempt to set the analysis in "globalist" terms (p. 13), the results will tell us only which actors are powerful in the organization and not which attributes make the actor powerful. This latter contingency approach to power (dealing with the circumstances under which actor j will be powerful, or alternatively, those under which his resources will be effective) requires that we look not at causal relations but statistically at interactions. Yet in chapter 5 Abell clings tenaciously to

causal logic, in spite of the clear implications that he is really dealing with a covariance problem and an argument of opportunities, not causes. In this chapter, he gives a rather ingenious causal model for organizational outcomes which has, as two of its major intervening variables, power and influence. In the main, he posits that "weighted centrality and programmability *interact* in conferring power and influence on the unit" (p. 124). Further, "the more difficult it is to replace or substitute a unit task, then the more power and influence the performer will potentially possess" (p. 125). Needless to say, we have here two variables interacting to create an interaction between other variables. This is a complex methodological problem which reflects a highly important conceptual one.

The three chapters dealing with empirical applications of this framework fall far short of the framework itself. Chapter 3 (by D. G. Harper and E. H. M. Argent) purports to be an analysis of power and bargaining relationships in a forging firm. Briefly, the data consist of reports of the top 28 managers on how powerful each of the respondents considered every other respondent to be. (Other data are referred to but not really used.) The authors are able to conclude that some managers are more powerful than others (p. 96), that the sales department is considered to be (and considers itself to be) more powerful than other departments, and finally that reports of power do not vary with hierarchic level. The first conclusion is not terribly exceptional, and its lack of strength stems mainly from an inadequacy in the theory, which does not determine who should be considered actors. Clearly, taking each member of the firm to be an actor is not the most efficient approach, and the method of aggregating linearly dependent preference schedules (see above) seldom works. Conclusions two and three are both based on reputational measures, from which Abell broke earlier (p. 11). That sales were reputedly most important was found earlier (and more easily) by Perrow (in Zald, pp. 59–89). The finding that power does not vary with hierarchic level is rather farcical, and I suspect it tells us more about reputational measures than it does about the world. Parenthetically, it should be noted that chapter 7 suffers from most of the same defects, if less obviously.

To summarize, Abell's work is a brave, if extraordinarily uneven, attempt. There are moments of high clarity and insight, but the work as a whole suffers from a rather painful lack of articulation between theory and method and other more subtle theoretical problems. Although repeated mentions are made of forthcoming companion volumes, the work must be judged as it is, and despite its promise, it is far from complete. We do, however, await the further volumes in the series.

Multinational Co-operatives: An Alternative for World Development. By J. G. Craig. Saskatoon, Saskatchewan: Western Producer Prairie Books, 1976. Pp. xiv+135. $4.95 (paper).

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Participation by large, domestic cooperatives in multinational ventures might seem to confirm the common prejudice that established co-ops are really no different from large, profit-making businesses. Professor J. G. Craig's *Multinational Co-operatives* successfully demonstrates, however, that such cooperative endeavors serve basically different values and have fundamentally different consequences for the developing world. The author develops this argument through a series of descriptive case studies of successful multinational cooperatives. Chapters are devoted to the International Co-operative Insurance Federation, CF Industries (a large international fertilizer producer), three European consumer cooperative multinationals, the International Co-operative Petroleum Association, and the International Co-operative Bank. The book opens with a review of prevailing definitions of multinational enterprise (MNE), along with a brief contrast of the profit rationale of investor-owned MNEs, as opposed to the service rationale of cooperative MNEs. Noting the famed "co-operative principles" of the latter, the author hypothesizes that their service rationale "ought to make them more responsive to the needs and aspirations" (p. 8) of developing nations. The case studies, indeed, confirm "that the extent of impact varies with the nature of ownership and the accompanying rationale involved in decision-making" (p. 101). The comparatively beneficial impact of multinational cooperatives prompts the author to urge their consideration as alternatives to capitalist multinational corporations in world development. By credibly advancing this argument the study fills a significant gap in the literature on cooperatives, multinational enterprise, and socioeconomic development.

The book contains much useful, interesting information. After its initial summary of various attempts to differentiate domestic, international, and multinational corporations, it emphasizes the distinctiveness of co-ops as organizations whose prime beneficiaries are their user-members. The viability of the world cooperative movement is illustrated partly by describing the complex structure of its multinationals. The Scandinavian Nordisk Andelsforbund, for instance, provides purchasing services to its member national consumer cooperative wholesale organizations, which, in turn, supply their primary (local) or secondary (regional) member co-ops with a great range of consumer goods for sale to individual members. It is impressive that a movement of which Americans are scarcely aware has captured 10% of the total food trade in Western Europe and markets 27% of the petroleum products used on American farms. The case studies also provide convincing contrasts between profit-oriented and cooperative

MNEs. For instance, the joint project of Chevron Chemical Company and International Mineral and Chemical Corporation to build a fertilizer plant in India led to establishment of a company nearly 50% of whose profits belong to Americans and whose managing director is appointed by Chevron. In contrast, construction of a similar plant through joint efforts of cooperative CF Industries and the Indian government will lead to complete Indian ownership and control. Similar findings for the other multinational cooperatives lead to the conclusion that they may avoid many of the problems of profit exploitation, economic domination, and local control associated with capitalist multinational corporations in world development.

Though written by a sociologist, the book is not aimed mainly at his profession, which fact, ironically, probably explains why the study may not fully satisfy some sociologically critical readers. One difficulty is simply that the book is very short. For instance, the extensive sociological literatures on corporate "profit-maximization," modernization, interorganizational relations, organizational environment, and development administration are not discussed. The controversy over whether investor-owned MNEs do developing nations more harm than good is relegated to an appendix where various pros and cons are simply listed, without treatment.

Furthermore, the author neglects opportunities to move the study beyond exploratory description to identification of critically important variables and formulation of central hypotheses regarding the role of MNEs in socioeconomic development. Two complex typologies are presented, including one summarizing the author's main argument. The latter cross-classifies corporate rationale (profit, mixed, and service) against "form of international business activity" (ranging from domination of foreign subsidiaries by a parent company to highly egalitarian "consortium" forms of MNE organization). Consortia are presumably more likely to adopt service orientations, while traditional corporate domination is linked to profit orientation. But is type of rationale a cause or a result? What circumstances lead to changes in rationale? Is there a tendency for "discrepant" cases (e.g., consortia with profit orientations) to resolve the discrepancy somehow? If so, how, why, and under what circumstances? Neither of the two typologies is put to its best sociological use of generating interesting hypotheses for further study.

Finally, insufficient treatment of some issues may weaken the general thesis for a few readers. For instance, the argument that the "service" goals of co-ops are as acceptable a criterion of organizational "rationality" as the "profit" goals of capitalist MNEs ignores the common use of effectiveness and efficiency as yardsticks of rationality. Using the latter criteria, are profit-oriented MNEs more "rational?" The issue is not discussed, though the author notes toward the end that multinational cooperatives may be less efficient due to the slowness of democratic decision making. Another ambiguity is the question of just how comparable cooperative MNEs are to the capitalist corporations to which the former could allegedly serve as "alternatives." Obviously they differ in that the former are

service oriented, democratically controlled, and generally smaller. But in addition, at least one of the consumer co-op MNEs (Euro-Coop) is more of an advisory or lobbying group (to the European Economic Community) than a multinational business per se, and the international involvements of some other multinational co-ops are very limited—such as the one-shot joint venture of CF Industries, whose normal business is largely in the United States and Canada.

However, the author readily admits the tentative nature, small scale, and recent origin of most multinational cooperatives. His contention is simply that in many respects they are truly multinational in nature—which seems indisputable—and that they hold considerable promise as an alternative in future world development. His book performs a valuable service by pointing out and encouraging this possibility.

Why Poor People Stay Poor: Urban Bias in World Development. By Michael Lipton. Cambridge, Mass.: Harvard University Press, 1977. Pp. xi+467. $15.00.

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Michael Lipton's focus is given succinctly in his opening sentences: "The most important class conflict in the poor countries of the world today is not between labour and capital. Nor is it between foreign and national interests. It is between the rural classes and the urban classes. The rural sector contains most of the poverty, and most of the low-cost sources of potential advance; but the urban sector contains most of the articulateness, organization and power. So the urban classes have been able to 'win' most of the rounds of the struggle with the countryside; but in so doing they have made the development process needlessly slow and unfair" (p. 1). This urban bias in world development is here thoroughly dissected to determine its roots, characteristics, and possible alternatives.

Lipton is eminently qualified for the task. In addition to being Professional Fellow at the Institute of Development Studies, University of Sussex, he has wide direct experience with Third World development, having served on technical missions in various countries (including Bangladesh, Sri Lanka, the Sudan, and elsewhere) for the ILO and World Bank and other international organizations. He is editor of the *Journal of Development Studies* and an author, editor, and contributor for several related studies. This research was supported by the Rockefeller Foundation.

The author's position is not directed against industrialization; rather, he holds that "a developed mass agriculture is normally needed before you can have widespread successful development in other sectors" (p. 23). The first third of the book is divided into two parts. Part one addresses the dominance of decision making by the urban elite, wherein government

manages development "from, by and for people in cities" (p. 68). Part two, "Rural and Urban Sectors: The Poverty of Ideology," examines the ideologies of Marxism, neoclassical economics, and moralists. Literary backgrounds of urban bias ranging from Gandhi and others to Fanon ("Exploitation can have a black or brown face as easily as a white face") are reviewed briefly.

Part three, "Urban Bias: Some Evidence," comprises two-thirds of the text. Necessarily, the analysis is predominantly in terms of economics, but en route it provides a partial "course" in development economics for other social scientists. This is hardly bedside reading; it needs to be studied sitting straight up, without distractions, and with exceeding care. Lipton acknowledges data problems throughout, but he effectively marshals the evidence available and in nearly 100 pages of notes and tables directs the reader to an exceptionally broad literature that will be of inestimable value to social scientists, especially economists and national planners; a detailed index enhances the utility of the whole volume.

Lipton initiates his analysis of urban bias in world development with chapters 5 and 6, in which he discusses explanations, evaluations, and the significance of the increasing rural-urban disparity in welfare and earnings. Chapters 7 and 8 explore unbalanced shares in urban and rural capital and capital efficiency; together with chapters 10, 12, and 13, these discussions will be of special interest to economists. Chapter 9, "The Myths of Urbanization," covers ground more familiar to sociologists. A few questions arise, however. For example, in spite of demonstrated difficulties of measuring intelligence and levels of education anywhere and the paucity of evidence on these subjects for developing countries, the author subscribes to the view that "successful migrants who remain in the city are richer and better educated than their fellow-villagers" (p. 216). And Bert Hoselitz may be surprised to find himself described as "primarily a sociologist" (p. 219). Lipton examines briefly the villagers' "voting with their feet" cityward and "pseudo-urbanization" and its impact on the village. He observes that "urbanization remains the hope of the hopeless, the outlet of the occasional exceptional villager, but the opium of the development expert" (p. 237). In chapter 11, "The Rural Skill Drain," he finds rural education largely irrelevant to rural needs, "a huge sieve, through which the ablest young people pass to the cities, there to help the urban elite" (p. 259).

Chapter 10, "The Need for Savings," reviews that complex subject and together with chapters 12, "Tax Policy Towards the Rural Sector," and 13, "Price Twists," will be of special importance to economists and development specialists, but will be illuminating to others as well, even if long and difficult. Lipton demonstrates that, contrary to some authorities, "agriculture and the rural sector are *over*taxed, relative to the rest of the economy" (p. 271). These and most other chapters provide sociologists and others with a reasoned antidote of basic economic factors sometimes overlooked or undernoted in their temptations to generalize, so readily apparent in the literature on developing countries.

Lipton's concluding chapter 14, "What Can Be Done?" approaches an-

swers, first by evaluating alternatives ranging from persuasive to revolutionary means. He concludes that dealing with urban bias by persuasion or revolution is unlikely to be successful. Rather, he reviews the pros and cons of peasant movements, labor movements, and actions that might be taken by businessmen, politicians and planners, research at home and abroad, developed countries, and international organizations. What is desperately needed is a convergence of rural and urban interests in total national development, both in policy and implementation. The author sees some light on the horizon: "The few Less Developed Countries that might plausibly be regarded as relatively successful in steering the fruits of development towards the poor would presumably include: in Asia, China, Israel, South Korea, Taiwan, perhaps Ceylon (Sri Lanka); in Africa, Malawi and Tanzania; in America, Guyana and Cuba" (p. 74).

How food allocation affects national development is central to Lipton's analysis. A subject of continuing crucial importance, it has been examined by many, including Gunnar Myrdal in *The Challenge of World Poverty* (New York: Random House, 1970). More recently, and for a wider audience, it has been explored in connection with international aid by Frances Moore Lappé, Joseph Collins, and Cary Fowler in *Food First* (Boston: Houghton Mifflin, 1977) and by Susan George in *How the Other Half Dies* (Montclair, N.J.: Osmun & Co., 1977).

Since there is a plethora of specialists with contrary views about national development, this book will spark controversy; economists, in particular, may detect flaws not readily discernible to other social scientists. But as an incisive, carefully analyzed and documented study it merits reading, comprehension, and action by both responsible decision-making nationals of Third World countries and their external advisers. Both data and observation have led me to formulate elsewhere the conclusion that most urban residents in the Third World live close to the line of "subsistence urbanization," with only a very small—albeit powerful, as this study demonstrates—minority comprising the urban elite decision makers. Lipton's evidence of their existence and influence is impressive, however, and presents the spectacle of a worrisome and stubbornly persistent urban bias in a Third World where, it appears, the urban tail wags the rural dog.

Social Change: Social Theory and Historical Processes. By Anthony Smith. London and New York: Longman, 1977. Pp. viii+184. $8.50.

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Smith states that he is skeptical about "theories" of social change and holds that in the entire complex field of change "approaches and perspectives" have greater value than "overall frameworks or theories" (p. 2). Indeed, when he views what he takes to be theories of social change, he tends to concentrate severely on the question of what is wrong with them

and usually has no trouble in finding a good deal wrong. He is particularly bothered by sociological readings of change that carry a heavy "neo-evolutionary" load of concepts like structural differentiation, adaptation, and integration. The basic weakness of neo-evolutionary accounts, he argues, is their "overdetermination" of change. Such accounts appear to raise up ghostly phenomena on the order of "the 'spirit' of modernism which has allegedly brought evolutionary success to the West" (p. 68).

Overdetermination is clearly considered to be connected with excessive attention to unanticipated or unintended consequences of action. Where are active, creative human beings to be found amid all these unanticipated or unintended consequences and crystallizations or precipitated structures of action? We must return to human volition at its roots, particularly as it is shown in the work of "collective agencies" (p. 69). Something of a puzzle arises. Smith would not have us stray very far from the events of history. Perhaps we should try to build some sort of bridge between sociological theories (or at least approaches) to change and straightforward history not especially concerned with anything smacking of the sociological. But how would we do this? Smith appears to leave so little sociological land that one fears one end of the bridge could never take hold. It won't help much to say, for example, that we need perspectives allowing for both diffusion and immanent development. That leaves the sociological land far too infirm. Nor do we seem likely to get great help in this connection from what Smith calls "transunit influences with special reference to mutual interactions between rival centers of power and culture" (p. 135).

The bulk of this book is devoted to "images of change" and to discussions of neo-evolutionism, of the study of diffusion, and of the work of Marx and Weber on change. The relatively long discussion of Marx and Weber is fitted rather perfunctorily into the rest of the book and does not add to its strength or point. There is a failure of attention to the convergence of a number of analyses of change that can be drawn from the relevant literature, that do not, indeed, constitute fully developed "theories" but that are significant and most suggestive up to the present day. Thus, Smith draws on Sahlins and Service to expound the notion of an adaptation so felicitous that it becomes inhibitive and stands in the way of further adaptive innovation. Although Toynbee is mentioned briefly and the bibliography lists *A Study of History*, the brilliant development in the fourth volume of that work of much the same idea as Sahlins and Service present is not referred to. Nor is Herbert Spencer's development of the idea. Sahlins and Service, Toynbee, and Spencer enrich one another in juxtaposition. Smith accordingly misses an opportunity of a type that one would think would especially attract someone who believes approaches and perspectives in this field of change have greater value than theories. Although Smith tells us (p. 1) that he does not try to be comprehensive in his coverage of theories, it is regrettable that he neglects Kroeber's *Configurations of Culture Growth* and Sorokin's *Dynamics*, which contain much that is relevant to his themes.

In its way, this volume is a highly methodological exercise, constantly calling our attention to procedural strategies and assessing their potentialities and their limitations. But judged as methodology it is decidedly low powered and leaves one reader, at least, with an impression of wordiness and sterility. As "theoretical" discussion, it attains only an exceedingly modest height.

Much reading evidently lies behind this book. Smith has a flair for significant quotations. It is a pleasure to find him quoting (p. 37) Spencer's statement in his *Principles of Sociology* that "social progress is not linear but divergent and redivergent," thereby, one may hope, helping to relieve Spencer of some of the more excessive imputations of rigidity to his thought. It is a pleasure, too, to find a long quotation (p. 112)—familiar but strategic—from the preface to the second edition of *Capital,* whence the inevitabilist component in Marx, conducing to the view that "History" has its own ineluctable way, whatever humans may desire, comes very clear. Unfortunately, the incidental merits of Smith's work are too small to compensate for the shortcomings of a poorly conceived analysis.

Images of the World in the Year 2000: A Comparative Ten-Nation Study. Edited by H. Ornauer, H. Wiberg, A. Sicinski, and J. Galtung. Atlantic Highlands, N.J.: Humanities Press, 1976. Pp. xvi+729. $45.00.

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This massive 10-nation study of the imaged future in the year 2000 has its roots in the encouragement given in the early sixties by Julian Hochfeld, Polish Deputy-Director of UNESCO's Social Science Division, to a group of young scholars in Norway, Poland, and France to gather materials for the development of a body of theory on the significance of public opinion for foreign policymaking. It grew "organically" from a three-nation study of images of a disarmed world to the final 10-nation multidimensional representative sample study of the future as imagined by 9,000 women and men between the ages of 15 and 40. Reading it engenders excitement, frustration, and irritation: excitement over the daring scope and unusual character of the data and the careful recording of a model of collaboration among scholars trained in very different ideological traditions; frustration because the book is very long and some reports are given in great detail, making it difficult to retain the larger picture of what the study is about; irritation because the same data are bent and stretched in so many different ways by different authors that interpretations and conclusions far outrun what the data themselves can carry.

There are certain insoluble problems connected with the reporting of large-scale cross-nation studies. Not only must the data be shared in sufficient detail to be useful to others, the frames of reference of all the researchers must be shared too, because it is these frames that give meaning

—if sometimes contradictory meaning—to the data. The collaborators in this study have interacted and argued every step of the way to final publication, and readers who can hang on all the way through are in a position to make their own judgments about interpretations given. An excellent feature of the book is the appendix reporting marginals for each nation on each question. The UNESCO time budget study by Alexander Szalai, *The Use of Time* (The Hague: Mouton, 1973), undertaken at about the same time and of similar cross-national scope, handled its reporting in the same way, and after several years of returning to that study again and again I conclude that the decisions on inclusiveness are correct. Both books remain useful for many purposes.

Respondents in the study are from Eastern Europe (Czechoslovakia, Poland, Yugoslavia), Western Europe (Federal Republic of Germany, Finland, Great Britain, Netherlands, Norway, Spain), and Asia (India, Japan). (Germany is the eleventh country, and its data came too late for inclusion in all the analyses.) The 200-question survey instrument taps beliefs and wishes about personal and national futures and perception of trends in the international system regarding peace, war, and development. Most of the responses are coded on a five-point positive-neutral-negative scale, and data are reported in terms of acceptance ratios to make the most of the information in the distribution of answers for each type of aggregate. The data are analyzed by nation, sex, age, and social strata. National culture, to the researchers' surprise, turned out to be more important than age, sex, class, or type of social system in determining responses about the future, suggesting that the nation is seen by respondents as an all-powerful actor overwhelming individual and group preferences. Very few questions dealt with the middle range of events over which people might feel they have control. Respondents from the First World displayed notable "development fatigue" with regard to the future. Western Europe sees its best times in the past, its peak in the present. Socialist and Third World nonsocialist countries see the best times ahead, the peak in the future. Although the study was done at the height of the student revolutions of Europe, no "youth international" appears in this study. Within each country youth are somewhat more optimistic about the future than older persons, but youth does not override national perceptions.

The center-periphery schema associated with Galtung's work is used in much of the analysis, leading to the not-surprising conclusion that top-dog or center nations and top-dog strata within nations want things to stay as they are and that underdog or periphery nations and groups would like a change.

There is an obvious asymmetry in the capacities of center and periphery to act on preferences. The analytic treatment of women, the working classes, mass peace movements, and developing countries as peripheries made me uneasy. The finding that absolutist thinking and the inability to make appropriate differentiations in complex situations are characteristic of peripheries (chaps. 16, 18) is somewhat tautological to the center-periphery concept itself, which assumes that all the skills are at the center

and all the needs at the periphery. This gets uncomfortably close to the kind of stereotyping that middle-class sociologists have been guilty of in relation to the culture-of-poverty thesis in the United States. The failure of the findings to differentiate between social systems in the way that Marxist theory would predict leads to an interesting debate between Galtung and Sicinski on methodological questions about surveying attitudes toward the future. How to deal with false consciousness, for example? Sicinski admits that the survey instrument did not reflect Marxist theory adequately, in part because survey methodology has not been worked on enough from the Marxist perspective.

There are many interesting and some theoretically rich propositions strewn throughout the book which unfortunately cannot be discussed in this brief review. Nevertheless there is a certain overall theoretical flatness, and the book does not throw very much light on the interaction between images and policymaking, except to confirm that the powerful prefer things as they are and will probably keep them that way. The research takes no account of Polak's historically based theory of images of the future as time bombs reacting back on present behavior or of his analysis of the optimism-pessimism dimensions of imaging (Fred Polak, *The Image of the Future,* trans. Elise Boulding [New York: Oceana Press, 1961]; abridged ed. Elise Boulding [San Francisco: Jossey Bass/Elsevier, 1972]). The vexing issues of the extent to which attitudes toward the future determine present and future behavior cannot be dealt with through analyses of social position alone, as the authors recognize.

The study concludes with the reflection that people in general do not have a very well developed capacity to think about the future and with a plea for a general extension of the time horizon under which the human condition is considered, in order that more possibilities be taken into account and alternative behaviors for center and periphery actors considered. I would add that a theory of weak actors is badly needed in any further study of the dynamics of social change through imaging the future, since many of our most powerful images of the future have come from the weak. In addition, further work along these lines must incorporate more adequate conceptualizations from Marxist thought about the future and should be designed to test more specific models of the links among imaging, structural constraints, and decision-making processes.

Three Decades in Shiwa: Economic Development and Social Change in a Japanese Farming Community. By Mitsuru Shimpo. Vancouver: University of British Columbia Press, 1976. Pp. xxvi+141. $15.00.

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The "three decades" of this book's title are those of Japan's spectacular economic growth. The impact of this growth on rural life is the subject of

this informative study focused on Shiwa, a community in northern Japan. This case study, undertaken in 1969 and updated in part to 1973, is the first in English to put flesh on some often-cited bare statistics that signal rather than explain the explosive revamping of Japan's countryside in response to a suddenly ballooning economy. Some of the changes are the rapid advance of mechanization, the shift to part-time farming, and the addition of nonfarm income that keeps rural living standards up with urban buying power, each five times above its 1950 level. The details of Shiwa help us understand how these major changes mesh with the near absence of change in other major respects: for example, the remarkable stability in numbers of the rural population until very recent years, or the minuscule family farm that still prevails in Japan, its holdings edging only slightly upward to an average 2.6 acres per farm.

Shiwa is a former village reincorporated with seven others as a "town," though it is still fully rural. It is hardly a dozen miles from Morioka (206,000 population), the major city of mountainous Iwate Prefecture, yet until after the war Shiwa was a disadvantaged, hard-scrabble locality. But land reform in 1947–49 ended longstanding bonds of tenancy, making independent owners of almost all cultivators, though on a very small scale. Then irrigation water, long a scarce resource owing to Shiwa's position in a side valley off the prefecture's major stream, suddenly flowed abundantly when the government finally completed a large dam in 1952 (chap. 1), enabling the area to more than double its rice production. Shortly thereafter, machinery adapted to small-scale rice cultivation multiplied throughout Japan, including Shiwa (chap. 2). These events set the stage for a continuing drive to diversify and upgrade farm economy, a problem that has required workable adaptation to the particular setting of Shiwa (chap. 3). Social consequences are traced in each of these three chapters and set out in overview in chapter 4, which gives most attention to intrahousehold relationships but also treats other community institutions. Finally, in chapter 5, the author looks ahead to probable patterns of development in rural Japanese communities in general, picturing several alternatives depending mainly on proximity to urban centers but also on choices made by agents of change. In a frank five-page appendix the author tells how he presented himself to the people he sought to study.

Two virtues contribute depth and contour to this study. First, Shimpo provides time-series tables that give periodic soundings (triennial, quinquenniel) from about 1950 to 1968 or later on a wide range of agricultural matters. His diachronic presentation makes clear the acceleration in trends of agricultural change. (One may quibble that the corpus of tables gives no picture across the board for any single recent year.) Second, he gives history its due by outlining the shape of village life and social structure in the water-short generations prior to 1952.

This history gives a fair view of matters abundantly familiar in Japanese sociological literature but rare in non-Japanese reportage on rural Japan. The old canal system gave preferential water rights to upstream landholders that became the basis for pervasive social hierarchy in village

affairs. Only by desperate work, day or night, could downstream people (often tenants or junior household branches) make their residual water suffice to get their rice seedlings transplanted without too much drop in yield. Shimpo also describes the temporary migration (*dekasegi*) by most men in slack season to wage jobs at sake breweries. Would that he gave as much attention to off-farm workers in the new, revolutionized economy.

The social reshaping of farm society that acclerated through the 1960s is given more detailed attention, of course. I can mention only a few items. Diversification, with its attendant pragmatic cost accounting of time and effort, plays hob with traditional hamlet solidarity, which was rooted in a uniform way of life and in social exchange at uneven cost to people at various levels on the hierarchal ladder. Moreover, government investment or subsidy and other external resources have either trivialized or displaced the functions of traditional local groups and leaders. Within households as in the community, elders formerly able to assert rightful authority are baffled by machinery and diminished by having their economic contribution shrink in proportion to off-farm income the younger generation brings in. As diversified cultivation makes individual contributions more apparent, it brings the shock of discovery that a woman's work may prove more lucrative than her husband's—not that such things equalize status, but they help.

As Shimpo explains, he dedicated himself to examining farming and farmers' lives, not to studying the entire community. We need not cavil that he bypassed the relatively few nonfarm households in Shiwa. It is a more serious lapse, however, to pay almost no attention to the proliferating number of off-farm workers living in farming households, since their proportionate earnings (not specified here after 1950) now surely outweigh agricultural earnings—findings elsewhere show up to 85% of "farm household" income derived from nonagricultural work. Until we know more about total household income, and about these relatively invisible community members, we must be skeptical about the doctrine that pervades this book, to wit, that the well-being of Shiwa now and in time to come depends on keeping farmers farming. So the Ministry of Agriculture and the agricultural cooperative that Shimpo admires firmly believe, for this is their mission. But, we must say, perhaps so, perhaps not.

Although much of the author's prediction for the future is measured and reasonable, his anticipation of growth in the size of farm units to be worked with heavy machinery (whether jointly owned or under single ownership) does not take specific account of one major obstacle: sky-high land prices. The government has conspicuously held to a hands-off policy on land prices, which have been boosted far above all other price levels by speculation anticipating nonagricultural uses. Other elements of farm policy may founder on this rock, for if entrepreneurs cannot either buy or rent land at prices that make sense for farming, except for a few uses of exceptionally high return, the hope must be that present owners can effectively join efforts for cooperative development. Shimpo, following government agricultural experts, outlines some current experiments and possibilities along

this line—yet these in general run counter to all that his book tells us about farmers' increasing independence in the past three decades, and strike me as very unlikely to be widely successful.

But none of us can look with certitude into the future in a land so rapidly changing and so prone to unexpected but workable adaptations. Certainly, the series of studies of Japanese rural society published 15 or more years ago did not fully anticipate the sort of society that has taken shape since then. This narrative, the first of a new generation, rewards quite well the reader interested in upbeat cases of constructive coping with social change.

The Making of Modern Belize: Politics, Society, and British Colonialism in Central America. By C. H. Grant. New York and London: Cambridge University Press, 1976. Pp. xvi+400. $32.50.

Donna B. Birdwell

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Grant's contribution to the Cambridge Commonwealth Series is a history of sociopolitical development in the colony of Belize (formerly British Honduras), a fragment of the former British Empire lodged between the Yucatan and Guatemala on the Caribbean seacoast. The period covered encompasses the awakening of nationalist sentiment and a series of constitutional reforms designed to lead the little country from colonial status toward some viable form of statehood.

The Making of Modern Belize is based on Dr. Grant's 1969 University of Edinburgh dissertation in which he "attempted to combine a general political history of Belize in the modern period with a structural analysis of its main institutions, such as political parties, trade unions, and local authorities" (p. xiii). The volume has been updated to include at least the major events from 1969 through 1974 and "recast . . . in terms of Belize being a cultural borderland, a meeting place for the British-oriented Creole and the Latin complexes in the Pan-American area" (p. xiv). This reorientation focuses attention on "four aspects of the problem of political integration": (1) relations between the "two major cultural complexes, the Creole and the Mestizo," (2) relations across an "elite-mass gap" and the extent to which these cross-cut ethnic divisions, (3) the problems of regional integration, and (4) the difficulties involved in finding an external orientation that will be beneficial to a secure independence (pp. 23–24).

Grant finds a convenient theoretical focus in the concept of "pluralism," as it has been developed by M. G. Smith and others for various Caribbean countries, and its supposed opposition to a more narrowly socioeconomic or class-conflict model of society. Predictably, he finds neither extreme entirely satisfactory, a conclusion which he reaches easily by about the midpoint of the book: "The entire situation was essentially too dynamic for

the society to be frozen and rigidly categorized as 'plural,' based on differentiation in the ethnic and cultural structure, or as 'proletarian,' based on differentiation in the class structure. Each framework by itself is of limited value because it necessarily excludes factors that cannot be subsumed in its particular scheme" (p. 176).

The main value of this book for social scientists, then, does not lie in any theoretical contribution, although the theoretical orientation induces Grant to present his data in a way that is eminently useful. *The Making of Modern Belize* is a history book, providing us with a detailed account of the internal battles waged in the attempt to make a viable national entity out of a tiny, underdeveloped, multiethnic colony which "has been one of the most neglected territories in the history of British colonialism" (p. 76). In company with histories by Wayne Clegern and Nigel Bolland, Grant's book completes a triad of works which cover in some depth and detail the entire span of Belizean history.

The social scientist who wishes to make use of the wealth of data to be found in the pages of *The Making of Modern Belize* must be forewarned that the book does have its shortcomings. Grant's own cultural background —he is Guyanese, and thus not unfamiliar with some of the problems of British colonialism on the Latin American mainland—makes him a generally sympathetic and perceptive analyst of Belizean affairs, but there are points at which this familiarity may have prompted him to gloss over blocks of data that would have improved the soundness of this analysis.

First, despite his admonition concerning "latter-day social scientists who talk to the political elites in the cities of the new states and then write about the social and economic conditions of the rural poor" (p. 44), Grant's own study is overwhelmingly urban in orientation. This is, in part, excusable since Belize City has been the most active political center throughout the period examined. However, this bias leaves the reader with the impression that regional integration is a matter of proselytizing indifferent rural masses into urban-originated political movements, when in fact some of the "out-districts" have produced political movements of their own. For example, the efforts of the rural population of Corozal District to become cane farmers in their own right rather than mere laborers on the large plantations influenced politics there to an extent not apparent in Grant's account.

Second, Grant's perception of a bimodal distribution of cultural loyalties in terms of the two complexes, Creole and Mestizo, is a clear oversimplification of Belizean ethnic diversity and interethnic relations. For example, the Caribs, now known as Garifuna, do not ally easily with any other ethnic group. The Kekchi Maya, although a smaller component, are also somewhat difficult to place, due to their adoption of English as a second language, while the Mopan and Yucatec Maya have maintained Spanish as their second language. Grant's failure to acknowledge that the Kekchi and Mopan are in fact Mayan groups further indicates his limited understanding of the details of Belizean ethnic divisions, as does his use of the term "Mestizo," which has little currency in Belizean parlance.

Finally, Grant has totally ignored several publications that would have aided his analysis significantly. Most conspicuously absent from his bibliography and nearly 1,500 footnotes are any references to the major works of Grant Jones, Norman Ashcraft, or Belize's own Assad Shoman. The failure to refer to such works may be taken as an indication that Grant's manuscript was in fact little revised from its dissertation form of 1969, since all were published after that date (but before 1974). The publication delay was apparently related to the difficulty of justifying the printing of such a large (and expensive) volume on such a small and little-known country. The arguments for justification form a large part of Grant's 25-page introduction. But they are perhaps best expressed—at least insofar as the general reader is concerned—by Professor W. J. M. Mackenzie of the University of Edinburgh in his brief foreword, which concludes thus: "How far can a social future be chosen and shaped, how far is a social entity merely a leaf driven by the winds and currents of the ocean of history? Belize has set out to sail that ocean in a flimsy and leaky vessel. But so have we all: you too, *hypocrite lecteur*, are in essence a Belizean, *leur semblable, leur frère*" (p. xi).

Population and Development: The Search for Selective Interventions. Edited by Ronald G. Ridker. Baltimore: Johns Hopkins University Press, 1976. Pp. xx+467. $22.50.

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While it is true that some underdeveloped countries do not as yet regard their present high level of fertility as detrimental to economic development, an increasingly large number of people from both developed and less-developed areas are concerned about its adverse effects on development efforts of the less-developed areas.

Why is it that people who can least afford adequate nurture for one or two children opt for five or six and even more? What feasible measures can countries that believe they have population problems take to reduce high fertility? Answers suggested by *Population and Development: The Search for Selective Interventions* make it worthwhile reading for both scholars and policymakers. The book tries to show, in the words of the editor, "what is known about the linkages between" fertility and its determinants. In this regard it is strongly recommended. But when the various authors start "to consider policies that might affect these determinants in a direction favorable to a decline in fertility, and to discuss research needs and possibilities to fill gaps in our understanding of these linkages and the efficacy of possible policies," a number of problems surface.

Reading carefully through the volume, one feels unsatisfied with most parts of the analyses, research proposals as well as policy recommendations of the various authors. The editor has judiciously screened the various

chapters and has brought out the specific shortcomings of each. However, the underlying problem in the analysis of problems of fertility decline has not been adequately commented upon.

In the course of the respective chapters, except those dealing with the value of children, the authors acknowledge glibly the difficulty of isolating the effects of the variables they investigate from the overall variables that bear on socioeconomic development. This problem should have been given a central place. For instance, the long-run effect of income redistribution on fertility becomes diffused since it is difficult to attribute causality to income redistribution per se; so also with respect to education. The authors discussing the effects of education on fertility emphasize their belief that, although other related variables bear on those effects, an increase in the number of years in school strengthens the direct effect of education on fertility. There are, however, situations in which persons of equally high levels of education but living in two or more contrasting social settings exhibit different reproductive behavior. This fact buttresses the view that examining a single variable as a determinant of fertility patterns is unsatisfactory, to say the least. The same conclusion is reached when one examines any of the variables in this book, namely, nutrition and health, mortality, female employment, mass media, and residential location.

This is not to say that each of these variables does not directly affect fertility. Accompanying each variable, though, is an interplay of factors whose individual effects are difficult to isolate. By way of illustration, going to school exposes a child to more information about the world around him; he probably works in an urban setting on completion of school and marries later; knows about the methods of contraception; has a higher income, lower mortality, better nutrition and health, etc. Apart from these further exposures, there are societal factors which in many equations are grouped under "error term." They are inexplicable. Therefore, analyzing the effects with respect to fertility of each variable in isolation from others can only be of mere statistical interest.

This brings us to the chapters on the value of children. Mueller, in a very long chapter, tries to argue that the value of children is negative in a peasant economy because a child produces less than he consumes. This is not the point. A child whether he produces or not must necessarily consume. If he is supposed to consume 15 units of food, produces five units, and is subsidized by his parents by 10 units of food, he is an economic asset by five units! Apart from that, whether his contribution is negative or positive, the decision to have children in a peasant economy is not based on such economic rationalization as Mueller and others tend to indicate. The decision to bear more or fewer children transcends individuals or even friends or relations; it is both societally determining and societally determined. To induce a decline in fertility, therefore, a society must be restructured in such a way that all or most of the fertility-related variables change in the direction favorable to its decline.

It appears that most of the research proposals inherently take cognizance of the time lag involved before fertility can begin to decline. However,

none of the proposals suggests the measurement of each indicator of development (or modernization) along with changes in the fertility-related variables. It is likely that at a certain point in the continuum of development those indicators have advanced to stages that can cause fertility to decline. The decline of fertility in certain countries may be demonstrably due to their having reached such a stage in the development continuum. The same approach could show that the time lag inevitable before the decline of fertility probably does not fall short of that experienced in the currently developed regions of the world.

Also, some of the proposals for educating the masses about small family size are expensive and impracticable, for example, providing television sets in both towns and villages for programs related to population education.

Because of the time element and the huge financial implications of research and policy recommendations, it is doubtful that this volume provides new knowledge with which to aid the people of the less-developed regions. Besides, the problem of fertility is of such magnitude that analysis of individual fertility-related variables and the isolation of one or more of them as tools for policy prescription may not be adequate in view of the congeries of factors that impinge on family formation.

Mortality Patterns in National Populations: With Special Reference to the Recorded Causes of Death. By Samuel H. Preston. New York: Academic Press, 1976. Pp. xi+201. $16.50.

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This work focuses upon the relationships between various causes of death and the level and structure of national mortality. In so doing, it proceeds from the analyses of mortality decline that have been done for individual countries, for example, T. McKeown and R. G. Record's "Reasons for the Decline of Mortality in England and Wales during the 19th Century" (*Population Studies* 16 [1962]: 94–122) and P. Newman's *Malaria Eradication and Population Growth: With Special Reference to Ceylon and British Guiana* (Research Series no. 10 [Ann Arbor: Bureau of Public Health Economics, School of Public Health, University of Michigan, 1965]). Preston has made a fairly precise determination of the relative contribution of each cause of death to overall mortality change. He broadens the scope of his conclusions, however, by including all countries for which usable cause-of-death information is available. This wide coverage links his work with more general analyses. Among the latter may be mentioned G. Stolnitz's "A Century of International Mortality Trends" (I, *Population Studies* 9 [1955]: 24–55; II, *Population Studies* 10 [1956]: 17–42) and "Recent Trends in Latin America, Asia, and Africa" (*Population Studies* 19 [1965]: 117–38), K. Davis's "The Amazing Decline of Mortality in Underdeveloped Areas" (*American Economic Review* 46 [1956]: 305–18),

and E. E. Arriaga and K. Davis's "The Pattern of Mortality Change in Latin America" (*Demography* 6 [1969]: 223–42). Indeed, a major highlight of the book is Preston's ability to blend the broad perspective taken in these international analyses with the precision of the case studies.

Partly because five of the seven chapters have appeared elsewhere, the chapters seem to be individual units rather than integrated parts of a conceptual whole. Some unity, however, is provided by a common data source and a set of statistical methods used throughout. The basic data set, mortality rates subclassified by 12 cause-of-death categories for 180 national populations, derives from an earlier volume by Preston, N. Keyfitz, and R. Shoen, *Causes of Death: Life Tables for National Populations* (New York: Seminar Press, 1972). Here Preston includes 165 cases (the others were eliminated because of lack of comparability) which cover 43 countries at time points ranging from 1861 to 1964. Western nations are heavily represented because data are less available for other parts of the world. The methods used to analyze the data are discussed below.

The first question that Preston addresses is which causes of death have the greatest impact on overall mortality change. He applies a components framework using regression techniques that allow him to estimate the percentage contribution of each cause to the decrease in age-standardized crude death rates. (He performs this analysis upon life table equivalents —for example, life expectancy at birth [$\mathring{e}_0$]—as well.) He argues that the statistical results represent the "normal" or "typical" pattern of mortality decline. Nonetheless, there are distinct regional and temporal patterns. For example, diarrheal disease plays an especially prominent role in non-Western populations and has gained importance over time.

Preston's general model lists "other and unknown diseases" as making the greatest relative contribution to mortality decline (34%, an average of the estimates obtained separately for males and for females), followed by influenza/pneumonia/bronchitis (26%), other infectious and parasitic diseases (15%), respiratory tuberculosis (12%), and diarrheal disease (11%). The key part played by the residual category leads to some ambiguity in the interpretation of these results. However, Preston argues that poor diagnostic and coding practices have led to the misclassification of cardiovascular disease as "other and unknown" pathologies, especially in high-mortality populations. Hence, he believes that cardiovascular disease contributes more to mortality change than is commonly thought.

Next, he examines cross-sectional relationships between national life expectancy at birth ($\mathring{e}_0$) and income per capita in the 1930s and the 1960s. He finds that the correlation coefficient between these two variables was virtually the same in the 2 decades. However, the relationship has shifted: lower-income countries can now achieve a higher national $\mathring{e}_0$ than formerly for the same level of per capita income. Income growth per se accounts for only 10%–25% of the shift; the residual is attributed to the influence of exogenous factors (e.g., the diffusion of public health technologies). These factors are as important in the industrialized nations as in Third World countries.

In addition to discussing how causes of death affect the overall level of mortality, Preston examines variations in the age and sex patterns of mortality as influenced by these causes for populations at the same mortality level. Preston shows that much of the variation in age patterns of mortality can be accounted for by different cause-of-death structures. The same variable is related to sex differences in mortality. Here perforce the author creates his own measure: the difference between actual male mortality and male mortality predicted on the basis of female mortality. When this measure is (absolutely) large, cardiovascular disease, "other and unknown diseases," neoplasms, influenza/pneumonia/bronchitis, and respiratory tuberculosis are likely to be responsible (in decreasing order of importance). Moreover, the sex differences cluster geographically and culturally in size and they increase over time.

Finally, Preston investigates some of the consequences of eliminating, one by one, certain causes of death in the United States. His procedure is to compare the population projections obtained when a specific cause is immediately reduced to zero with those in which the cause maintains its current level. In general, the elimination of cardiovascular disease has the greatest impact on a number of variables, including population size, instantaneous growth rate, percentage of population 65 years and older, number of progenitors alive, and marriage duration. Preston also determines that disease elimination has very little effect upon per capita income.

For the most part, *Mortality Patterns in National Populations* is a descriptive, empirical work. Preston should be commended for his innovative use of multivariate techniques to reduce a complex set of data to manageable summary statistics. The workhorse for most of the analysis is a regression-based decomposition technique which has received its widest application in the field of economics. Preston lists two sources, T. Espenshade's *The Cost of Children in Urban United States* (Population Monograph no. 14 [Berkeley: Institute of International Studies, University of California, Berkeley, 1973]) and J. L. Nicholson's "Variations in Working Class Family Expenditure" (*Journal of the Royal Statistical Society* 112 [1949]: 359–411). To those might be added a number of others.

Since this decomposition technique is not a totally familiar demographic tool, a more detailed description of it would have been welcome. On the one hand, the method is analogous to the decomposition procedures presented by E. M. Kitagawa in "Components of a Difference between Two Rates" (*Journal of the American Statistical Association* 50 [1955]: 1168–94): It determines the contribution of specific components to a change in the sum of these components (i.e., the overall rate) in the population(s) of interest. In this context, the technique performs a descriptive function, and the results are not immediately generalizable to other populations. However, properly formulated, the regression approach to decomposition can be used to construct a model of typical mortality change. Such a model would enable inferences to be made to populations outside the sample from which the results were derived. The regression decomposition method, then, can be used for descriptive and/or inferential purposes. Of the two, Pres-

ton's intent seems to be more the former than the latter. The reader sho keep this in mind: Despite the characterization of the results as rej senting a "typical" pattern of mortality decline (e.g., p. 9), whether estimated coefficients would be similar for a different sample of popı tions is an important question that is not explicitly considered.

In sum, Preston has addressed a number of important ideas regard the worldwide mortality decline in the 20th century. His work prese many interesting problems for future research. However, his contribut to mortality research would have been even greater if he had taken a m theoretical approach, provided a more detailed introduction to his te niques, and organized his arguments in a clearer and more coherent fashi

Here to Stay: American Families in the Twentieth Century. By Mary Bane. New York: Basic Books, 1976. Pp. xvi+195. $11.50.

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Acting on her assumption that the family was indeed a declining in tution, Bane undertook to discover data to verify that assumption. Her vestigation led her to conclude otherwise. The data on which she ba her conclusion that "the family is here to stay" are demographic. The f part of her book is a systematic presentation of her findings; the seco is a discussion of the policy implications of her general conclusion that family is here to stay. There is no attempt to be conceptual, or to deve any sort of theoretical framework to explain her findings, or to relate th significantly to other work in this area. Nor is there any effort to use mu variate techniques in her data analyses. Nonetheless, her array of findi and the policy discussions do have some merit.

The following represent Bane's major substantive conclusions: Childre living arrangements have not changed much over several generations. spite of divorce trends, people continue to marry and remarry. Elde persons are less likely to be living with kin than was true several deca ago, and divorced adults are less likely to be living with their kin. Fami are not, and need not be, systematically isolated from the communi in which they reside.

Bane then raises certain policy issues and presents alternative choi which might be made in response to them. One such issue is how to fos what she calls "sexual" or sex-role equality. A good deal that appears this chapter has been said before by sociologists and nonsociologists ali Had Bane drawn on that literature, she might have been able to advar the discourse beyond where it now stands. More original are her sugg tions as to how to protect the rights of children—how to balance paren power and control over children with state power so as to act in the b interests of offspring. A separate chapter is devoted to ways to prom the economic well-being of children and those who care for them. Ba

presents an excellent critique of current AFDC policies and raises other kinds of related social welfare questions. Some of her observations could be helpful to lobbyists and others interested in influencing President Carter's 1977 welfare reform proposals.

In sum, sociologists who focus purely or largely on social policy questions will find this book useful. Those concerned about a meshing of theory, substance, and policy will—unfortunately, I'm afraid—be disappointed.

Resistance through Rituals: Youth Subcultures in Post-War Britain. Edited by Stuart Hall and Tony Jefferson. New York: Holmes & Meier, 1976. Pp. 287. $10.00.

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Resistance through Rituals is a provocative and frustrating amalgam of moral polemic, sociological analysis, and abstruse theorizing about working-class youth in England. Stylistically, the book is odd indeed. A ponderous and, at times, obscure Marxist vocabulary is combined with offhand references to the argot of the underlife of British pop culture. To make matters more difficult, the typography looks as if hastily typed seminar papers were sent off to a publisher who simply photocopied the original manuscript.

Associated with the Centre for Contemporary Studies, the group of sociologists represented here believes that the "exotic," "bizarre," or "deviant" forms of youth culture in England today do not simply represent a distinctively youthful or generational response to mass media's attempt to shape their leisure activities and consumption patterns. Rather, in the opinion of these radical scholars, youth culture arises out of and cannot be understood apart from the way in which young people translate the problematics of their class position into symbolic forms which both articulate the common elements of their social experience and subvert the cultural authority of the dominant social classes. In other words, youth culture is an ideological means of resistance to the standards and values of the dominant culture.

These scholars accept the notion that youth cultures form coherent social worlds which draw upon the taken-for-granted meanings of family and neighborhood relationships in British working-class life. Nevertheless, they insist that the seeming autonomy of these cultural phenomena from the struggle for power and privilege between superordinate and subordinate social classes is illusory. They claim that we can understand the significance of working-class youth culture when we see how the institutions which presumably link youth to the larger society reproduce the class structure of the society as a whole. The school and the labor market are obviously the institutions that connect working-class families and communities to the larger society. Unfortunately, we learn almost nothing in the

essays on various forms of youth culture about the meaning of school, work, and family for the members of these groups. Nor do the authors show us how working-class youths' experience in these institutions shapes their response to the dominant culture.

This brings us to a recurrent theme in the book. Many of the writers seem to have a love-hate attitude toward Howard Becker and other representatives of the naturalistic Chicago school of sociology. They criticize Becker for his vague theoretical model of the relationship between deviant subcultures and the distribution of power in capitalistic societies (i.e., Becker's notion of moral entrepreneurs). And they suggest that their work represents a definitive advance over this approach. However, in my opinion, the descriptive analyses of the Teds, Mods, Skinheads, Drug Users, Communes, and other groups do not come close to the analytic rigor and conceptual elegance of naturalistic sociologists such as Becker and Matza. Even more important, the writers simply assert that the attitudes, styles, and activities of these groups were, in fact, symbolic modes of opposition to the dominant culture. Nowhere do they demonstrate, either empirically or theoretically, that youthful behavior which conventional representatives of society call "criminal," "destructive," "vicious," or "absurd" is actually an expressive mode of rebellion, a protest against an inferior status in the class structure. It seems to be stretching the point, for example, to explain the Skinhead attacks on racial minorities as a defense of traditional working-class communities against the forces that threaten these solidarities.

These scholars argue that we must return to a classic Marxist model. It is the material interests around which social groups cohere that constitute the driving force behind these cultural formations and that provide the grounds against which their political efficacy should be judged. For working-class youth, the dialectics of class revolve around the issue of becoming absorbed by the dominant culture's definition of right and wrong ways of behaving and by the dominant culture's ideas about the proper distribution of economic opportunities and resources. In the final analysis, these scholars claim that youth subcultures represent a "magical" solution to class problems because expressive or symbolic opposition to the dominant culture's moral authority does not lead to effective forms of political action. Ideologically, youth do not see that the institutions such as the school which are inserted by the dominant society into working-class life create the illusion that their problems are due to their age, not to their class affiliation.

In quite abbreviated outline, this strikes me as the core argument of a highly discursive and loosely organized book. I find this book rather unsatisfactory for two reasons. As I mentioned before, there is almost no systematic integration between theoretical categories and empirical analysis, and theoretical arguments emerge and disappear in the text according to the whim of a particular writer. Second, the book is singularly free of the kind of rich ethnographic description that one ordinarily associates

with studies of subcultures. Only rarely are youth allowed to speak for themselves, and there are only vague, journalistic accounts of the kinds of activities, relationships, and norms that give a subculture its special meanings for its members. Nonetheless, this book does give the reader a glimpse into the social worlds of British working-class youth and, for that reason, will be of some value to anyone interested in youth culture.